The American Political Science Review

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Articles and Essays by Peter Eisinger, Ada W. Finifter, John C. Pierce and Douglas D. Rose, Philip E. Converse, Arthur G. Stevens, Jr., Arthur H. Miller, and Thomas E. Mann, David J. Elkins, Donald R. Kelley, John Chamberlin, William I. Bacchus

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Ronald P. Formisano, Roger E. Wyman

The Growth of U.S. Political Culture

Kenneth A. Shepsle, Robert Lyle Butterworth,
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Peter C. Fishburn

Formal Theories of Voting and Coalitions

Glenn Tinder

Transcending Tragedy: The Idea of Civility

Timothy A. Tilton

The Social Origins of Liberal Democracy:
The Swedish Case

Stanley Kelley, Jr. and Thad W. Mirer

The Simple Act of Voting

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CONGRESSMEN'S VOTING DECISIONS
John W. Kingdon

This analysis of the ways congressmen arrive at their voting decisions examines the influence of various political actors, as well as congressmen's decision rules, their sources of information, and their place in the larger political system. 313 pp.; Cloth: $9.95; Paper: $4.95; 1973.

VIOLENCE AS POLITICS: A Series of Original Essays
Herbert Hirsch and David C. Perry

Original essays explore the centrality of violence in political and social behavior, answering questions about the rationality, normality, prevalence, and significance of violence. 262 pp; Paper: $4.95; 1973.

STRUCTURE AND PROCESS IN INTERNATIONAL POLITICS
Raymond F. Hopkins and Richard W. Mansbach

Based on the latest empirical research, this book on international relations blends historical and case study materials within a well-defined, theoretical framework. 498 pp.; $10.95; 1973.

CHALLENGE AND DECISION: Political Issues of Our Time, Fourth Edition
Reo M. Christenson

Lucid, concise, coherent, and well rounded, the Fourth Edition of this text focuses on the most crucial and controversial aspects of current political issues. 227 pp.; Paper: $3.95; 1973.

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Mark A. Tessler, William M. O'Barr, and David H. Spain

This book examines the confrontation between old and new value systems among three African peoples experiencing rapid social change. 363 pp.; Paper: $4.95; 1973.

Now Available in Paperback

PARTICIPATION IN AMERICA: Political Democracy and Social Equality
Sidney Verba and Norman H. Nie

Winner of the 1973 Gladys M. Kammerer Award in the field of U.S. National Policy.Tentative: 428 pp.; Paper: $5.95; May, 1974.

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ARTICLES

473 **Deferential-Participant Politics: The Early Republic's Political Culture, 1789–1840.** The concepts of "party" and "party system" may be obscuring the nature of early national political culture. The presence of a modern party ethos before the 1830s seems to be taken for granted, as are assumptions regarding the alleged benefits of party. Historians have not yet demonstrated, however, the many dimensions of institutionalized party behavior. Focus is recommended on three observable elements of party (after Sorauf): as organization, in office, in the electorate. Studies of party self-consciousness developing over the entire 1789–1840 period are necessary in various political units. Evidence is inconclusive, but weighs on balance against a first party system of Federalists and Republicans (1790s–1820s). While relatively stable elite coalitions and even mass cleavage patterns perhaps developed at staggered intervals in different arenas, especially during the war crisis period of 1809–1816, the norms of party did not take root and pervade the polity. The era to the 1820s was transitional, a deferential-participant phase of mixed political culture roughly comparable to England's after 1832. Theories relating party to democratization, national integration, and political development, should be reconsidered.

By Ronald P. Formisano, Associate Professor of History, Clark University.

488 **Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture.** The middle-class character of the leadership of American reform movements has been well established. While leaders of the progressive movement in early twentieth-century America also conform to this pattern, the nature of the voting base of support for progressivism has not yet been established. The purpose of this paper is twofold: first, to examine whether or not middle-class voters supported progressive candidates at the polls; and secondarily, to test the relative strength of cultural factors (i.e., ethnicity and religion) versus class considerations as determinants of voting behavior in the early twentieth century. The results demonstrate that, at least in the key progressive state of Wisconsin, middle-class voters failed to support progressive candidates in either general or primary elections; to the contrary, they provided the bulwark of support for conservative opponents of reform. Using bivariate and partial correlational analysis, the paper also shows that ethnocultural factors remained as the most powerful determinant of voter choice among urban voters in general elections, but that class considerations often proved more influential in motivating voters in primary election contests.

By Roger E. Wyman, Assistant Professor of History, Rutgers University, Newark.

505 **On the Size of Winning Coalitions.** A recent note by Robert Butterworth is critical of William Riker's size principle on several important grounds. There is, however, an important omission in his analysis which this present essay aims to correct. The author goes on to tie assertions about coalition structure in n-person zero-sum games to a solution theory for such games. In the appendix to this essay the general five-person game, of which Butterworth's game is a special case, is considered in some detail. The effect, with one reasonable solution theory, is a favorable appraisal of the size principle.

By Kenneth A. Shepsle, Associate Professor of Political Science, Washington University, St. Louis.

Comment. By Robert Lyle Butterworth, Assistant Professor of Political Science, University of Pittsburgh.

Rejoinder. By Kenneth A. Shepsle.

525 **The Paradox of Not Voting: A Decision Theoretic Analysis.** Various analysts have noted that the decision to vote in mass elections is difficult to justify from the standpoint of an expected utility maximization model. Put simply, the probability that a citizen's vote will affect the outcome is so small that the expected gains from voting are outweighed by the costs in time and effort. Such analyses treat rational behavior as synonymous with expected utility maximization. In this paper we show that an alternative criterion for decision making under uncertainty, minimax regret, specifies voting under quite general conditions. Both two and three candidate plurality elections are considered. Interestingly, a minimax regret decision maker never votes for his second choice in a three candidate election, whereas expected utility maximizers clearly may. Thus, the model proposed has implications for candidate choice as well as turnout.

By John A. Ferejohn, Assistant Professor of Political Science and Morris P. Fiorina, Assistant Professor of Political Science, California Institute of Technology.

537 **Paradoxes of Voting.** Five voting paradoxes are examined under procedures which determine social choice from voters' preference rankings. The most extreme forms of each paradox are identified, and their potential practical significance is assessed using randomly generated voter preference profiles.

The first paradox arises when the winner under sequential-elimination simple-majority voting is less preferred by every voter than some other alternative. The fifth paradox occurs when one alternative has a simple majority over every other alternative and one or more of the simple-majority losers beats the winner on the basis of every point-total method that assigns more points to a first-place vote than to a second-place vote, more points to a second-place vote than to a third-place vote, and so forth.

The other three paradoxes are solely concerned with point-total procedures. They include cases in which the standard point-total winner becomes a loser when original losers are removed, and in which different truncated point-total procedures (which count only first-place votes, or only first-place and second-place votes, and so forth) yield different winners.

The computer simulation data suggest that the more extreme forms of the paradoxes are exceedingly unlikely to arise in practice.

By Peter C. Fishburn, Research Professor of Management Science, The Pennsylvania State University.

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17 **Transcending Tragedy: The Idea of Civility.** This article belongs in the area of what Karl Jaspers calls "existential elucidation." It is concerned less with political ideals than with the relationship of the person to those ideals and to the realities that often contradict them.

During recent centuries political activity has been increasingly governed by the confidence that history is under human control. The tragedies and disappointments of the twentieth century, however, cast serious doubt on this confidence. Thus it is incumbent on us to reconsider man's whole stance in relation to history. The core of the article is the definition of an alternative stance, which I call "civility."

The clue to civility was provided by Plato when he suggested, in *The Republic,* that although the ideal city probably could not be realized in history, its form might be reproduced here and there in the souls of individuals. In pursuance of this clue, civility is defined, on the one hand, as partial detachment from action, and from the ideological preoccupations frequently accompanying action, and, on the other hand, as concentration on governance of the self. Although such governance entails historical independence, it does not set one apart from others; on the contrary, its fundamental principle is openess to the totality of the human.

By Glenn Tinder, Professor, Department of Politics, University of Massachusetts, Boston.

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1 **The Social Origins of Liberal Democracy: The Swedish Case.** Implicit in Dahrendorf's *Society and Democracy in Germany* and explicit in Moore's *Social Origins of Dictatorship and Democracy* are respectively a liberal and a radical model of democratic development. Neither of these models adequately accounts for the experience of Sweden, a remarkably successful "late developer." Although Swedish industrialization proceeded with little public ownership of the means of production, with limited welfare programs until the 1930s, and above all with restricted military expenditure—all factors Dahrendorf implies are crucial for democratic development—it did not produce the traditional liberal infrastructure of bourgeois entrepreneurs nor a vigorous open market society. Similarly only three of Moore's five preconditions for democracy obtained in Sweden: a balance between monarchy and aristocracy, the weakening of the landed aristocracy, and the prevention of an aristocratic-bourgeois coalition against the workers and peasants. There was no thorough shift toward commercial agriculture and, most important, there was no revolutionary break with the past. Consequently, one has to evolve a radical liberal model of development which states the conditions for the emergence of democracy in Sweden without revolution. This model contains implications for the further modernization of American politics.

By Timothy A. Tilton, Assistant Professor of Political Science, Indiana University.

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2 **The Simple Act of Voting.** The research reported in this article involved tests of a model by which voting decisions can be explained and predicted. Data for the tests came from surveys conducted in five presidential elections by the Survey Research Center of the University of Michigan. Predictions made in terms of the model show it to be a good basis both for predicting the division of the vote and for predicting the votes of individual voters. Extensive analyses of incorrect predictions suggest them to be in great part the sort of errors one would expect, were voters arriving at their voting decisions in the manner described by the model. The validity of the model has implications of importance for practical politics, political history, and political theory.

By Stanley Kelley, Jr., Professor of Politics, Princeton University and Thad W. Mirer, Assistant Professor of Economics, S.U.N.Y. at Albany.

Racial Differences in Protest Participation. Understanding of the phenomenon of political protest has been inhibited by the view that protest is fundamentally extraordinary or unconventional in character and that those who use it do so because they lack the resources to employ more conventional means of political expression. This article challenges this unqualified view by examining survey data based on black and white samples from the city of Milwaukee which relate to racial attitudes toward protest, the social characteristics of protest participants, and to the uses and organization of protest in the two racial communities. The analysis reveals widespread support for protest in the black community in contrast to the general antipathy found among whites. Both black and white protesters are found to be socioeconomically better-off than nonprotesters in their respective racial communities, but a variety of indicators suggest that black protesters are more integrated and typical members of their community than white protesters are of theirs. Data on the uses and organization of protest show that it has become an institutionalized feature of the black pursuit of urban politics in Milwaukee in contrast to its generally ad hoc and less frequent role for whites.

We may conclude from all this that protest represents a widely accepted, integral part of black politics in the city, while for whites protest is indeed unconventional, a violation of dominant social norms. This conclusion is used as a basis for speculating on the relationship of protest participation to the possession of social resources and on the capacity of social resources to offset the costs incurred in the form of social disapproval for violating white norms against protest behavior.

By Peter K. Eisinger, Assistant Professor of Political Science, University of Wisconsin, Madison.

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607 **The Friendship Group as a Protective Environment for Political Deviants.** This is a study of the relationship between partisan preference and social integration in natural work groups in several automobile plants. The basic finding is that the number of work group friends increases from the Democratic to the Republican end of a standard party identification scale. Since the political context of the study sample is heavily Democratic, attention is focused on the fact that Republicans in this environment are political deviants. It is suggested that friendship integration is a function of perceived deviance in that deviating from group norms leads to social insecurity, cognitive dissonance, and a need for opinion evaluation, all of which motivate affiliative behavior. Several hypotheses are deduced from this proposition. The first is that Republicans have less political contact in nonwork contexts, but more in the work group, than Democrats do. Second, the relationship between partisanship and friendship integration should be greater for members of social groups in which the pro-Democratic norm is stronger than for those in which this norm is weaker. Third, strength of identification with the norm-bearing group ought to be positively related to friendship integration among deviants, since identification would make the group's norms more salient and increase the deviant's discomfort. Fourth, political deviants should tend to choose each other as friends to a greater extent than political conformers do. Finally, since friendship alliances apparently serve a protective function for political deviants, it is hypothesized that among deviants (but not among conformers), friendship integration should be related to political participation. All the hypotheses are supported. The results are interpreted in terms of the critical function of social support for political deviants in pluralist societies. Since pressures for conformity are strong, it is important to understand the ways in which minorities deal with them. Friendships in work groups, ostensibly nonpolitical, therefore have important political functions.

By Ada W. Finifter, Associate Professor of Political Science, Michigan State University.

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626 **Nonattitudes and American Public Opinion: The Examination of a Thesis.** This paper utilizes the 1956–58–60 SRC panel study to examine the degree to which Americans hold attitudes on issues of public policy. The conclusions reject the thesis that only 20 to 30 per cent of the American public have true attitudes and that the remainder either refuse to take a position or respond randomly. The nonattitude thesis is rejected on the basis of: (1) a conceptualization of attitudes which allows for variation in responses through time without necessarily indicating the absence of attitudes or their random fluctuation; (2) an evaluation of the major statement of the nonattitude thesis; (3) a probability model for measuring attitudes in a panel study based on the assumption of twin samples, i.e., a sample of the population at one point in time, and a sample of the individual's attitude through time; and (4) the application of the probability model, leading to the conclusion that the number of individuals with attitudes has been severely underestimated. The implications of that finding are drawn for the relation of responses to attitudes and for democratic elitism.

By John C. Pierce, Associate Professor of Political Science, Washington State University and Douglas D. Rose, Assistant Professor of Political Science, Tulane University.

Comment. By Philip E. Converse, Professor of Political Science, University of Michigan.

Rejoinder. By John C. Pierce and Douglas D. Rose.

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667 **Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group.** In 1959 a number of progressive Democratic congressmen organized the Democratic Study Group (DSG) as a vehicle for countering certain conservative biases then present in the decision-making process in the House of Representatives. This paper presents brief descriptions of the difficulties faced by these congressmen in their efforts to pass more "liberal" legislation and of the organization and

activities of the DSG. The analytical focus is on an assessment of DSG success in developing an effective communication network as a means of achieving its policy goals. The central hypothesis is that this communication network has had an impact on the voting behavior of DSG members. Roll-call data from the 84th through the 91st Congress are examined to ascertain whether longitudinal patterns in the voting of DSG members, non-southern non-DSG Democrats, southern Democrats, and Republicans tend to confirm or deny this hypothesis.

By Arthur G. Stevens, Jr., Assistant Professor of Government, University of Virginia, Arthur H. Miller, Study Director, Center for Political Studies, University of Michigan and Thomas E. Mann, Staff Associate, American Political Science Association.

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682 **The Measurement of Party Competition.** Most measures of party competition were designed for the United States or other two-party systems. The measure proposed here is intended for multiparty as well as two-party systems. It is a formalization of the notion of uncertainty: election outcomes are competitive to the degree we are uncertain who will win. By ascertaining through survey research the degree of party loyalty over time, one can calculate the chances of alternative outcomes in hypothetical replications of any given election. Since the chance that a losing party might have won is one measure of the importance of that party, this approach to party competition also provides a precise indicator of the number of parties in the system (i.e., whether it is two-party, three party, etc.). The method is applied here to data on Canadian federal elections in 1965 and 1968.

By David J. Elkins, Assistant Professor of Political Science, University of British Columbia.

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701 **Toward a Model of Soviet Decision Making: A Research Note.** This research note offers a partial model of decision making in the Soviet Union cast in terms of the level of conflict intensity within the political system, the identity of the major participants, and the corresponding mode of decision-making behavior. It also deals with the rationalization of decision making in the post-Stalin era and the role of interest groups in policy formation.

Recognizing the multifunctional nature of decisions made within politicized bureaucratic structures, the model outlines three levels of conflict intensity and decision-making behavior:

(1) Analytic conflict occurs over maximizing (technical) decisions and elicits a decision-making style described as research and persuasion. The most influential actors are specialists and technicians.

(2) Organizational conflict occurs over mixed maximizing and integrative decisions and calls forth a combination of analytic and bargaining techniques. Key actors are institutionalized interest groups.

(3) Systemic political conflict is associated with integrative (political) decisions reached either through informal high-level bargaining or voting within higher party bodies. The most important actors are leadership factions and interest groups with political resources.

By Donald R. Kelley, Assistant Professor of Government, Monmouth College, New Jersey.

'07 **Provision of Collective Goods as a Function of Group Size.** In *The Logic of Collective Action,* Mancur Olson shows how the activities of various political organizations can be fruitfully analyzed using the theory of collective goods. Several of Olson's major conclusions concern the relationship between the size of a group and its ability to provide its members with collective benefits. He concludes that as group size increases, the amount of collective benefits provided will become increasingly suboptimal, and that the absolute amount of collective benefits provided will decrease. This paper shows that Olson's conclusion concerning the relationship between group size and the absolute amount of collective benefits provided is not generally true. Within the framework of analysis used by Olson, it is demonstrated that the relationship is determined by the interaction between two effects, an "income" effect which may cause the level of benefits provided to increase as group size increases, and a "congestion" effect which may cause the level of benefits to decrease as group size increases. The final result is that for "inclusive" collective goods the relationship is an increasing one, while for "exclusive" goods it is a decreasing one. Some implications of this result for the use of the theory of collective goods in studying political processes are discussed.

By John Chamberlin, Lecturer in Political Science and Assistant Research Scientist, Institute of Public Policy Studies, University of Michigan.

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17 **COMMUNICATIONS**
From Robert Axelrod, Fred I. Greenstein, James W. Clarke and Henry C. Kenski, Donald D. Searing and Joel J. Schwartz, Nigel Howard, John C. Harsanyi, Steven B. Wolinetz, Morton A. Kaplan

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33 **EDITORIAL COMMENT**

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736 BOOK REVIEWS AND ESSAYS

Diplomacy for the 70's: An Afterview and Appraisal. Numerous attempts have been made since World War II to reform U.S. government structure and procedures for managing foreign affairs, but success has been distinctly limited. In evaluating the procedures, proposals, accomplishments, and failures of the most recent State Department reform program, this review suggests some reasons why rationalization of foreign affairs organization has been so difficult to achieve. Unless fundamental questions of the Department of State's appropriate relationship to the rest of government are confronted directly, it is impossible to deal effectively with internal organization, operations, and staffing. Yet because of a restricted mandate, *Diplomacy for the 70's* did not address itself to these prior questions. And for a number of reasons, including a misplaced belief in the efficacy of management reforms as contrasted with political initiatives, lack of aggressive senior level support, ineffective followup, and budgetary restrictions, it fared no better than previous reform programs, even taken on its own limited terms. Unless such basic problems are dealt with, future reform attempts are likely to be no more successful.

By William I. Bacchus, Staff Member, Commission on the Organization of the Government for the Conduct of Foreign Policy.

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Deferential-Participant Politics:
The Early Republic's Political Culture, 1789–1840*

RONALD P. FORMISANO
Clark University

Introduction

Political parties in the United States took their basic form within a period extending from the 1790s to the 1840s. Though historians perhaps have compartmentalized this process excessively, theoretical concerns are cutting through the time boundaries of conventional periodizations. Amidst a still great variety of studies in this area, three general trends may be discerned. (1) A few bold spirits venture to work with theories of modernization, national integration, and political development. Their outlook is at least implicitly cross-national and comparative. (2) Others, writing less ambitious histories, use the middle-range concepts of the first group, and occasionally generate their own. (3) The great majority of historians continue to write monographs conceived in traditional modes, tilling circumscribed plots of time, ignoring theory and comparison. Obviously, the contributions of the latter two will attain more general interest as they lose preoccupation with the unique and make explicit their theoretical assumptions. The theorists, in turn, surely need continuous interaction with empirical studies at the grass-roots. Ironically, the main point of convergence for these disparate efforts, the concept of party, may be a source of shared illusion. In general, catch-phrases and catch-words such as "party system," "democratization" or "the decline of deference," propositions which began as unresearched hypotheses, have almost imperceptibly become imbedded in the conventional wisdom. Generalizations about party formation, especially, seem to be outrunning their empirical bases, and "party system" may have mushroomed into an anachronism distorting the character of early national political life.

The basic idea of *party*, often loaded with cultural connotations, serves to organize political phenomena which may not fit a party mold. The era from Washington to "Tippecanoe and Tyler Too" should be thought of primarily as a transitional phase of political culture in which the passage from a traditional, notable-oriented and deferential politics on the one hand to a party, electorate-oriented and egalitarian style of politics on the other, did not come about abruptly. The alleged first party system of Federalists and Republicans (1790s–1820s) reached backward into eighteenth century political culture as much as it forecast the shape of future politics. Partisan behavior was certainly present, and some men even advocated "party" as a positive good. But such precocious partisans were rare, and partisan activity is not necessarily the same as institutionalized party behavior—however haphazard that may be when fully operational in the American context. As a transition in political culture, then, the early republican era is best viewed as a deferential-participant phase somewhere between traditional forms and mass party politics, having some features of both.

This is not merely a question of timing, though even as such it has important ramifications, such as its bearing on the controversy over the causal relationship between party and democracy. Although few investigations have been designed to assess directly claims made in praise or blame of parties' role in the process of "democratization," many recent writers confidently celebrate the advent of Political Party in history. They believe that a competitive, two-party system opened up political life, brought distant governments closer to more citizens, and made power more responsive to constituent demands. In the celebratory tradition, party virtually equals democracy. Parties receive praise also as guarantors of stability. Some theorists believe that societies such as the United States, England, India, and Japan have enjoyed stable political systems because strong parties channeled newly mobilized electorates.[1] Thus, the wonders worked by parties increase, and they rise once again to almost providential proportions—even as our present parties show striking signs of decay.[2]

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* The author wishes to thank David H. Fischer, William G. Shade, Walter Dean Burnham, and Joel Silbey for their generous criticism, and the National Endowment for the Humanities for financial support.

[1] J. Rogers Hollingsworth, "An Approach to the Study of Comparative Historical Politics," in *Nation and State Building in America: Comparative Historical Perspectives* ed. J. R. Hollingsworth (Boston: Little, Brown, 1971), p. 266.

[2] Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970), pp. 91–134.

Although celebrants of party have written valuable studies, even the wisest seem mesmerized by several assumptions which rest on faith rather than works, on values rather than data. The most sweeping assumption is the Idea of Political Progress: that the political system of the United States "matured" from 1789 to 1840 toward a more democratic, efficient, equitable, moral and just form, with competitive parties as the key vehicle of change. The Idea of Political Progress through Party depends in turn on a premise which Frank Sorauf has called "the myth of party primacy." Sorauf explained this as a tendency to consider American parties "as independent agencies . . . —as some *élan vital,* some autonomous, ordering force set loose in the system. It has been customary," Sorauf added, "to speak of the parties as 'democratizing' American politics as if they were reforming missionaries from some far-off places."[3]

Critics of party, from Moisei Ostrogorski to Edward Pessen, have challenged the celebrants' equations. They observe that politics remained as much under elite control after, as before party competition in an extended electorate. They describe "democratization" as largely a matter of rhetoric and style. Beneath the hoopla of "The Age of the Common Man" they discern the iron law of oligarchy at work. But the skeptics tend to carry their critique too far, and so err in another direction. Like the celebrants, the debunkers of party are reacting against a liberal-conservative ideological schema imposed earlier on parties by Progressive historians: the result is an exaggerated view of parties as electoral machines unrelated to conflicts over values and social belief-systems. Thus the critics tend to empty party politics of its social, cultural and ideological content.[4]

Critics, celebrants, and those in between often join in using a metaphor about American parties: the party as machine; a centralized, disciplined, and rational electoral organization. Admittedly oligarchic, it is an institution allegedly characterized by wide participation in internal decision making. The machine metaphor, partly as a simple literary convenience, has exaggerated the cohesion of United States parties throughout their existence, but particularly in the early republic. Scholars have overestimated the extent to which *institutionalized* parties structured politics before the 1830s. To be institutionalized an organization must: (1) be well bounded or differentiated from its environment; (2) have functions that are complex and separated internally on some regular and explicit bases; and (3) use universal rather than particular criteria in conducting its internal business.[5] Above all, it has a life—and goals—of its own; its pre-eminent goal is survival. If criteria of institutionalization are applied with any rigor, then fully developed parties cannot be said to appear in the United States until the late 1830s or early 1840s, that is, parties with structures characteristic of our present relatively decentralized and undisciplined parties.[6]

Too often historians have focused on certain sectors of apparent partisan activity—Congressional debates in the 1790s, patronage under Jefferson, or cadre men organizing in the 1820s—and assumed that a party "system" existed throughout the polity. An explicit rather than a working definition of party can help avoid this simple error. Sorauf's definition of party as a "tripartite organization" can focus the historian's attention on what is "empirically observable." Sorauf thinks of party as composed of three elements: (1) "the organization proper"—party officials, activists, members, "the purposeful, organized, initiating vanguard of the party"; (2) "the party in office" —including legislative parties, caucuses, floor leaders, whips, and patronage networks; (3)

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[3] Frank J. Sorauf, "Political Parties and Political Analysis," in *The American Party Systems: Stages of Political Development,* ed. William Nisbet Chambers and Walter Dean Burnham (New York: Oxford Univ. Press, 1967), p. 49.

[4] This point is made by: Herbert Ershkowitz and William G. Shade, "Consensus or Conflict? Political Behavior in the State Legislatures During the Jacksonian Era," *Journal of American History,* 58 (December, 1971), 591–621.

[5] Nelson W. Polsby, "The Institutionalization of the U.S. House of Representatives," *American Political Science Review,* 62 (March, 1968), 145. Polsby's full definition of an institutionalized organization: "(1) it is relatively well-bounded, that is . . . differentiated from its environment. Its members are easily identifiable, it is relatively difficult to become a member, and its leaders are recruited principally from within the organization. (2) The organization is relatively complex, that is, its functions are internally separated on some regular and explicit basis, its parts are not wholly interchangeable, and for at least some important purposes, its parts are interdependent. There is a division of labor in which roles are specified. . . . (3) Finally, the organization tends to universalistic rather than particularistic criteria, and automatic rather than discretionary methods for conducting its internal business. Precedents and rules are followed; merit systems replace favoritism and nepotism; and impersonal codes supplant personal preferences as prescriptions for behavior."

[6] The decentralizing tendencies of American parties are discussed in Morton Grodzins, "American Political Parties and the American System," *Western Political Quarterly,* 13 (December, 1960), 974–98.

"the party-in-the-electorate—the least stable, least active, least involved, and least well-organized of the three sectors," the individual voters who identify with the party and for whom it is a "symbol that provides cues and orders to the political cosmos."[7] These three elements should be thought of as variables present over time in different combinations and intensity, on local, state, and national levels. Party exists when activists, officials, and voters interact and when they consciously identify with a common name and symbols. Sorauf's three analytic categories are of course transcended in reality by an entity which aspires to overarch federalism and the separation of powers. Indeed, the party's inherent ambition to unify various levels of government violated the Whig heritage of the Revolution and some of the most sacred values of eighteenth-century political culture.[8]

With such a definition, party origins in the United States can be approached best in an expanded time framework. All elements of party in specific localities might be studied, or one or more elements in states or regions might be followed from the 1790s through the 1830s. Those studies carrying through the period might, as a bonus, rescue the 1820s from their relative conceptual limbo.

If guidelines such as these are pursued, the "first party system" may well become a casualty of subsequent research. As will become clear below, there is some reason to be skeptical of the concept. Although I personally do not now believe in a first party system of Federalists and Republicans as conventionally portrayed, the evidence at hand is inconclusive. When read critically, with the criteria outlined above in mind, the first system literature does not support its own conceptual burden. Nor does the residue of evidence permit us to discard the notion that some partisan activity developed in places at various times from 1800 to 1820, and that some states produced tentative party action in one or more of Sorauf's three categories of party. An institutionalized national party system, however, was most remote. I doubt there will be much justification for calling the first sporadic gropings toward national organization in 1800 and afterward the first modern parties, or for describing Federalist-Republican rivalries as a system of patterned roles, functions, and expectations. Yet this essay will achieve one of its goals if it induces even a temporary suspension of belief in the first party system. Beyond that, it might stimulate refinement of concepts describing stages of party growth.[9]

Critique of the First Party System Literature

Although the concept of "party system" by itself reflects the impact of social science on political history (particularly Parsonian sociology),[10] the historical construct objected to here is not of recent origin. For a long time historians have believed that Hamiltonians and Jeffersonians organized parties soon after the establishment of a national arena. In older monographs, researchers seldom provided explicit definitions of party.

Absence of definitions, however, is not a fatal criticism of modern general works which support the historiographical edifice of the first party system: the well-known books of Joseph Charles, Manning Dauer, and Noble Cunningham.[11] Moreover, William Chambers's synthesis, *Political Parties in a New Nation* (1963), not only defined party politics as distinct from colonial and revolutionary political formations, but also introduced a high degree of conceptual refinement to the historical discussion.

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[7] Sorauf, "Political Parties and Political Analysis," pp. 37–38. Chambers recently defined party in the modern sense "as a relatively durable social formation which seeks offices or power in government, exhibits a structure of organization which links leaders at the center of government to a significant popular following in the political arena and its local enclaves, and generates in-group perspectives or at least symbols of identification or loyalty." "Party Development and the American Mainstream," in Chambers and Burnham, *American Party Systems*, p. 5.

[8] The last sentences are paraphrases from William G. Shade, personal communication.

[9] The passages which follow will argue in effect that the first parties do not meet the criteria of party set forth by one of their leading historians, as, e.g., in William N. Chambers, *Political Parties in a New Nation: The American Experience, 1776–1809* (New York: Oxford University Press, 1963), pp. 44–48. Since then, however, Chambers has proposed a very useful taxonomy of political parties; see W. N. Chambers, review of *Political Parties Before the Constitution*, by Jackson Turner Main, *Reviews in American History*, 1 (December, 1973), 499–503.

[10] One representative work among many is David Easton, *The Political System: An Inquiry Into the State of Political Science* (New York: Knopf, 1953).

[11] Joseph Charles, *The Origins of the American Party System: Three Essays* (Williamsburg, Va.: Institute of Early American History and Culture, 1956); Manning J. Dauer, *The Adams Federalists*, 2nd ed. 1953; (Baltimore: Johns Hopkins University Press, 1968); Noble E. Cunningham, Jr., *The Jeffersonian Republicans: The Formation of Party Organization, 1789–1801* (Chapel Hill: University of North Carolina Press, 1957); Cunningham, *The Jeffersonian Republicans in Power: Party Operations, 1801–1809* (1963). A similar error is made by Lisle A. Rose, *Prologue to Democracy: The Federalists of the South, 1789–1800* (Lexington: University of Kentucky Press, 1968), which otherwise offers useful concepts.

Viewed as a group, these works display a disturbing symmetry between their methods and conclusions. In taking a nationwide perspective they quite naturally analyzed party formation *from the top down.* Party conflict originated, they found, at the center, in Washington's cabinet and the national legislature. As faction turned into party it radiated out to the states after 1794–95, with national elites stimulating local party cadres into being. It can be easily argued, as these historians know, that these activities did not constitute purposive party building.[12] Nor, in my view, did they intentionally set out to perform political functions. Policy conflicts came first, followed by attempts to reach out to state and local factions for support. Diverging elites increasingly sought electoral support to maintain, increase, or gain power, to be used to initiate, protect or change policy. This argument might be extended, but my initial objection is simply that the top down method exaggerates the extent of party penetration across the country. For example, focusing on the "apex" year of 1800, historians in search of national parties have been able to create them by piecing together a wide assortment of campaign activities up and down the Atlantic seaboard. But the fragments so assembled have not been shown to exhibit Chambers's criteria of durability and regularity over time.[13]

Moreover, recent monographs by Alfred Young, Paul Goodman, and James Banner, although written within the first system mold, contradict the thesis that party formation flowed from the center outward. All three authors emphasize internal stimuli and indigenous historic conflicts. Young concluded that "the national party appears to have been no more than a loose amalgam of state groups." Goodman spoke of Federalists and Republicans as "loose collections of provincial interests. . . . Highly local, evolving from rivalries within towns and cities, counties and states, they appealed to an electorate without firmly anchored, hereditary loyalties."[14] These studies suggest that the initial vantage point from which scholars describe party formation affects their interpretation of origins.

It would be misleading to leave the impression that only recent state monographs have questioned elements of the first system model. Take, for example, Pennsylvania, a large, heterogeneous state which is universally said to have produced sophisticated parties far in advance of other states. Given the location of the national government in Philadelphia during the 1790s, it has been taken for granted that Pennsylvanians, led by a vanguard of professional politicians, responded early to "national stimuli." Yet a close reading of the standard books covering Pennsylvania politics from 1790 to 1816, by Harry M. Tinkcom and Sanford W. Higginbotham, will not carry the burden of party institutionalization which the first system syndrome has anachronistically laid upon it.

Tinkcom made clear that party names remained uncertain throughout the 1790s. When the terms "Federalist" and "Republican" were used *before 1796,* they "most accurately applied to opinion groups, to ideological 'climates,' " not to "parties which in the mass designated themselves as Federalist or Republican." Nor in 1796 did leading newspapers use consistent party labels, either in reference to their opponents or themselves.[15] County nominating meetings across the state presented "tickets" for state legislative elections, but these assemblages were not "self-avowed party groups. . . . Almost invariably the candidates were sponsored by the conventional 'numerous and respectable meeting of citizens.' " Yet by 1797, Tinkcom said, "general party terms were beginning to creep into editorial phraseology."[16] Tinkcom's evidence for this claim, as for many like it, came principally from Philadelphia newspapers. Indeed, it is not too unfair to say that Tinkcom's study of party emergence in Pennsylvania from 1790 to 1801 very much reflected the surfacing of partisan consciousness in the minds of two Philadelphia editors, John Fenno and Benjamin F. Bache.[17] Tinkcom did find individual instances of advanced party thinking in 1797 and 1799, but the absence of party thinking and organization impressed this reader far more. In 1799, for example, an "assembly of plain republicans" nominated ("Federalist") James Ross for governor. After the election, the victorious "Republican" candidate Thomas McKean did not call himself a Republican but favored the term "Whig" as he had throughout the decade. In that election "there was much stealing of thunder; the Republicans insisted they were the true 'Federalists,' and their opponents . . . declared with equal vigor that they were the 'real republicans.' "[18]

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[12] For an excellent synthesis that is not tied to the first system model and admirably eschews "party" for the period 1790s–1820s, see Roy F. Nichols, *The Invention of the American Political Parties: A Study in Political Improvisation* (New York: Macmillan, 1967), esp. pp. 158–247. On partisan consciousness in the 1790s, see Franklyn George Bonn, Jr., "The Idea of Political Party in the Thought of Thomas Jefferson and James Madison" (Doctoral dissertation, University of Minnesota, 1964).

[13] Durability and regularity are criteria suggested by Chambers, *Political Parties in a New Nation,* pp. 45–47.

[14] Alfred F. Young, *The Democratic Republicans of New York: The Origins, 1763–1797* (Chapel Hill: University of North Carolina Press, 1967), pp. 570, 578. Paul Goodman, *The Democratic Republicans of Massachusetts: Politics in a Young Republic* (Cambridge, Mass.: Harvard University Press, 1964), p. 204. James M. Banner, Jr., *To the Hartford Convention: The Federalists and the Origins of Party Politics In Massachusetts, 1789–1815* (New York: Knopf, 1970), pp. 217, 221, 227–28. A similar point is made by Norman K. Risjord, "The Virginia Federalists," *Journal of Southern History,* 33 (November, 1967), 486–517.

[15] Harry Marlin Tinkcom, *The Republicans and Federalists in Pennsylvania, 1789–1801: A Study in National Stimulus and Local Response* (Harrisburg: Pennsylvania Historical and Museum Commission, 1950), pp. 52, 154, 161–62. Compare Charles, *Origins,* p. 42, n. 82.

[16] Tinkcom, pp. 179–80.

Higginbotham took up where Tinkcom left off. Although summaries of both books often lead one to believe otherwise, Higginbotham did not show that a party system "took off" in Pennsylvania after 1800. Rather, the Federalists "virtually disappeared" by 1802, while the Republicans, who had organized at the local level, spiralled into factionalism. By 1805 the contest for governor was between two Republican factions, with the winning coalition composed of incumbent Republicans and Federalist allies. Partisan conflict revived in 1808, and again in 1812, as foreign trade and war issues brought Federalism back to life. Even during the War of 1812, however, party conflict lost its edge, and an indestructible Republican factionalism persisted.

Higginbotham provided little evidence for an ongoing two-party system. For national elections the Federalists were nonexistent. Their organization was so casual "as to make it a real question whether they constituted a party or a state of mind."[19] The Republicans had local organizations, but their state party was little more than a loose collection of county groups. Higginbotham concluded that "there existed no continuing party organizations on the national level, nor even within that states. Furthermore, the course of politics in Pennsylvania was principally concerned with state and local rather than national issues."[20] Pennsylvania Republicans never developed a regular mode of nominating state candidates, and experimented with caucus, convention or mixed methods throughout the period. Does all this deserve to be called a "system"?

Doubts about the first system also arise from flaws in descriptions of party formation in Congress. One scholar discovered two self-conscious parties "crystallizing" in 1792 in the national legislature.[21] Joseph Charles put party emergence no earlier than 1794 and, on balance, favored 1795. Cunningham and Chambers located the nearly complete polarization of Congress in the Jay Treaty session of 1795–96. Dauer's evidence, as I read it, could make a case for the 1797–98, Alien-Sedition Congress.[22] Quite recently, a quantitative study of congressional roll calls from 1789 to 1796 concluded that two voting blocs "which were forged in the First Congress had reached maturity by the Third, well before the Jay Treaty controversy. By 1795 not only divisive foreign policy decisions, but also domestic matters and governmental routine, had become the objects of systematic party dispute."[23] This latter study seems to represent a severe case of the voting bloc fallacy—by which parties are equated with voting blocs—which can also be found in the general works. As James S. Young pointed out in his provocative study, *The Washington Community, 1800–1828* (1966), the tendency of legislatures to form voting blocs does not automatically prove the presence of party in proportion to the cohesion of the blocs. After analyzing the early government community as a political anthropologist might, Young maintained that patterns of congressional voting correlated highly with the boarding-house residences of legislators. The members tended to live mostly with men from their own regions and, with great regularity, to vote with these same messmates. Although Young should have tested more thoroughly for partisan motivation before reading it out of the picture, his book constitutes a thrust at the very nerve center of the first system, meriting critical attention. It cannot be refuted by ignoring it. The issue is not simply one of regionalism versus party, but rather of a multiplicity of motives and influences affecting legislative behavior. A more flexible approach to party development should incorporate and transcend Young and the more conventional view.[24]

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[17] Tinkcom, p. 191. Bache's death in 1798 interfered with Tinkcom's observation of parties because, as Tinkcom said, no other Philadelphia publisher emulated his advanced evaluation of parties.

[18] Pro-party thinking, Tinkcom, pp. 192–93, 236–37; regarding Ross, pp. 229, 309; McKean, pp. 253, 262; 1799 election, pp. 237–38. The partisan vocabulary used on a patronage list by William Duane in 1801 is quite revealing, p. 267.

[19] Sanford W. Higginbotham, *The Keystone in the Democratic Arch: Pennsylvania Politics, 1800–1816* (Harrisburg: Pennsylvania Historical and Museum Commission, 1952), p. 19.

[20] Higginbotham, pp. 329–30.

[21] John C. Miller, *The Federalist Era, 1789–1801* (New York: Harper and Row, 1960), p. 99.

[22] Dauer, *Adams Federalists,* pp. 132, 133, 135, 170–71.

[23] Mary P. Ryan, "Party Formation in the United States Congress, 1789 to 1796: A Quantitative Analysis," *William and Mary Quarterly,* 28 (October, 1971), 541. In a detailed critique of Ryan, Henderson accepts the first system framework, yet suggests strategies which potentially transcend it, H. James Henderson, "Quantitative Approaches to Party Formation in the United States Congress: A Comment," With a Reply by Mary P. Ryan, *William and Mary Quarterly,* 30 (April, 1973), 307–24, especially pp. 319–20.

Multivariate analysis of roll calls needs to be accompanied by evidence that men ran for Congress as openly identified members of a party. To show convincingly that the core factions of Federalist and Republican interests came to think of themselves in party terms, consideration must be given to legislators' self-images, and their concepts of role, loyalty, and representation.[25]

With respect to the self-image of political leaders in the 1790s, it is generally agreed that they did not see themselves as party builders.[26] Furthermore, to the extent that Federalists or Republicans became conscious of "party," most seem to have deliberately rejected the norms of systematic party competition. Men largely remained rooted in an older politics, one that remained hostile to the idea of party as "legitimate opposition." Some state parties tried to eliminate the opposition until well into the nineteenth century. Jefferson as president was not above the Sedition Law effort of trying to silence the opposition. Public officials and electorates did not believe that virtue demanded adherence to party norms.[27] Rather, many thought party at best a necessary evil and some activists did not come to revere party as a positive good until 1840 and beyond. Resistance subsided unevenly to the idea of party as an *enduring institution commanding a separate loyalty*.

Party as Organization, 1789–1840

Using Sorauf's tripartite approach to observing parties, one should be able to assess roughly the degree of institutionalization of the first parties as organizations, in office, and in electorates. This analysis can also serve to indicate some research opportunities. Federalist and Republican national organizations proper, for example, were incipient at best, and research here may have been carried as far as it's going to be.[28] Still, a long-run analysis of national organizations has not been attempted, and histories of the 1824 to 1844 period are rife with conflicting claims about when national organizations took charge of campaigns. As late as 1836, however, the Whig "party" had no less than four favorite-son sectional candidates running for president. Yet historians profess to see national party organizations emerging even earlier than this national anti-Democratic coalition.[29]

Furthermore, although the study of organizations is the study of men, the organizers themselves have received little attention. We need studies of elite motivation, socialization, and recruitment. Specifically, the much heralded replacement of traditional notables by a "new class" of professional politicians should be systematically studied. Rising professionals, like declining deference, litter the entire early national period. These catch-phrases sprawl everywhere across the era as loose, all-purpose, and unexamined hypotheses. Collective biographies of local or regional elites could help resolve some of these questions empirically.[30]

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[24] A developing rebuttal is mostly unpublished at the moment, e.g., Harry William Fritz, "The Collapse of Party: President, Congress, and the Decline of Party Action, 1807–1817." (Doctoral dissertation, Washington University, 1971); and Ronald L. Hatzenbuehler, "Foreign Policy Voting in the United States Congress, 1808–1812." (Doctoral dissertation, Kent State University, 1972). Hatzenbuehler has published an article, discussed below. Fritz showed that formal partisanship hardly existed in 1809, but from 1812 to 1815 found a high degree of voting cohesion, especially on war related issues. The return of peace, however, "demolished the party system." Fritz directly challenged Young's revisionism, but seemed to replace it at several key points only with assertions, ignoring particularly the problem of party label and self-consciousness, p. 241, n. 51, and ff.

[25] Extensive projects of congressional roll call analysis are underway, descriptions of which appear frequently in the *Historical Methods Newsletter,* e.g. 5 (June, 1972), 139.

[26] The modern party, in Paul Goodman's words, was outside the range of consciousness of that generation. Goodman, "The First American Party System," in *American Party Systems,* ed. Chambers and Burnham, p. 57.

[27] Robert A. Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966). Richard Hofstadter, *The Idea of a Party System: The Rise of Legitimate Opposition in the United States* (Berkeley: University of California Press, 1970). Leonard W. Levy, *Jefferson and Civil Liberties: The Darker Side* (Cambridge, Mass.: Harvard University Press, 1963), pp. 42–69.

[28] James Staton Chase, "Jacksonian Democracy and the Rise of the National Nominating Convention," *Mid-America,* 45 (October, 1963), 229–49; Banner, *To the Hartford Convention,* pp. 295–312.

[29] Joel H. Silbey, "Election of 1836, in *History of American Presidential Elections, 1789–1968,* Arthur M. Schlesinger, Jr., and Fred L. Israel, eds. (New York: McGraw-Hill, 1971), I, *1789–1844,* 577–600. Richard P. McCormick, *The Second American Party System: Party Formation in the Jacksonian Era* (Chapel Hill: University of North Carolina Press, 1966), pp. 13–15, 238–40. The essays in the Schlesinger-Israel volume testify to the absence of national organizations before 1840.

Any redistribution of influence or power could be illuminated further by studies focused on public and private patronage. Carl Prince's study of New Jersey's Jeffersonian Republicans, although also conforming to first system orthodoxy, pointed to what might be done regarding state patronage.[31] Prince identified 256 Republican activists during the period 1800 to 1816, and found that nearly two-thirds held appointive office. Thus the party rewarded its activists. Or did it? The existence of an institutionalized party and unambiguous party identity is not indisputably established by Prince.[32] Appointment policy generally seems not to have become governed completely or even mostly by partisan considerations. Only slightly more than 20 per cent of *all* state and federal appointees in New Jersey from 1800 to 1816 were veteran Republicans. In 1816, 22.3 per cent of federal appointees were known to be active partisans.[33] On what bases were the remaining three-quarters selected? The Republicans dominated appointments during the era almost completely, and the balance could not all have been Federalist holdovers. Prince's data thus raise a question about *patronage modes* in general. The data might be taken as an index of limited penetration of party into patronage. More certainly, they suggest the need for inquiry into the universe of patronage practices from 1789 to 1840.[34]

Such an approach to patronage offers a way of getting at the alleged relationship between "rising party" and "declining deference." Types of patronage probably changed from the 1780s to the 1840s. Eighteenth century patronage should have reflected a traditional social setting, characterized by a *patron-client* type of patronage. Family connections would surely be important in this milieu, and, indeed, family continued to be important in state and federal patterns of job distribution throughout the early republic.[35] With the rise of party, however, the influence of family and local notables could be expected to decline. In the context of party, expanded suffrage, and a mobilized electorate, political party patronage would increasingly mediate between citizens and government. Party elites in turn would use patronage resources to serve electoral and private ends, and these leaders would become more important relative to other elite groups.[36] The movement from a patronage centered on clients and family to one centered on interest-groups and constituents might form part of the story of the decline of deference. Change from one mode to another occurred gradually: in Prince's New Jersey, for example, patron-client modes seem to have continued mixed with party directed patronage well into the nineteenth century.

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[30] A provocative essay which attempts to put the professionals' rise in perspective is Lynn W. Marshall, "The Strange Stillbirth of the Whig Party," *American Historical Review*, 72 (January, 1967), 445–68; note Marshall's disagreement with Chambers, p. 462, n. 47. The approach recommended here might emulate: James Q. Wilson, *The Amateur Democrat: Club Politics in Three Cities* (Chicago: University of Chicago Press, 1962).

[31] Carl E. Prince, *New Jersey's Jeffersonian Republicans: The Genesis of an Early Political Machine, 1789–1817* (Chapel Hill: University of North Carolina Press, 1967).

[32] In an article on Jefferson's patronage policies, 1801–03, Prince showed that of 316 strategic second level offices Jefferson removed 146 (46 per cent), 118 of whom Prince identified as "hardcore Federalist party cadre," "The Passing of the Aristocracy: Jefferson's Removal of the Federalists, 1801–1805," *Journal of American History*, 57 (December, 1970), 565. But activists may be dismissed or rewarded for electioneering, chairing meetings, etc., without there necessarily being a party system. Though Jefferson's removals may not have differed much from Jackson's (p. 566), one must still wonder why Jackson's patronage actions had much greater impact on what men thought about politics and spoils. That political folklore regarded Jackson's administration as a turning point was not solely a creation of Mugwump historians; see Leonard D. White, *The Jacksonians: A Study in Administrative History, 1829–1861* (New York: Macmillan, 1954), pp. 301, 308–11.

[33] Prince, *New Jersey's Jeffersonian Republicans*, pp. 223, 245.

[34] For comment on persisting "traditional local structures" within centralized, "modern" polities, see Stein Rokkan, "The Comparative Study of Political Participation: Notes Toward a Perspective on Current Research," in *Essays on the Behavioral Study of Politics*, ed. Austin Ranney (Urbana: University of Illinois Press, 1962), p. 70.

[35] Prince, *Jeffersonian Republicans*, pp. 240–42; Sidney H. Aronson, *Status and Kinship in the Higher Civil Service: Standards of Selection in the Administrations of John Adams, Thomas Jefferson, and Andrew Jackson* (Cambridge, Mass.: Harvard University Press, 1964).

[36] Alex Weingrod's discussion of such a change in a Sardinian village, 1870–1950, is highly suggestive. Rather than the landlord, it is "the party functionary who provides information regarding loan programs, who can contact the government's tax collector on the villager's behalf, or who can send his son on to high school. The new men of influence are thus likely to be political men, and it is their ability to deal effectively with the wider system that gives them power. Patron-client relationships may continue . . . but it is more likely that political-party directed patronage becomes increasingly significant: the party boss and his workers—the professionals—control even wider resources and they are likely better to provide for their 'constituents' than the patron can for his 'clients.' " "Patrons, Patronage, and Political Parties," *Comparative Studies in Society and History*, 10 (July, 1969), 384.

Space permits only briefest mention of other possible themes of "organization proper." The role of voluntary associations and political societies has not been adequately assessed.[37] Similarly, tantalizing hints exist of the political utility of militia companies which may have contributed more than "color" to campaigns through the 1820s. In the absence of parties before the 1830s, militia outfits, political societies (Cincinnati, Washington Benevolent, Democratic Republican), and secret orders such as the Freemasons may have acted as recruiting and training agents for the political establishment generally. In western New York's Genesee County, for example, militia companies and Freemasons' lodges seem to have performed just such a function in the 1820s.[38]

Some objects of inquiry obviously span two or three elements of party structure, such as the significant shift from caucuses to conventions as a means of making nominations. Solid work has been done on this process for the Federalist-Republican period and the late 1820s,[39] but not since Ostrogorski, perhaps, has anyone tried to explain the entire process. The conventions eventually assumed control of a mobilized electorate, and their rise is interwoven with the decline of deference. Whether conventions were "more democratic" or not is still a subject of debate.

The Party in Office

Historical observation of the party in office, as suggested above, is concentrating on party in legislatures. Roll-call analysis offers a direct means of measuring interparty conflict and intraparty cohesion. Yet the various quantitative techniques now being applied to early legislatures will not alone serve to dispel doubts about the presence of self-conscious parties. J. S. Young's findings for the Congress, 1800–1828, combined with other recent studies, suggest that party voting cannot be taken for granted as a fixed, all-pervasive condition. Rather, *party voting* (and bloc voting) *should be conceived of as a variable within individual legislatures and across series of legislative sessions.* In Congress before the 1820s, for example, peaks of party or proto-party voting probably developed during war and foreign policy crisis sessions of 1797–1800 and 1812–1814. Scrutiny of individual sessions in that period suggests that incipient party voting fluctuated over the course of months and weeks.[40] Roll-call analysis can be used to identify precocious partisan types, men who promoted norms of party regularity, and whose style looked ahead to the ethos of an institutionalized party system.

The study of legislatures in other periods offers embarassing riches of sophistication in methods, although state legislatures before the Civil War are generally almost *terra incognita* to historians.[41] Between the 1780s and 1830s, particularly, the historical landscape with respect to state legislative behavior is a great desert, and one to be approached with caution. J. R. Pole warned that assemblies of this period in their usages and language often bore a "most deceptively modern appearance." Pole would like to know more about "what the choice between rival candidates really meant to voters in terms of interest and policy."[42] Similarly, we need to know to what extent party entered into such choices. A recent analysis of roll calls in six legislatures from 1833 to 1842 is suggestive. In it, Herbert Ershkowitz and William Shade argue that broad ideological differences separated Whig and Democratic state legislators. Indirectly, the authors indicate that party cohesion in the legislatures did not rise markedly before 1835, in some cases 1836 or 1837.[43] Such differential rates of party formation in state legislatures offer an excellent opportunity for comparative analysis.

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[37] Eugene Perry Link, *Democratic-Republican Societies, 1790–1800* (New York: Columbia Univ. Press, 1942), made a beginning. The Society of Cincinnati was very important to the "Federalist interest" in the South in the 1790s (Rose, *Prologue to Democracy*, pp. 19–45).

[38] Kathleen Smith Kutolowski, "The Social Composition of Political Leadership: Genesee County, New York, 1821–1860" (Doctoral dissertation. University of Rochester, 1973).

[39] George D. Luetscher, *Early Political Machinery in the United States* (Philadelphia: University of Pennsylvania Press, 1903), and David H. Fischer, *The Revolution in American Conservatism: The Federalist Party in the Era of Jefferson* (New York: Harper 1965), are valuable on the early period. For the 1820s, Robert V. Remini, *The Election of Andrew Jackson* (Philadelphia: Lippincott, 1963).

[40] This hypothesis is based on my reading of such conflicting sources as Young, *Washington Community;* Roger H. Brown, *The Republic in Peril: 1812,* 2nd ed. (1964; New York: Norton, 1971); and particularly Ronald L. Hatzenbuehler, "Party Unity and the Decision for War in the House of Representatives, 1812," *William and Mary Quarterly,* 29 (July, 1972), 367–90. Particularly suggestive are Hatzenbuehler's charts showing Republican and Federalist legislators' indices of cohesion values for nonunanimous Foreign Policy Roll Calls, Twelfth Congress, First Session, House, 387, 388. Congressional parties' strength in the 1840s is demonstrated in Joel H. Silbey, *The Shrine of Party* (Pittsburgh: University of Pittsburgh Press, 1967).

[41] Pertinent bibliographies appear in: Robert P. Swierenga, ed., *Quantification in American History: Theory and Research* (New York: Atheneum, 1970), pp. 127–30; and Charles M. Dollar and Richard J. Jensen, *Historian's Guide to Statistics: Quantitative Analysis and Historical Research* (New York: Holt, Rinehart, 1971), pp. 277–81.

[42] J. R. Pole, "Suffrage and Representation in Massachusetts: A Statistical Note," *William and Mary Quarterly,* 14 (October, 1957), 578.

The historical problem of applying *party labels* to political factions before 1840 must be addressed if quantitative methods are to have meaning. Too many anachronisms have been created, for example, in the name of the *Biographical Directory of Congress,* which is often not reliable for party identification. Meanwhile, the recognizable label is crucial to current definitions of party. The International Comparative Political Parties Project, now engaged in a comprehensive, comparative analysis of parties throughout the world, requires that "to qualify as a political party, an organization must have as one of its goals that of placing its *avowed representatives* in government positions."[44] If strictly applied, this criterion of open identification would shatter confidence in the concept of the first party system. The description of party labels as "fluid" in the 1790s is an understatement, and after 1800 many Federalist and Republican candidates showed ambivalence and reluctance to run as "avowed representatives."[45] Names are hardly trivial matters, as Freud and others have taught us.

In formal terms the question of party labels amounts to whether the Federalist and Republican coalitions *as parties* constituted significant reference groups. Identification with Federalist or Republican "interests" might have been qualitatively different from later Democratic and Whig party loyalty.[46] If so, we are led again to consider a transformation of political culture.

Party and Voters, 1789–1840

Discovering the party as reference group in the electorate imposes no easy task on historical research. Interpreters of the first system readily admit the difficulty of observing electoral behavior as well as the instability of voter loyalties. To some extent the finding of instability may be related to limited visibility. The problem may be conceived of broadly as requiring the discovery of historical patterns of social cleavage which became translated into party conflicts. Considered in the mass, parties are coalitions built on hierarchies of cleavage bases, such as section, region, ethnicity, religion, economic interest group, and class, which take different ranks over time as policy and value commitments shift. Research on the early national-antebellum period can contribute much to a general need for information about "the processes through which political alternatives *get set* for different local electorates."[47]

The early national electorate, at the mercy of the Law of Available Data, has tagged along as the stepchild of party history in this era. There are descriptions, old and new, of the social bases of Federalist and Republican allegiance. They tend to center on 1800, while the period of obvious party collapse after 1815 seems peculiarly invulnerable to analysis of social cleavages, except in gross sectional terms, as if only parties left behind traces of the social bases of politics.[48]

Descriptions of Federalist and Republican voters are unpersuasive, although discussions of patterns of elite division probably cannot be much improved.[49] One recent attempt to describe Federalist and Republican voters in 1800 appeared as a 26 page appendix to David H. Fischer's influential book on the Federalist party. Fischer admitted that his essay was based on impressionistic evidence and fragments of electoral returns, but managed to scan the entire nation in a suggestive analysis. His task was made easier by the vast territory which could be excluded from consideration: remote areas of low population density and poor communication, where "party consciousness was not sufficiently clear to be meaningful."[50] These included several western states and sizeable internal frontiers in older states. Fischer identified a number of variables influencing party choice, but leaving aside his findings, objection may be made to the very enterprise —common among historians of the first parties—of assuming the existence of party in the electorate, and not regarding voter loyalty *per se* as meriting its own proofs.[51]

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[43] Ershkowitz and Shade, "Consensus or Conflict?," *Journal of American History,* LVIII, 597, 599, 603, 613, 594–613 *passim.* Other work is underway in this area: Peter Levine; "Party Behavior in the New Jersey Legislature: 1829–1844," and Rodney O. Davis, "The Influence of Party on Political Leadership in Illinois in the Jacksonian Era," Papers presented at the Organization of American Historians annual meeting, Chicago, April 13, 1973.

[44] Kenneth Janda, *A Conceptual Framework for the Comparative Analysis of Political Parties* (Beverly Hills, Calif.: Sage Publishers, 1970) pp. 77, 83. "The project will cover some 150 political parties in 50 countries, consulting a random sample of party systems stratified equally according to 10 cultural-geographic areas of the world [for 1950–1970]," p. 77.

[45] Regarding New York in the 1790s, Young, *Democratic-Republicans;* even some ultra-Federalists after 1800, Fischer, *Revolution in Conservatism;* New York in the 1820s, Alvin Kass, *Politics in New York State, 1800–1830* (Syracuse: Syracuse University Press, 1965). George R. Nielsen ("The Indispensable Institution: The Congressional Party During the Era of Good Feelings" (Doctoral dissertation, University of Iowa, 1968) found party labels "not available" until the late 1820s, p. 3.

[46] Michael Wallace, "Changing Concepts of Party in the United States: New York, 1815–1828," *American Historical Review,* 74 (December, 1968), 453–91; Ronald P. Formisano, "Political Character, Antipartyism, and the Second Party System," *American Quarterly,* 21 (Winter, 1970), 683–709.

[47] Seymour Martin Lipset and Stein Rokkan, "Cleavage Structures, Party Systems, and Voter Alignments: An Introduction," in *Party Systems and Voter Alignments: Cross-National Perspectives,* ed. Lipset and Rokkan (New York: Free Press, 1967), pp. 5, 6, 53; also, Stein Rokkan, *Citizens, Elections, Parties: Approaches to the Comparative Study of the Processes of Development* (New York: McKay, 1970).

[48] For detailed study of social bases of political cleavage in a pre-party situation, Van Beck Hall, *Politics Without Parties: Massachusetts, 1780–1791* (Pittsburgh: University of Pittsburgh, 1972).

Putting first things first, partisanship as such, apart from its social bases, must be shown before the group bases of party voting can be discussed sensibly. Whatever the methods, some attempt must be made to show that parties were reference groups in the electorate, that stable loyalties existed throughout the political universe. Monographs dealing with specific localities over time would be particularly welcome.

ers."[52] Judging by the evidence now available,
The ICPP Project considers an institutionalized party as "one that is reified in the public mind so that 'the party' exists as a social organization apart from its momentary leadthis phenomenon does not seem to have emerged until the late 1830s; then followed the first classic period of party loyalty.[53]

Some historians nevertheless claim to find party loyalty of this type as early as the 1790s because they discern then the appearance of ticket voting for slates of candidates. In a preparty situation, however, ticket voting may exist without institutionalized parties. Ticket voting alone does not establish the presence of a self-conscious party in the electorate, which is a durable psychic phenomenon.[54]

Unlike party loyalty and social cleavages, one feature of voting behavior from 1789 to 1840 has been charted. J. R. Pole and others have described a gigantic leap in voter turnout in state elections after 1800. In the late 1790s turnout in some states began to rise from levels of between 15 and 40 per cent. From 1804 to 1816, however, in most states "an extraordinary surge" carried participation "to unprecedented heights—68 to 98 per cent of adult males." In the 1820s turnout in state elections began to decline, and presidential polls gained attention, until in 1840 presidential voting equalled or surpassed earlier totals in state elections.[55]

For many writers electoral mobilization on this scale means "democratization" or "the expansion of voter participation in an increasingly open and free electoral process."[56] As noted earlier in this essay, many historians credit party competition and organization with causing boom turnouts, and thus yoke together "party" and "democracy." Others blink at the dazzling vote totals and inquire whether such electoral fireworks implied any reallocation of power. While voters responded to party "projections" of democratic procedures, according to Prince, small coteries still ran politics. McCormick wondered if high turnouts caused parties to promote and governments to enact more democratic policies, and seemed to conclude that such effects were minor.[57]

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[49] A recent summary of the literature on elites and voters is in Richard Buel, Jr., *Securing the Revolution: Ideology in American Politics, 1789–1815* (Ithaca: Cornell University Press, 1972), pp. 72–90, 319–24. Buel's conclusions are subject to the same criticism as Fischer's. Notable strength is indicated by Buel's comment that "during the formative stage of the first party system, the disposition of the regional leadership was often more critical than anything else in determining the political complexion of an area." p. 75.

[50] Fischer, *Revolution in Conservatism*, pp. 202–03.

[51] Interyear correlations for gubernatorial elections in New York from 1792 show that stable electoral cleavage developed only from 1809 to 1816 and not again until the 1830s (Lee Benson, Joel Silbey, and Phyllis Field, "Toward a Theory of Stability and Change in American Voting Patterns: New York State, 1792–1972," Paper presented at the Mathematics Social Science Board Conference on Quantitative Studies in Popular Voting Behavior, Cornell University, June, 1973). A strong argument, however, for stable partisan cleavages in Maryland's electorate for two decades after 1798 has recently been made by David A. Bohmer, and John M. Rozette, "Toward the Study of Individual Level Historical Voting Data: Some Theoretical and Practical Considerations," unpublished manuscript. Bohmer and Rozette discuss a number of studies now underway using invaluable poll book data for individual voters, which should go a long way toward resolving some of the issues raised above.

[52] Janda, *Conceptual Framework*, p. 88.

[53] For general discussion of the 1830s "new politics" and of relevant literature see Edward Pessen, *Jacksonian America: Society, Personality, and Politics* (Homewood, Ill.: Dorsey, 1969), pp. 154–307, 375–83; pertinent studies include: Lee Benson, *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton: Princeton University Press, 1961), and Ronald P. Formisano, *The Birth of Mass Political Parties: Michigan, 1827–1861* (Princeton: Princeton University Press, 1971).

[54] For a case of ticket voting without party, see Tinkcom, pp. 170–71, 239–40, 256.

[55] Fischer, p. xv, summarizing works by Pole and McCormick.

[56] Fischer, p. xi.

These disagreements about the democratizing role of party need more systematic study. Meanwhile, most scholars share a tendency to exaggerate the causal impact of party organization in raising turnout and creating interest in politics generally.[58] Celebrants and critics alike have neglected history and sociology in favor of too mechanistic a political science. What kinds of events and social cleavages came together to raise political consciousness in attentive publics? What historical and social conditions favor both leader activism and mass turnout? Why did some states reach high levels of turnout very quickly? Why did some counties in the same state differ substantially in turnout rates?[59] Comparative analysis here contains much potential for understanding change in political culture.

Rather than regarding organization solely as an independent variable,[60] organization and turnout should be seen also as dependent variables. Both are two forms of participation which involves several modes of action besides voting: campaigning, citizen initiated contacts with government, and cooperative activity. Studies of participation based on cross-national data conclude that voting is only weakly related to other modes and to "general political involvement."[61] This means that historians interested in shifting citizen orientations cannot be exclusively concerned with voting and need to diversify approaches to participation.

The Decline of Deference

While the rise of participation has at least a broad index for one of its modes, the long decline of deference tends to elude measurement. Any student passingly familiar with the early republic knows that deference ended sometime during these years, enjoying a decorous but rather drawn-out finale. Its definite passing away has probably been claimed for every decade from the Revolution to the Civil War. Many writers favor the 1820s, while others see declining deference permitting the growth of parties as early as the 1790s.

More than any other word, "deference" characterizes eighteenth century Whig political culture by referring directly to men's ideas and unspoken assumptions about how society actually worked.

> Fundamentally, deference meant the acceptance of the view by the whole of society that, whether by chance or simply by habit, people would naturally delegate power to a select minority. The belief that this minority would govern in the interests of the entire population was the unifying spirit that bound the society together. According to Professor Pole: "In a society whose moral cohesion was supplied by the sense of deference and dignity, it was possible for the broad mass of the people to consent to a scheme of government in which their own share would be limited."[62]

In the first-system literature, declining deference and rising parties are now virtually locked in a symbiotic embrace. In this happy union, one serves to explain the other. From 1795 to 1816, increased voting did not result simply from an enlarged franchise; rather, the inhibitions formerly imposed on the exercise of the suffrage by habits of subordination were gradually broken down by party competition. Thus, anything undermining deference prepared the way for parties, and parties hastened the departure of deference and spurred voter turnout.

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[57] McCormick, "Political Development and the Second Party System," ed. Chambers and Burnham, p. 107. Prince, *Jeffersonian Republicans*, p. 249. Prince's discussion of these issues is excellent, and it is somewhat arbitrary to classify him in this way. Edward Pessen maintains that party's advent did not affect existing policy preferences in administration of city governments: "Who Governed the Nation's Cities in the 'Era of the Common Man'?," *Political Science Quarterly*, 87 (December, 1972), 591–614. See also, Richard B. Dawson, "Social Development, Party Competition, and Policy," ed. Chambers and Burnham, pp. 203–37.

[58] Exaggerating organization: Banner, *To the Hartford Convention*, pp. 268–93; Richard P. McCormick, "New Perspectives on Jacksonian Politics," *American Historical Review*, 65 (January, 1960), 288–301. Fischer suggested that party competition, organization and turnout went together, cf. pp. xi–xx, 182–99, and especially 187–92.

[59] Compare, e.g., voting data from St. Mary's County (low) from 1790 to 1812 with those for Prince George's (and other counties with high turnouts) in Maryland; see J. R. Pole, "Constitutional Reform and Election Statistics in Maryland, 1790–1812," *Maryland Historical Magazine*, 55 (December, 1960), 285–92.

[60] Modern studies disagree about party organization's impact on turnout, but few political scientists claim that organization raises turnout by much more than 5 per cent; see William J. Crotty, "Party Effort and Its Impact on the Vote," *American Political Science Review*, 65 (June, 1971), 439–50.

[61] Sidney Verba, Norman H. Nie, and Jae-on-Kim, *The Modes of Democratic Participation: A Cross-National Survey* (Beverly Hills, Calif.: Sage Publishers, 1971), pp. 10, 11–19, 63.

[62] John B. Kirby, "Early American Politics—The Search for Ideology: An Historiographical Critique of the Concept of 'Deference,'" *Journal of Politics*, 32 (November, 1970), 827. For a discussion of deference in relation to social science literature, Richard D. Brown, "Modernization and the Modern Personality in Early America, 1680–1865: A Sketch of a Synthesis," *Journal of Interdisciplinary History*, 2 (Winter, 1972), 214–20.

Yet the matter of timing remains as a source of confusion. Apart from general disagreement, competent historians seem prone even to self-contradiction because their application of the concept is so promiscuous.[63] It is also common to associate declining deference with large, heterogeneous, differentiated states such as New York and Pennsylvania. It seems logical that states with advanced social complexity should have experienced first the decline of traditional politics and the emergence of professional politicians adept at brokerage and organization. The New York-Pennsylvania syndrome thus produces a set of nicely fitting "correlates": economic development, social complexity, heterogeneity, declining deference, rising parties: democracy. Yet we are also told, and it has long been recognized, that habits of subordination remained strong in New York for a long time. The importance of notables in Pennsylvania politics clearly manifested itself in 1799, and the tide of party there after 1800, as discovered earlier, seems to have been less than overwhelming. The assumption that "economic development leads to pluralistic, competitive political structures," by natural law as it were, has been called a "naive and culturally biased outlook [which] confuses political development with political democratization."[64]

The transition from deference to Participation, Parties, & Co., should not be seen, I believe, as absolute. New ways are hardly ever entirely devoid of the old. The content of tradition in political parties has not been sufficiently emphasized, especially during the formative period from the 1790s to the 1840s. Fascinated with parties as a sharp departure in Western political culture, historians have tended to underplay or ignore the extent to which these organizations fused with and synthesized traditional forms.

Interest groups, for example, came to be part of a new "pluralist" order. What were the phases of transition from deference to groups? To what extent did deference to "influentials" blend with identification with groups which retained actual or symbolic leadership by notables? In nineteenth century English politics, Nossiter has observed, influence often "shades into the pressure of group membership."[65] It is a delicate historical problem, surely, to determine where group identity began and where "influence" ended. Studying leadership groups over time, with collective biography and samples of national, regional, and specific community elites, seems one obvious approach to the decline of deference.[66] (The importance of lawyers among the "new class" of politicians is often hinted at, but their role remains vague.) More difficult will be the observation and measurement of "habits of subordination." Works attempting this must remember that the replacement of the traditional by the modern came in stages with mixed forms prevailing for some time.[67]

It will help, then, to see the period from 1795 to the 1830s as intermediate in the decline of deference—and political development —in which the North American states resembled the political culture characteristic of England for several decades after the Reform Bill of 1832. This halfway American phase could borrow a term from this longer English era, namely a deferential-participant political culture.[68] Gradual mobilization and fluctuating turnouts during this phase seem related not only to certain provocative issues, elite conflict, organization, and electioneering, but also to "nonelites' maintenance of respectful and partially acquiescent attitudes toward political authority—a cultural variable."[69]

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[63] Inconsistencies appeared in what is on the whole a stimulating essay: Paul Goodman, "The First American Party System," in Chambers and Burnham, pp. 59, 61, 69, 85, 87.

[64] Tinkcom, p. 233; Dixon Ryan Fox, *The Decline of Aristocracy in the Politics of New York, 1801–1840* (New York: Columbia University Press, 1919); Kass, *Politics in New York,* pp. 16–19; several essays by Pole stress persisting deference, including "Representation and Authority in Virginia from the Revolution to Reform," *Journal of Southern History,* 24 (February, 1958), 16–50. The quotation is from Bette A. Nesvold, "Studies in Political Development," in *Macro-Quantitative Analysis: Conflict, Development, and Democratization,* ed. John V. Gillespie and Nesvold (Beverly Hills, Calif.: Sage Publishers, 1971), pp. 283–84.

[65] T. J. Nossiter, "Aspects of Electoral Behavior in English Constituencies, 1832–1868," in *Mass Politics: Studies in Political Sociology,* ed. Erik Allardt and Stein Rokkan (New York: The Free Press, 1970), p. 172.

[66] For a discussion of techniques: Richard Jensen, "Quantitative Collective Biography: An Application to Metropolitan Elites," in *Quantification in American History,* ed. Swierenga, pp. 389–405; a bibliography is in Dollar and Jensen, *Historian's Guide to Statistics,* pp. 281–83.

[67] For an illuminating discussion of persisting traditional forms of deferential politics mixing with primitive partisan modes, Richard R. Beeman, *The Old Dominion in the New Nation, 1788–1801* (Lexington: University Press of Kentucky, 1972). Beeman's study shows the strength of deferential voting in Virginia in 1800, and maintains that Jefferson's victory ended emergent party politics and brought the return of "nonpartisan, gentlemanly style" habits (pp. 233, 234, 237–38). On the inability of the national Republicans to function as a party from 1800 to 1805, see Richard E. Ellis, *The Jeffersonian Crisis: Courts and Politics in the Young Republic* (New York: Oxford University Press), pp. 3–107.

[68] The phrase "deferential-participant" is borrowed from Gabriel Almond and Sidney Verba, who gave it

Political "Violence" and Ideological Intensity

During this transitional stage some men strained toward party norms, while many others remained powerfully attached to the codes, prerogatives, and sensitivities of men of respectability, honor, and influence. The defensiveness of a political culture in transition, in combination with other causes, helps explain the "violence" or ideological intensity of the years 1795 to 1814. ("Violence" tended to be used in the early republic to mean uncontrolled, excessive, and distorting energy of expression and emotion. The connotation of physical violence is more characteristic of our own time.)

The "violence" of ideological cleavage in the early republic is often attributed in part to the novelty of party conflict. The extremism and paranoia of some Federalists and Republicans has served to support the argument for the presence of party. Ironically, this deduction is often turned on its head by historians discussing the 1830s, some of whom claim that a hallmark of "modern" parties since Jackson has been their relatively nonideological character. The retreat from ideology resulted, it is argued, from a necessity to compromise within parties to build winning coalitions. While this line of reasoning often neglects, in my view, the generalized ideological or belief-system differences between Whigs and Democrats, the earlier Federalist-Republican conflict does seem to have been distinguished by greater ideological intensity—amounting almost to a difference in kind. Many scholars have dealt with this theme of Federalist and Republican passion recently, and most agree that it derived from a conviction that the future of the republic hung on their struggle for power. A *shared* republican mentality among Federalists and Republicans invested their rivalry with hyperbolic drama, and made them believe that the stakes were higher than they were. Roger Brown has also stressed "inexperience with political parties," and Richard Buel has noted that while moderns are not wholly nonideological, neither were early republicans innocent of electoral calculation. But Buel emphasizes, correctly, that ideological tenacity went hand in hand with a rejection of party and compromise.[70] Not only were parties evil, but there was something mean and dishonorable about the behavior necessary for their operation. Therefore, "strong ideological focus"[71] before 1820 is not necessarily a clue to modern party emergence; rather, it often might have signalled the persistence of an older set of men's expectations of themselves and their enemies.

Conclusion

The analysis above of separate strands of party contains a general proposal regarding party origins. During the 1830s partisan behavior emerged in several arenas in fairly rapid sequence: in the national legislature, in state legislatures, among partisan activists, and in electoral publics for all manner of elections. The coalescence of party, a process nearly complete by 1840, can be taken as a model with which to analyze earlier, uncompleted and often unintentional anticipations of party systems. This is not to say that historians have described party institutionalization as fully as might be desired. On the contrary, a systematic, "tripartite" approach has been implicit at

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a slightly different meaning: *The Civic Culture: Political Attitudes and Democracy in Five Nations,* rev. ed. (1963; Boston: Little, Brown, 1965), p. 455. On the durability of deference and influence in England, see H. J. Hanham, *Elections and Electoral Management: Politics in the Time of Disraeli and Gladstone* (London: Longmans, 1959), pp. ix, xi, xiv–xv, 200–03, and *passim;* and James Cornford, "The Adoption of Mass Organization by the British Conservative Party," in *Cleavages, Ideologies and Party Systems: Contributions to Comparative Political Sociology,* ed. Erik Allardt and Yrjö Littunen (Turku: Publs. of the Westermark Society, 1964), pp. 401–11.

[69] Eric A. Nordlinger, "Political Development: Time Sequences and Rates of Change," in *Politics and Society: Studies in Comparative Political Sociology,* ed. Nordlinger (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 346.

[70] Brown, *Republic in Peril,* pp. 182–83; John R. Howe, "Republican Thought and the Political Violence of the 1790's," *American Quarterly,* 19 (Summer, 1967), 147–65; Marshall Smelser, "The Federalist Period as an Age of Passion," *American Quarterly,* 10 (Winter, 1958), 391–419; Robert E. Shallope, "Toward a Republican Synthesis: The Emergence of an Understanding of Republicanism in American Historiography," *William and Mary Quarterly,* 29 (January, 1972), 49–80; Buel, *Securing the Revolution,* p. 91.

[71] "Strong ideological focus" prevails when political conflict concentrates "on a single, stable issue domain which presents an ordered dimension that is perceived in common terms by leaders and followers." Donald E. Stokes, "Spatial Models of Party Competition," in *Elections and the Political Order,* ed. Angus Campbell, Phillip E. Converse, Warren E. Miller, and Donald E. Stokes (New York: Wiley, 1966), p. 176.

best in Jacksonian studies, scattered throughout monographs dealing with different elements of party. Indeed, it may be unrealistic to ask one scholar to deal comprehensively with all levels of party activity as they developed, even in a single state; but it may be expected that researchers will demarcate explicitly the arenas they intend to study, and specify the evidence needed to demonstrate party in that sector. Similarly, there are untapped possibilities for studying Jacksonian parties as organizations proper, in office, and in the electorate. To take but one example: accounts of party formation after 1828 contain perfunctory references to the importance of patronage, but hardly a glimpse of the enormous role played by cadre-men appointed to land offices, post offices, customs houses, and the like, in the building of organizations, and possibly in forming local electoral cleavages.[72]

Subsequent studies may modify these conclusions, yet the available evidence (read with some skepticism) prompts the hypothesis that early republican politics were rooted far more deeply in preparty political culture than is conventionally allowed. Before 1834, "peaks" of elite conflict in national and state affairs generated some movement toward party action. In Congress and in many states intense policy cleavages brought forth incipient party-like behavior, especially during such war crisis years as 1797 to 1800 and 1808 to 1814. But Walter Dean Burnham correctly characterized the era as "a pre-party phase in American political development," and J. R. Pole emphasized the "incompleteness of party organization" and warned that despite high participation it would be a mistake "to see the parties of this formative period in a modern light, outlined by modern definitions." The Federalist and Republican "interests"—a very accurate contemporary word—rather remained much closer to "relatively stable coalitions" than to durable cadre parties of regular internal organization and fairly stable, self-conscious mass followings.[73] The parties of the *deferential-participant* phase only approached an institutional threshold.

The partisan blocs which formed temporarily in national and state legislatures hesitated halfway between stable coalitions and party, and were ambivalently tolerated in the political culture. These temporary blocs should not be seen as exercising complete sway even over their own assembly halls. Most of the time they were smaller rings within larger circles. The universal empire of party was to come only after 1835.

Ideally, histories of party formation should recognize not only that the three elements of party eventually became related to each other, but also that they interacted with broad trends in polity and society. Some men built parties by conscious choice; others helped create them unintentionally; still others deliberately resited the new way. None acted, however, entirely under conditions of their own choosing. Leaders of nascent parties, for example, occasionally rewrote the rules of the game (electoral, legislative, administrative) to facilitate their gaining, wielding, or maintaining power. But studies of these institutional manipulations will not be very informative if they ignore the wider social, economic, and communications changes to which elites (and electorates) responded.[74] Movements in various political theaters, split apart in researchers' division of labor, were often not only simultaneous but also intertwined responses to pervasive social change.[75]

The relationship between social change and party formation, it is assumed here, can be studied fruitfully as an aspect of political culture. So far in this essay, "political culture" has served as one of those "catchwords" described by Petter Nettl, that is, "deliberately vague conditioning concepts that follow at the end of a string of precise argument." Catchwords usually indicate the territory of another discipline, said Nettl, and permit evasion by suggesting that for the explanation to proceed further one must enter foreign territory.[76] It is time to close the escape hatch: "Political culture is, simply, the political aspect of the culture of a society . . . a historical system of widespread, fundamental, behavioral, political values . . . classified into subsystems of identity, symbol, rule and belief."[77] Most definitions stress the psychological components (especially citizen attitudes to authority). Thus Harry Eckstein defines political culture as consisting of

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[72] Land office patronage and party in the Northwestern and Southern states are treated briefly in Malcolm J. Rohrbough, *The Land Office Business: The Settlement and Administration of American Public Lands, 1789-1861* (New York: Oxford University Press, 1968), pp. 271–76.

[73] Burnham, "Party Systems and the Political Process," in Chambers and Burnham, *American Party Systems*, p. 289; Pole, "Constitutional Reform in Maryland, p. 281. "Relatively stable coalitions" as pre-party phenomena are discussed in Richard A. Pride, *Origins of Democracy: A Cross-National Study of Mobilization, Party Systems, and Democratic Stability* (Beverly Hills, Calif.: Sage Publishers, 1970), pp. 692–94.

[74] Burnham's discussion of realignments is relevant here (*Critical Elections*, pp. 1–10, 13, 27–28).

[75] It is easier to assert such patterns than to show them. Political scientists typically explain increased voter participation by inferences which are in effect deductive theories of mass mobilization. Thus: economic development causes changes in society, which lead to new experiences for more citizens, and hence to the growth of certain political attitudes, and thus to more participation. This hypothesis is adapted from Norman H. Nie, G. Bingham Powell, and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships, Part I," *American Political Science Review*, 63 (June, 1969), 372. Though they frequently make similar claims, historians have yet to demonstrate any of this.

> politically relevant values (purposive desires), cognitions (conceptions of the nature of reality), and expressive symbols, from language to visual ceremony. It refers in particular to the 'internalized expectations' in terms of which the political roles of individuals are defined and through which political institutions (in the sense of regularized behavior patterns) come into being.[78]

Patterns of voter turnout, to illustrate, may be reflective of political culture. When from 1800 to 1840 voters shifted their attentiveness from state to national elections, turnout rates became indicators of changed cognitions and values. Aside from being attracted to vote in presidential elections, average citizens also may have found "politics in general" to be more interesting.[79] This may be one of the least studied important themes in political history. Surely citizens came to political consciousness at very different rates given the size, heterogeneity, and uneven economic development of society. In any event, voter turnouts thus reflected changes in "internalized expectations" generally entertained about political institutions. They recorded movements in men's minds as well as tidal social change. The arrival of party, whatever the final verdicts on timing, also signaled profound shifts in political culture.

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[76] J. P. Nettl, *Political Mobilization: A Sociological Analysis of Methods and Concepts* (New York: Basic Books, 1967), pp. 42–43. Nettl's description of the necessary role of catchwords in social science, and of political culture as "the main catchment area of modern politics" is must reading, pp. 43–53. See also Lucian W. Pye, "Culture and Political Science: Problems in the Evaluation of the Concept of Political Culture," *Social Science Quarterly*, 53 (September, 1972), 285–96.

[77] Donald J. Devine, *The Political Culture of the United States: The Influence of Member Values on Regime Maintenance* (Boston: Little, Brown, 1972), pp. 14–18.

[78] Harry Eckstein, "A Perspective on Comparative Politics, Past and Present," in *Comparative Politics: A Reader*, ed. Eckstein and David Apter (New York: Free Press, 1963), p. 26.

[79] M. Kent Jennings and Harmon Zeigler, "The Salience of American State Politics," *American Political Science Review*, 64 (June, 1970), 523–35; Moshe M. Czudnowski, "A Salience Dimension for the Study of Political Culture," *American Political Science Review*, 62 (September, 1968), 878–88.

Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture*

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During the 1950s and 1960s, a central theme in the historiography of the progressive movement in the United States was the middle-class character of progressivism. Stimulated by George E. Mowry's *The California Progressives* (1951), several biographical studies of national and state progressive leaders appeared, all of which confirmed Mowry's findings of the middle-class nature of progressive leadership and seconded his conclusion that progressivism was an "upper-middle-class movement."[1]

The greatest stimulus to the study of progressive leaders, however, came with the appearance of Richard Hofstadter's *The Age of Reform* in 1955, in which he developed a "status revolution" thesis regarding the motivation of the middle-class progressives. Hofstadter contended that a loss of status by displaced professionals and business men of the "Mugwump type" impelled them to turn toward reform of the political and economic system which had brought about the upheaval in status.[2] Mowry's and Hofstadter's views were challenged on several levels during the 1960s.[3] The most effective challenge to Mowry's and Hofstadter's contentions came from several studies which demonstrated that political leaders other than progressives—notably Old Guard Republicans and Democrats—shared the same social and economic characteristics: they were just as middle-class as progressive Republican leaders and the leaders of the Progressive party.[4] The sum total of all these studies was to prove something that historians probably should have known all along, namely that political leadership in the United States is largely middle-class. Consequently, the middle-class character of progressive leadership loses the significance Mowry, Hofstadter, and others have attached to it.

But any movement, political or otherwise, has *both* leaders and followers. Despite the preoccupation with progressivism as a middle-class movement, none of the major studies mentioned above examined the electoral basis of support for either progressive or conservative Republicans in any scientific or comprehensive manner.[5] Clearly, progressive leaders came from the middle and upper classes. But did middle- and upper-class voters support them at the polls? Who did provide the bulk of votes for progressive candidates? These important questions have yet to be answered in any significant manner.

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[1] George E. Mowry, *The California Progressives* (Berkeley: University of California Press, 1951), chap. 4, particularly 87–89; Alfred D. Chandler, Jr., "The Origins of Progressive Leadership," in Elting E. Morison, ed., *The Letters of Theodore Roosevelt* (8 vols., Cambridge: Harvard University Press, 1954), VIII, 1062–1065. Mowry later extended his research to include more than 400 progressive leaders, arriving at the same conclusions. Mowry, *The Era of Theodore Roosevelt, 1900–1912* (New York: Harper, 1958), chap. 5.

[2] Hofstadter's argument is too well-known to require any more extensive elaboration here; see *The Age of Reform* (New York: Knopf, 1955), chap. 4. Soon afterward, "status displacement" theories were applied with somewhat reckless abandon to several aspects of American history. A good summary of this development is Gerald W. McFarland, "Inside Reform: Status and Other Evil Motives," *Soundings,* 54 (Summer 1971), 164–165.

[3] *The Age of Reform* was initially assailed as leaning towards a psychological interpretation of history. See, for example, Norman Pollack, "Hofstadter on Populism: A Critique of 'The Age of Reform,'" *Journal of Southern History,* 26 (November 1960), 478–500, and William B. Hesseltine's review of the book in *Wisconsin Magazine of History,* 39 (Summer 1956), 280. The best criticism along this line is David P. Thelen, "Social Tensions and the Origins of Progressivism," *Journal of American History,* 66 (September 1969), 323–341, which uses recent findings in the behavioral sciences to refute Hofstadter.

Other scholars have emphasized the coalitional nature of progressivism, especially the contribution made by non-middle-class groups. See, for example, J. Joseph Huthmacher, "Urban Liberalism and the Age of Reform," *Mississippi Valley Historical Review,* 49 (September 1962), 31–41; and John D. Buenker, "Urban, New-Stock Liberalism and Progressive Reform in New Jersey," *New Jersey History,* 87 (Summer 1969), 79–104, and "The Progressive Era: A Search for a Synthesis," *Mid-America,* 51 (July 1969), 175–193.

[4] The best of these is Richard B. Sherman, "The Status Revolution and Massachusetts Progressive Leadership," *Political Science Quarterly,* 78 (March 1963), 61–65. See also William T. Kerr, Jr., "The Progressives of Washington, 1910–1912," *Pacific Northwest Quarterly,* 55 (January 1964), 16–27; Jack Tager, "Progressives, Conservatives and the Theory of the Status Revolution," *Mid-America,* 48 (July 1966), 162–175; and Eli Daniel Potts, "A Comparative Study of the Leadership of Republican Factions in Iowa, 1900–1914" (Master's thesis, State University of Iowa, 1954), part of which was published as "The Progressive Profile in Iowa," *Mid-America,* 47 (October 1965), 257–268.

Only a few studies have appeared which attempt to examine the electoral basis of progressivism, and they all deal only with California. Michael P. Rogin and John L. Shover find that a significant shift in support for the California progressives took place in 1914. When Hiram Johnson and the progressives swept to victory in 1910, middle-class voters indeed formed the bulwark of progressive votes. But after the passage of significant labor legislation between 1911 and 1913, middle-class voters deserted Johnson in increasing numbers, and the backbone of progressive voting strength shifted to working-class and immigrant voters, who had largely rejected Johnson in 1910. Organized labor and foreign-stock voters remained the strongest supporters of Johnson and other progressives well into the 1920s.[6] In California, then, the electoral basis of progressivism approximated the Mowry-Hofstadter model only in 1910; subsequent elections revealed a considerably different cleavage in the electorate.

A careful and intensive analysis of voting behavior in several cities in another key progressive state—Wisconsin—yields a more consistent pattern of behavior among middle-class voters. In that bastion of progressivism—the home of Robert M. La Follette and the recognized leader among all American states in the passage of advanced progressive legislation—the middle and upper classes did not lend enthusiastic support for progressive candidates at *any* time, in either general or primary elections. To the contrary, they provided the bulwark of support in urban areas for stalwart Republicans, the most adamant opponents of progressivism. There is no doubt that in terms of its voting strength, Wisconsin progressivism was *not a* middle-class or an upper middle-class movement.

Wisconsin Progressivism

Wisconsin's reputation as the leading progressive state makes it a natural choice for a study of the electoral basis of progressivism. Progressivism in Wisconsin is usually equated with Robert M. La Follette, the dynamic leader who is often credited with almost single-handedly making the Badger State the "laboratory of democracy." La Follette was indeed the dominant figure in Wisconsin politics in the Progressive Era. After he was elected governor as a regular Republican in 1900, his advocacy of election and taxation reform split the Republican party into progressive and stalwart factions, and the resultant warfare between the two groups dominated the state's politics for two decades. During his first two conflict-laden terms as governor between 1901 and 1904, La Follette himself was the main issue and point of division between the two factions. Even after his departure for the Senate in 1906, La Follette continued to exert a strong influence on the state's politics.

The progressive movement in Wisconsin did not completely revolve around La Follette, however; much of the state's pioneering progressive legislation was passed under the administrations of his successors, James O. Davidson and Francis E. McGovern. Davidson, who became governor when La Follette resigned to go to the Senate, broke with La Follette when La Follette tried unsuccessfully to block his nomination to a full term in 1906. Although Davidson continued to push for progressive legislation, he and other moderate progressives sought to heal the breach between progressive and stalwart Republicans, and his efforts secured a temporary restoration of relative peace within the party. The pinnacle of Wisconsin progressivism came during McGovern's first term in 1911, when the legislature enacted a sweeping and far-reaching legislative program that included a state corporate and personal income tax, workmen's compensation legislation, and the creation of additional regulatory commissions. But a sharp increase in expenditures and taxes occasioned by this legislation, combined with a bitter break between McGovern and La Follette, seriously weakened the progressive Republican voting coalition that had kept the progressives in power since 1900. In 1914, stalwart Emanuel L. Philipp, capitalizing on discontent over extravagance and high taxes, won the Republican gubernatorial primary, and his subsequent election brought an effective end to the Progressive Era in Wisconsin.[7]

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[5] Kerr included a brief analysis of the type of electoral districts which sent either progressives or stalwarts to the Washington legislature. "Progressives of Washington," pp. 22ff.

[6] Alexander Saxton, "San Francisco Labor and the Populist and Progressive Insurgencies," *Pacific Historical Review,* 34 (November 1965), 421–438; Michael P. Rogin, "Progressivism and the California Electorate," in Rogin and John L. Shover, *Political Change in California: Critical Elections and Social Movements, 1890–1966* (Westport, Conn.: Greenwood Publishing Co., 1970), pp. 35–61; Shover, "The Progressives and the Working-Class Vote in California," in Rogin and Shover, *Political Change in California,* pp. 62–89.

Like most other midwestern states, Wisconsin was solidly Republican throughout the Progressive Era. Securing the Republican nomination for a statewide office virtually guaranteed election. Consequently, the real contest for power took place within the GOP, between the stalwart and progressive factions. Although the Democratic party in Wisconsin had its share of progressive-minded leaders, its impotence and its general control by conservative and reactionary elements dictate that this study confine itself to analyzing the basis of support for progressive Republicans. Thus the use of the term "the progressives" in this study refers only to Republicans.

Purpose, Scope, and Method

The major purpose of this paper is to examine middle-class and upper middle-class voting behavior in order to test the hypothesis that progressivism was not a middle-class movement, using Wisconsin as the unit of analysis. Secondarily, it aims to investigate the relative strength of class considerations versus ethnic and religious factors as primary determinants of voting behavior in Wisconsin cities.

Six Wisconsin cities were selected for detailed analysis between 1900 and 1914: Milwaukee, Madison, La Crosse, Oshkosh, Sheboygan, and Kenosha. Although none of them was "typical," together they provide a good geographical, ethnic, and economic cross-section of Wisconsin. Criteria for selection were (1) Size: Except Milwaukee, each had a population of 20,000 to 35,000 in 1910; (2) Ethnic composition: except for solidly German Sheboygan, each had considerable ethnic heterogeneity; (3) Stability of ward boundaries; (4) Diverse political behavior: Kenosha usually divided evenly between the two major parties, and Sheboygan, Madison, La Crosse, and Oshkosh were normally Republican, although in varying degrees. In addition, Milwaukee and Oshkosh had significant Social-Democratic votes, La Crosse and Madison tended to favor progressive candidates, and Sheboygan and Oshkosh had sizable stalwart Republican elements.

Another advantage to using Wisconsin to analyze progressive voting support is the wealth of data contained in the Wisconsin state census of 1905.[8] The census lists the distribution by ward for 37 specific occupational categories, which have been rearranged into broader, more viable combinations for analysis. "Middle class" includes some professionals (clergymen, journalists, teachers), semi-professionals (policemen, nurses, etc.), and most white-collar functionaries (bookkeepers, clerks, etc.); "upper class" includes merchants, bankers, manufacturers and other proprietors, physicians, dentists, and lawyers; "upper plus middle class" is the sum of those two categories; "skilled workers" includes skilled tradesmen plus all foremen; "total workers" is the sum of the "skilled workers" category and all other classes of laborers.[9]

Before proceeding with the analysis, however, it is necessary to discuss two crucial problems which determined the procedure and methodology employed. The first involves the distribution of middle- and upper-class voters in the six cities. With the slight exception of Milwaukee, middle-class and upper-class residents lived in the same wards. Thus the wards in each city that were the most upper-class were also the most middle-class. Furthermore, in no ward did upper-class residents outnumber middle-class ones; in most wards, the ratio of middle-class to upper-class residents was two or three to one. No ward in any of the six cities, then, could be labeled "upper class." In addition, few wards could be definitely labeled "middle class:" only in La Crosse's Ward 17 did middle-class residents constitute a majority, and in few wards did they exceed 35 per cent.[10] Consequently, the term "middle-class wards" must be used only in a relative sense, namely to designate those wards housing the largest percentages of middle-class residents.

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[7] The best account of Wisconsin politics during this era is Herbert F. Margulies, *The Decline of the Progressive Movement in Wisconsin, 1890–1920* (Madison: State Historical Society of Wisconsin Press, 1968). See also Robert S. Maxwell, *La Follette and the Rise of the Progressives in Wisconsin* (Madison: State Historical Society of Wisconsin Press, 1956).

[8] The superiority of the Wisconsin data helps to avoid some of the deficiencies of the California studies. For example, the only ethnic variable used by either Rogin or Shover is "per cent foreign born." Since this is not broken down into smaller ethnic group components which might not vote alike, it tends to be largely meaningless: in most states, there *never* was any unified "foreign vote" because of basic religious and other cultural antagonisms among immigrant groups. The data for Wisconsin, however, permits the use of individual ethnic variables at the minor civil division level; it also permits the division of "working class" into skilled and unskilled categories.

Another problem with the work of Rogin and Shover is that they deal statistically with only one candidate in each election, usually Johnson. Thus votes for Johnson are equated with "progressive" votes; there is no comparison with other candidates running on the same ticket to serve as a control for Johnson's personal appeal or lack of it. As shown below, a similar analysis for Wisconsin using only the vote for La Follette would produce distorted results.

[9] *Wisconsin State Census, 1905* (Madison, 1906), I, 276–601.

Since middle-class and upper-class voters lived in the same wards, it is unnecessary (except occasionally in Milwaukee) to use "middle class" and "upper class" as separate variables. Instead, "upper class plus middle class" is used as the chief variable in the tables that follow. Combining these two categories results in a wider range of values and greater statistical reliability. As a check, statistical analysis that used "upper class" and "middle class" singly was also performed; these results, however, are reported only when they differ from results using the combined variable.[11]

The second major problem in analyzing the voting behavior of the middle and upper classes is the close connection between social class and ethnic identity. In the six cities, middle- and upper-class wards were populated largely by persons of old American or English stock.

Recently published studies have established the all-pervasive influence of cultural forces—especially ethnicity and religion—as the primary determinants of midwestern voting behavior in the late nineteenth century.[12] In Wisconsin, the primacy of ethnic and religious factors in voting behavior continued into the Progressive Era.[13] In the early twentieth century, these ethnocultural forces often acted in conjunction with class considerations in determining party identification and voter choice.[14] For example, Republican voters in urban areas were often *both* middle- or upper-class *and* of Anglo-American stock;[15] conversely, many Democrats were both working-class and from predominantly Catholic ethnic groups. This phenomenon poses serious problems. Did, for example, "Yankee" elements vote Republican because of their relative wealth or because of considerations conditioned by previous cultural strife, such as prejudice against foreigners and Catholics, both of whom they associated with Democrats? Did unskilled and poor Polish factory workers vote Democratic because they were Polish and Catholic or because they perceived the Democrats as offering something to labor?

Ethnocultural and class factors in voting behavior in "normal" elections

Because of the close relationship between class and ethnicity, it is imperative to determine the *relative* importance of class considerations as opposed to ethnocultural factors in terms of their impact upon middle- and upper-class voting behavior in a "normal" situation. It is meaningless to examine how the middle and upper classes reacted to progressive candidates in any given election without knowing how they usually voted in general elections. Consequently, the analysis proceeds in two stages. First, I have attempted to ascertain the relative strength of cultural and class factors upon voting behavior in two "normal" elections eight years apart. Second, I have examined the voting behavior of the upper and middle classes in both general and primary elections involving progressive candidates and issues.

Although every election has unique aspects and cannot strictly be said to be "normal," certain basic patterns in voting behavior—such as the relative percentage of total vote of a given party, or ethnic, religious, or occupational group patterns—often remain constant,

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[10] Middle-class percentages ranged from 6.2 in Oshkosh's Ward 6 to 55.8 in La Crosse's Ward 17. The average percentages of middle-class residents for Milwaukee and La Crosse were 23.8 and 24.4, respectively.

[11] The greater range of the upper class plus middle class variable (from 7.6 per cent in Oshkosh, Ward 6 to 60.4 per cent in La Crosse, Ward 17) plus its higher mean (28.9 per cent in Milwaukee and 29.6 per cent in La Crosse) give it greater statistical reliability than either upper class or middle class alone. Only four wards—two in La Crosse and one each in Milwaukee and Madison—had upper- plus middle-class populations in excess of 50 per cent.

[12] Paul J. Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900* (New York: The Free Press, 1970); Richard Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888–1896* (Chicago: University of Chicago Press, 1971). See also Robert P. Swierenga, "Ethnocultural Political Analysis: A New Approach to American Ethnic Studies," *Journal of American Studies,* 5 (April 1971), 66–79.

[13] Roger E. Wyman, "Voting Behavior in the Progressive Era: Wisconsin as a Case Study" (Ph. D. dissertation, University of Wisconsin, 1970), chaps. 8–15.

[14] The term "ethnocultural factors" here refers to the role of *both* ethnicity and religion, even though only ethnic variables are used in the analysis. During this era, religion was at least as important as, if not more important than, ethnicity as a determinant of party identification and voting behavior. Unfortunately, the lack of any published religious data at the minor civil division level makes it impossible to use religion as a quantifiable variable. With the exception of the Germans and a few other groups who were divided internally along religious lines, ethnicity and religion tended to be indivisible. Two good examples are the Poles and the Scandinavians. For a general discussion of the role of religion in Wisconsin politics during the Progressive Era, see Wyman, "Voting Behavior," chaps. 8, 9.

[15] The variable "percentage Anglo-American" used in this study is composed of native-born of native parents plus British stock (English plus Scots plus Welsh).

Table 1. Republican Vote for Governor, 1900 and 1908, By Wards with Particular Ethnic and Occupational Characteristics

City and characteristic of ward	No. of wards, 1908[a]	% Repub., 1900	% Repub., 1908
Milwaukee, city	23	50.5	33.6
Upper+Middle Class	8	54.2	40.2
Middle Class	5	53.7	39.1
Working Class	12	47.9	30.3
German	11	52.8	33.2
Republican Ethnic Groups	4	61.7	42.3
Democratic Ethnic Groups	9	48.4	30.2
La Crosse, city	21	53.9	50.7
Upper+Middle Class	7	61.4	54.8
Working Class	11	51.7	42.8
German	7	39.6	31.7
Anglo-American	4	67.4	57.6
Republican Ethnic Groups	9	67.5	60.0
Democratic Ethnic Groups	8	42.5	33.0
Oshkosh, city	13	49.8	49.2
Upper+Middle Class	3	61.1	45.1
Working Class	4	42.4	42.6
German	5	44.1	47.7
Republican Ethnic Groups	3	68.2	63.5
Sheboygan, city	8	46.6	46.9
Upper+Middle Class/Republican Ethnic Groups	2	56.0	53.7
Working Class	3	42.8	45.4
German	3	36.6	38.7
Kenosha, city	8	50.4	46.2
Upper+Middle Class	2	56.2	51.5
Working Class	4	42.0	42.8
German	3	37.8	36.6
Republican Ethnic Groups	3	60.5	53.7

[a] In 1900, Milwaukee had 21 wards and La Crosse 20. Madison is omitted from the table because of more significant ward changes between 1900 and 1908.

unless disturbed by short- or long-term forces. Elections in which disruptive short-term forces are minimal can thus be termed "normal." The vote for governor in 1900 and 1908 was chosen to represent "normal" elections. In 1900, La Follette did not run as a "progressive" or even on a reform ticket. He was the regular Republican candidate, he campaigned largely on national issues, and his vote in all parts of the state was virtually identical to that of presidential nominee William McKinley. In 1908 the genial Davidson, largely because of his efforts to restore harmony in the party while continuing moderate reforms, won united support from both stalwart and progressive Republicans in his re-election bid. In both 1900 and 1908, the divisive issues which so often split the Republican party between 1901 and 1914 were absent. Consequently, both La Follette in 1900 and Davidson in 1908 received full GOP support, as evidenced by the closeness of the Republican vote cast for both president and governor in each election.[16]

Table 1 demonstrates that voting patterns in 1900 and 1908 clearly divided along both ethnic and class lines.[17] In each city, Republi-

[16] In 1900, McKinley polled 60.06 per cent of the total vote to 59.83 per cent for La Follette, a minuscule difference of .23 per cent. In 1908, Taft won 54.01 per cent to 54.74 per cent for Davidson, a difference of .73 per cent. In striking contrast, La Follette ran 12.7 per cent behind Roosevelt in 1904.

For a discussion of Wisconsin election campaigns, see Margulies, *Progressive Movement in Wisconsin*, chaps. 2, 3; and Wyman, "Voting Behavior," chap. 7. On La Follette in 1900, see Robert C. Twombly, "The Reformer as Politician: Robert M. La Follette in the Election of 1900" (Master's thesis, University of Wisconsin, 1964), pp. 58, 61–100.

[17] In Table 1 and other tables, some ethnic and occupational variables (usually "middle class" and either "German" or "Democratic ethnic groups") do not appear for some cities. As explained earlier, there is no

can percentages of the total vote were consistently and decisively higher in upper- and middle-class wards and in wards populated by "Republican" ethnic groups (native-born of native parents, English, Welsh, Scots, Scandinavians, and Protestant Dutch) than they were in working-class wards and in wards with the highest percentages of Germans, Poles, Austrians, Russians, Irish, Italians, Bohemians, and Hungarians, the groups which make up the "Democratic" ethnic group variable.[18]

One section of the table warrants further explanation. The extremely low Republican percentages in all types of wards in Milwaukee in 1908 resulted from the three-party situation that had existed there since 1904. The presence of a strong Social-Democratic party made Milwaukee's voting patterns much more complex. Polling an increasingly large share of the city's vote in suceeding elections after 1900, the Social-Democrats were strong enough to capture most city and county offices in 1910.[19] As a result, political divisions in Milwaukee paralleled class lines more closely than in the other cities. Nevertheless, ethnicity remained important despite the increased class polarization. While the Social-Democratic party had a solid trade union foundation and was built upon class consciousness and class antagonisms, it also had a special ethnic appeal. Its most rapid growth was in wards that were both heavily German and had large working-class populations. For example, the only two wards which voted Social-Democratic in every election between 1904 and 1910 were those which ranked first and second in German population (each more than 85 per cent); ranked fourth and fifth in percentage of total workers; and ranked second and fifth, respectively, in skilled workers.[20]

The close connection between class and ethnocultural factors in voting behavior was not peculiar to 1900 and 1908 but was constant throughout the Progressive Era. Extensive ward-by-ward analysis of elections between 1900 and 1914 in all six cities reveals that Republican candidates consistently drew strong support from upper- and middle-class voters and from the "Republican" ethnic groups. Conversely, working-class voters and the groups composing the "Democratic" ethnic variable persistently gave lopsided support to Democratic candidates.

Clearly, both ethnicity and class were strongly associated with partisan voting patterns during this period. But since class and ethnicity were themselves closely related,[21] the question is: Which factor was the more important? Or were they fairly equal in their effect?

The statistical techniques of correlation and multiple regression analysis can help answer these questions. By applying these techniques to the data, we can (1) separate the relative impact of either of the two independent variables (ethnicity and class) upon any dependent variable (Republican vote in 1900, for example), and (2) measure the cumulative effect of both independent variables on the dependent variable.[22]

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need to use both "middle class" and "upper plus middle class" as distinct variables. Similarly, in a few cities per cent German and per cent Democratic ethnic groups are virtually identical in their distribution, and one variable is omitted to prevent needless duplication. In Sheboygan, for example, the two upper- plus middle-class wards and the Republican ethnic group wards were the same.

[18] The source for all ward level ethnic data is "Cultural-Ethnic Backgrounds in Wisconsin" (11-volume typewritten manuscript in possession of Rural Sociology Department, University of Wisconsin), a retabulation of the manuscript returns for the *Wisconsin State Census, 1905*. All ethnic percentages are the percentages of total family heads (the base) who are of foreign stock (foreign-born plus native-born of foreign parents). The "Republican" and "Democratic" ethnic variables were produced by adding the percentages of total family heads for each of the groups mentioned above who fell into either category. All election data comes either from the *Wisconsin Blue Book* for the year following the election or from the reports of the county boards of canvassers, in the Archives and Manuscripts Division, State Historical Society of Wisconsin. For a more comprehensive explanation of the methodology involved, see the methodological appendix in Wyman, "Voting Behavior."

[19] The Social-Democrats grew at the expense of both the Republican and Democratic parties. For a comprehensive account of Milwaukee politics in this era, see Bayrd Still, *Milwaukee: The History of a City* (Madison: State Historical Society of Wisconsin Press, 1948), chaps. 11, 12. On the Socialists, see Marvin Wachman, *History of the Social-Democratic Party of Milwaukee, 1897–1910* (Urbana: University of Illinois Press, 1945).

[20] Although the Social-Democrats were strongest among German skilled workers, religious factors were also present. Few Catholic Germans voted Socialist, and most German Socialists came from the freethinker and Protestant elements. In a recrudescence of anti-Catholicism in 1914 and thereafter, Social-Democratic candidates invariably won the approval from militant anti-Catholic organizations.

[21] In Milwaukee, per cent upper plus middle class correlated .498 with per cent Anglo-American and .411 with the Republican ethnic variable. In La Crosse, the correlation coefficients were .507 and .383, respectively.

[22] For each dependent variable, the standard regression equation [Y (vote) $= a + b_1x_1$ (ethnicity) $+ b_2x_2$ (class)] was calculated, using the stepwise method. For a comprehensive explanation of both correlation and regression, see Hubert M. Blalock, *Social Statistics* (New York: McGraw-Hill, 1960), chaps. 17–19, and Charles M. Dollar and Richard J. Jensen, *Historian's*

Table 2. Summary of Regression Analysis, Ethnic and Occupational Data with Republican Vote, 1900 and 1908

Dependent Variable	Independent Variable	Simple r	Beta	Signif. level of b $P=<$	Signif. level of equation $P=<$	R^2
		La Crosse				
Rep. Gov., 1900	Repub. Ethnic Groups	.881	.869	.0001	.0001	.777
	Upper+Middle Class	.364	.031	n.s.		
	Democ. Ethnic Groups	−.878	−.882	.0001	.0001	.770
	Working Class	−.202	.017	n.s.		
Rep. Gov., 1908	Repub. Ethnic Groups	.846	.751	.0001	.0001	.769
	Upper+Middle Class	.536	.248	.05		
	Democ. Ethnic Groups	−.834	−.789	.0001	.0001	.727
	Working Class	−.378	−.182	n.s.		
		Milwaukee				
Rep. Gov., 1900	Repub. Ethnic Groups	.632	.602	.01	.05	.403
	Upper+Middle Class	.319	.071	n.s.		
	Democ. Ethnic Groups	−.618	−.584	.025	.025	.384
	Working Class	−.417	−.056	n.s.		
Rep. Gov., 1908	Repub. Ethnic Groups	.714	.510	.001	.0001	.715
	Upper+Middle Class	.706	.497	.001		
	Working Class	−.852	−.651	.0001	.0001	.790
	Democ. Ethnic Groups	−.727	−.324	.01		

n.s. = not significant at the .05 level.

Table 2 summarizes the results of the statistical analysis for both Milwaukee and La Crosse in 1900 and 1908.[23] The *simple r* is the correlation coefficient between the vote and each independent variable; the *beta* value can be interpreted as a measure of the relative strength of each independent variable upon the vote;[24] the *significance level of b* states the statistical significance of each independent variable; the *significance level of the equation* states the statistical significance of the entire regression equation;[25] and the R^2 gives the percentage of variance in the vote that can be explained by the combined effect of class and ethnicity.

The data presented in Table 2 demonstrate that ethnocultural factors were relatively more important than class factors as determinants of voting behavior in 1900 and, to a lesser extent,

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Guide to Statistics: Quantitative Analysis and Historical Research (New York: Holt, Rinehart, and Winston, 1971), chap. 3.

[23] Milwaukee and La Crosse are the only two of the six cities with enough wards to make statistical analysis reliable. The normal N in all tables involving correlation coefficients is 23 for Milwaukee (19 in 1900) and 21 for La Crosse (20 in 1900). In all calculations, one or a few cases in some variables were removed after scattergram analysis revealed that they might be exerting undue influence upon the correlations. The most serious of these was the removal of three wards from the middle-class and upper- plus middle-class variables in Milwaukee—those wards with the highest percentages; thus all calculations involving those variables have an N of 20 rather than 23. This was done to increase the reliability of the analysis, despite the fact that removal of the three wards reduced the correlations (e. g., the correlation between GOP vote, 1908, and upper plus middle class is .842 with an N of 23 compared to .706 with an N of 20). For an explanation of the effect of extreme cases and the rationale for their removal, see Blalock, *Social Statistics*, pp. 290–292.

[24] The beta is the standardized regression coefficient. The author feels that in the case at hand it is superior to other measures as an indicator of the relative strength of each independent variable. See Blalock, *Social Statistics*, pp. 344–346; and Dollar and Jensen, *Historian's Guide to Statistics*, pp. 88–89. Other results, such as the *b*'s, standard error of *b*, and partial *r*'s, were eliminated for the sake of clarity and simplicity in the tables.

[25] Significance levels state the probability that results obtained could be the product of random variation. A *P* level of .05 means that there is a five per cent chance that the result could be accidental, provided there was no true relationship; a level of .001 reduces that chance to one in a thousand.

in 1908. The superiority of ethnicity in determining voter choice in 1900 is especially clear. An examination of the beta coefficients for both cities reveals that ethnicity was far more important than the upper plus middle class variable in influencing Republican vote; furthermore, only the ethnic variables are statistically significant. In La Crosse, ethnicity also remained more important than class in 1908. It is important to note, however, that the relationship between upper plus middle class and Republican vote had also become statistically significant, although at a lower level.

The data for Milwaukee present a different picture. In 1908, class factors had become virtually equal to ethnic considerations in influencing Republican voters. Actually, class factors seemed to be even more important than ethnic forces in influencing working-class voters to oppose the GOP.

The above statistical analysis plus a ward-by-ward analysis of all six cities indicates that, in general, class factors played an important role in voting behavior during the Progressive Era. Their importance varied from city to city and among different ethnic groups. Although class factors and ethnocultural forces generally acted in concert, they occasionally operated at variance with each other, as in the case of middle-class German support for the Social-Democrats. Statistical analysis of voting patterns in Milwaukee and La Crosse, however, indicate that class considerations were generally, but not always, secondary to ethnic and religious forces. Specifically, middle- and upper-class voters, who tended to be largely of native and English stock, tended to vote strongly Republican in "normal" general elections. But few elections in Wisconsin during the Progressive Era could be classified as "normal." The factional and ideological issues that became so important in the progressive-stalwart conflict within the Republican party upset the usual voting patterns of middle- and upper-class voters, and to a lesser extent, the working classes as well.

Anti-progressive voting in 1904

The election of 1904 was the most crucial and most dramatic episode of the Progressive Era in Wisconsin. The bitter political conflict generated by La Follette's tenure as governor exploded into full-scale internecine warfare among Wisconsin Republicans. As La Follette's second term neared its end, both the stalwart and La Follette factions looked to 1904 as the showdown for control of the party. Failing to block La Follette's renomination after a year-long campaign, the stalwarts bolted the La Follette-controlled convention, held their own convention and nominated a rival National Republican ticket. After the Wisconsin Supreme Court ruled that the La Follette slate was entitled to the official Republican label on the ballot, the stalwarts abandoned their own ticket and supported the conservative Democratic nominee, a tactic they had used unsuccessfully in 1902.[26]

La Follette's re-election in 1904 and the success of the rest of the progressive-dominated state Republican ticket, coupled with the elimination of many prominent stalwarts in the legislature, guaranteed the completion of La Follette's legislative program and dealt a crushing blow to the stalwarts, who were unable to mount a counterattack of any strength until 1910. The election returns, however, revealed that La Follette's triumph was a qualified one. While Theodore Roosevelt swept the state by landslide margins, polling 63.2 per cent of the vote, La Follette ran more than 53,000 votes behind the President, winning by a slight majority of 50.5 per cent; La Follette also ran some 20,000 votes behind the rest of the Republican state ticket, which garnered a median of 55.6 per cent of the statewide vote.

Nationally, the Republican party was not afflicted in 1904 with the bitter progressive-stalwart schism that eventually ruptured it in 1912. Since there was no identifiable progressive faction in the party in 1904, Theodore Roosevelt, the chief architect of the 1912 rupture, was a unifying force in 1904. Perhaps then at the height of his popularity, he received the full and enthusiastic support of all Wisconsin Republicans regardless of factional allegiances. But since McKinley and La Follette had run so close together in 1900, the great discrepancy between Roosevelt's and La Follette's vote demonstrates that thousands of stalwart Republicans who eagerly voted for Roosevelt refused to support La Follette and the other GOP nominees for state offices.

Although large stalwart defections from La Follette took place in all parts of the state, they were slightly greater in urban areas. Neither La Follette nor Roosevelt did so well in the six analysis cities as they did in the state as a whole. Roosevelt swept all six, but in none did his percentage exceed his state average. La Follette lost La Crosse and Oshkosh to his Democratic opponent, ex-governor George

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[26] For the politics of the La Follette administrations, see Margulies, *Progressive Movement in Wisconsin*, chap. 2, and Albert O. Barton, *La Follette's Winning of Wisconsin* (Madison, 1922).

Table 3. Republican Percentage of Total Vote, 1904, by Wards with Particular Ethnic and Occupational Characteristics

	Repub. Pres.	Repub. Gov.	Repub. Sec. St.	R. Pres. minus R. Gov.
Milwaukee, city	43.9	36.5	37.3	7.4
Middle Class	50.0	36.3	39.4	13.6
Upper+Middle Class	51.0	36.6	40.2	14.4
Working Class	39.9	35.2	35.0	4.7
German	44.2	38.8	38.7	5.4
Repub. Ethnic Groups	54.1	35.3	41.0	18.8
Democ. Ethnic Groups	40.5	36.8	35.8	3.7
La Crosse, city	59.0	43.8	46.7	25.2
Upper+Middle Class	66.1	41.5	46.6	24.6
Working Class	56.5	45.3	47.1	11.1
German	46.4	37.8	38.9	8.7
Anglo-American	69.0	47.6	52.9	21.4
Repub. Ethnic Groups	70.2	49.5	53.3	20.8
Democ. Ethnic Groups	48.0	38.8	40.2	9.3
Madison, city	60.5	50.4	50.3	10.1
Upper+Middle Class	69.3	54.9	55.6	14.4
Working Class	52.1	46.5	45.8	5.5
Repub. Ethnic Groups	69.1	54.6	54.9	14.5
Democ. Ethnic Groups	50.4	45.7	45.1	4.7

W. Peck, and only in Madison was he able to poll a majority, winning 50.4 per cent.

Even the most cursory comparison of the election returns with the ward occupational data reveals a pronounced lack of support for La Follette among middle- and upper-class voters in the six cities. In each, La Follette invariably ran most poorly, relative to the rest of the Republican ticket, in the middle- and upper middle-class wards.

Just how far La Follette and the progressive candidates on the GOP state ticket ran behind Roosevelt is demonstrated in Table 3, which compares the vote in ethnically and occupationally homogeneous wards in Milwaukee, La Crosse, and Madison for Roosevelt, La Follette, and the Republican candidate for secretary of state, plus the difference between the vote for Roosevelt and La Follette (hereafter referred to as the "stalwart" variable).[27] In each of the six cities, La Follette ran the farthest behind Roosevelt—and the rest of the state ticket—in middle- and upper-class wards and in wards populated by Anglo-Americans and normally Republican ethnic groups.

Specific examples from the six cities even more dramatically underscore the defections from La Follette. In Oshkosh, Roosevelt won 12 of 13 wards while La Follette won only three; La Follette ran more than 20 per cent behind the President in seven wards. In Ward 7, which had 51.8 per cent of its employed residents in upper- or middle-class occupations and was the only ward with a majority of old stock American families, Roosevelt amassed 71.5 per cent of the vote while La Follette lost heavily, receiving only 37.7 per cent; the rest of the Republican state ticket scored a mean percentage of 51.3. The seven Oshkosh wards where La Follette ran at least 20 per cent behind Roosevelt included the six most prosperous wards and the five wards with the largest percentage of old stock American voters and members of "Republican" ethnic groups. Conversely, the five wards where La Follette ran the least behind other Republicans included four of the five most working-class wards, four of the five most German wards, and five of the six with the highest percentage of "Democratic" ethnic groups.

The same pattern persisted in other cities. In Sheboygan, the three wards where La Follette trailed the state ticket by at least five per cent ranked one, two, and three in middle- plus upper-class residents; La Follette ran ahead of the state ticket in the other five wards. In Milwaukee, where a strong Social-Democratic vote produced a three-party race, La Follette ran more than 20 per cent behind Roosevelt and six per cent behind the state ticket in the two most middle-class wards. The five Milwaukee wards which Roosevelt won but La Follette lost were the five most Anglo-American wards and included the four wards with the highest percentages of Republican-leaning ethnic groups; they were also among the top seven upper- plus middle-class wards.

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[27] Kenosha, Sheboygan, and Oshkosh are omitted from Table 3 only because of space limitations. The voting patterns were similar to those of La Crosse and Milwaukee.

The lack of middle-class support for La Follette and the progressives was not peculiar to the six analysis cities but was repeated in city after city around the state, regardless of size, economic or ethnic composition, or location. In ethnically heterogeneous Green Bay, La Follette ran more than 24 per cent behind Roosevelt in two wards: One was the city's elite ward, with 52.7 per cent of its employed residents engaged in middle- or upper-class occupations, and the other ranked second in that category. In Eau Claire in northwestern Wisconsin, Roosevelt ran 40 per cent ahead of La Follette in the third ward, which had the largest percentage of upper- plus middle-class residents; in contrast, the ward with the least stalwart defection had the largest percentage of workers and a relatively large German population. In Racine, the Governor ran far behind the President in the city's second ward, whose homes along the shore of Lake Michigan housed a larger share of the city's upper and middle classes than did any other.

The rejection of La Follette and progressivism by the middle and upper classes is even more graphically illustrated by statistical analysis. Table 4 presents simple correlation coefficients between ethnic and occupational data from Milwaukee and La Crosse and the vote for Republican, Democratic, and Social-Democratic candidates plus the "stalwart" variable. The significance of the stalwart defections can be seen by comparing columns 1 and 2. The correlations between the social variables and Roosevelt's vote are very similar to the normal relationships described for 1900 and 1908. The correlations involving the vote for La Follette, however, are strikingly different: the normally solid relationships between Republican vote and both upper plus middle class and Anglo-American are virtually nonexistent. In La Crosse, for example, percentage upper plus middle class correlates .450 with Roosevelt's vote, but the correlation between it and La Follette is *negative* (−.173); furthermore, upper plus middle class correlates positively with the vote for La Follette's Democratic opponent.[28] Submitting the data to regression analysis further corroborates the above conclusions. In each city, the relationships between ethnic variables and Roosevelt's vote were all statistically significant. But none of the relationships between La Follette and either upper plus middle class or the Anglo-American variable was significant at even the .1 level.[29]

Correlations involving the stalwart variable in column 3 are even more important. The extremely high correlations between upper plus middle class and the stalwart variable clearly

Table 4. Correlation Coefficients, 1904 Election

	Repub. Pres., 1904	Repub. Gov., 1904	R. Pres. minus R. Gov.	Democ. Pres., 1904	Democ. Gov. 1904	Soc. Dem. Gov., 1904
			MILWAUKEE			
Upper+Middle Class	.577	.119	.674	.516	.548	−.662
Middle Class	.380	.061	.556	.477	.505	−.583
Anglo-American	.681	−.018	.768	.389	.651	−.778
Democ. Ethnic Groups	−.669	−.036	−.689	−.351	−.605	.752
Working Class	−.782	−.046	−.745	−.492	−.653	.769
			LA CROSSE			
Upper+Middle Class	.450	−.173	.792	−.405	.151	
Repub. Ethnic Groups	.835	.529	.557	−.839	−.524	
Anglo-American	.610	.123	.690	−.575	−.089	
Democ. Ethnic Groups	−.808	−.502	−.549	.809	.493	
Working Class	−.316	.187	−.626	.268	−.178	

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[28] Correlation coefficients between the same ethnic and occupational variables and the rest of the Republican ticket—represented by the vote for secretary of state—fell roughly midway between those with Roosevelt and La Follette. For example, the correlation between upper plus middle class and vote for secretary of state was .346. These results indicate that some stalwarts cut the progressive state ticket as well as La Follette, but that many other stalwarts voted the entire GOP ticket except for governor.

[29] These results are not included in the tables.

indicate the force of the upper- and middle-class rejection of La Follette and progressivism in 1904.

Table 4 also demonstrates the extraordinary support that La Follette received, relative to other Republican candidates, from Germans and other normally Democratic voters and from the working classes. In La Crosse, percentage working class correlates positively with La Follette but negatively with Roosevelt and also correlates strongly negatively with the stalwart variable. The Democratic ethnic group variable correlates solidly negatively with the stalwart variable in both cities. Clearly, La Follette was able to offset some of the heavy losses he suffered among normally Republican voters by picking up support from people who voted Democratic and Social-Democratic for other offices.

The differences in the results for Milwaukee and La Crosse can largely be accounted for by the different political and ethnic makeup of the two cities. The strong showing the Social-Democratic party made in Milwaukee in 1904 by winning 28 per cent of the total vote for president and 25 per cent for governor had a significant impact upon the normal relationships between the major party vote and the socioeconomic variables.[30] As a consequence, the vote for the Social-Democratic gubernatorial candidate in Milwaukee was included in the table.[31]

The different ethnic distributions of the cities were also important. La Crosse had a large Scandinavian population. Consequently, in La Crosse the "Republican" ethnic group variable was a more inclusive measure than it was in Milwaukee. It had two major components, Anglo-Americans (mean percentage = 28.3) and Scandinavians (mean = 15.4). Normally, both groups voted strongly Republican. In 1904, however, these two groups reacted differently to La Follette. Scandinavians gave him strong support while the more prosperous Anglo-Americans did not.[32] The difference is more noticeable in the different correlations obtained when percentage Anglo-American is used as the ethnic variable than when the more inclusive Republican ethnic group variable is used.

Table 5 summarizes the results of regression analysis using the stalwart variable. The results clearly indicate that in general, class factors were more important than ethnicity in producing the stalwart defections from La Follette. This was especially true in La Crosse. Although

Table 5. Summary of Regression Analysis, Ethnic and Occupational Data with Stalwart Variable, Election of 1904

Dependent Variable	Independent Variable	Simple r	Beta	Signif. level of b $P=<$	Signif. level of equation $P=<$	R^2
		LA CROSSE				
% Stalwart (Rep. Pres. minus Rep. Gov.)	Upper+Middle Class	.792	.596	.0001	.0001	.740
	Anglo-American	.690	.388	.005		
	Working Class	−.626	−.521	.001	.001	.556
	Democ. Ethnic Groups	−.549	−.419	.005		
		MILWAUKEE				
% Stalwart	Anglo-American	.768	.574	.0005	.0001	.702
	Upper+Middle Class	.674	.388	.01		
	Working Class	−.743	−.513	.01	.0005	.637
	Democ. Ethnic Groups	−.689	−.372	.05		

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[30] The strong reaction along both ethnic and class lines to the Social-Democratic party produces the rare phenomenon of *both* Republican and Democratic votes correlating positively (or negatively) with the same social variable. The difference in the Social-Democratic percentages for the two offices represents a defection of about 800 normally Socialist votes that went to La Follette.

[31] The vote for governor is the only Social-Democratic variable presented because there was no discernible difference in the correlation coefficients involving the Social-Democratic vote for president, governor, or secretary of state. The Social-Democratic vote was not included in Table 4 because it was of little consequence in La Crosse; in 1904, Social-Democratic candidates for president and governor received more than three per cent of the vote in only one ward.

[32] The La Follette faction of the party had assiduously courted Scandinavian voters since 1894. For the voting patterns of Wisconsin Scandinavians, see Wyman, "Voting Behavior," chap. 12.

both upper plus middle class and Anglo-American are statistically significant variables in terms of their influence on the stalwart variable, the beta coefficient indicates that class was considerably more influential. In Milwaukee, however, ethnicity rather than class was apparently more influential in producing stalwart defections.[33] In both cities, however, class factors were clearly more important than ethnicity in producing the negative relationship between the stalwart defections and workers and members of the "Democratic" ethnic groups.

The lack of support among the middle and upper classes for progressivism is abundantly clear in 1904, the most important and symbolic single election in the Progressive Era in Wisconsin. Nevertheless, rejection of the contention that progressivism was a middle-class movement cannot rest upon a single election. Detailed investigation of other general and primary elections continues to underscore the lack of middle-class support for progressive candidates.

Middle-class defections in other general elections

The second major factional defection from a Republican gubernatorial candidate took place in 1912, when stalwarts refused to support McGovern's re-election bid. Stalwart opposition coalesced around the twin issues of the income tax and the expense of the additional commissions created in 1911. Once again, middle- and upper-class voters were prominent in the unusual support they gave to a conservative Democratic nominee (in this case John C. Karel) rather than to the duly nominated Republican candidate.

Analysis of defections from McGovern in 1912 is complicated by the three-way presidential race and by the absence of a statewide Bull Moose ticket. The split in the normal GOP vote between Taft and Roosevelt permitted Wilson to carry the state, but McGovern won re-election despite the loss of several thousand stalwart votes to Karel.

In Kenosha, Karel ran at least ten per cent ahead of the progressive Wilson in six wards, surpassing Wilson's vote in the middle- and upper middle-class second ward by as much as 16.8 per cent. In Sheboygan, the three wards with the greatest discrepancies between the presidential and gubernatorial vote for both parties[34] were those wards ranking one, two, and three in percentage of upper- and middle-class occupations. Karel won all three of these normally Republican wards, which voted Republican in all other gubernatorial elections during the era except 1904, when two of them—the two most prosperous—had also deserted La Follette. In Oshkosh, the only ward where Karel ran far ahead and McGovern far behind was the seventh, the highest in percentage of middle- and upper middle-class residents. Once again, this pattern was repeated around the state. In Racine, for example, Karel won by a substantial margin in the normally Republican second ward, which had also knifed La Follette in 1904.

Progressive candidates and upper- and middle-class voters in primary elections

The progressives fared poorly among upper- and middle-class voters in primary elections as well as in general election contests. A major problem in analyzing Wisconsin primary elections during the Progressive Era, however, is that there were no clear-cut progressive versus stalwart battles. All contested Republican gubernatorial primaries between 1906 and 1914 either had more than two candidates or else were not fought along progressive-stalwart lines. The closest approximation to straight fights were La Follette's bid for renomination in the 1910 senatorial preference primary and the La Follette-Taft contest in the 1912 GOP presidential primary. La Follette's overwhelming victories in these two elections, however, largely because of his towering presence in the state and his national reputation, make meaningful analysis difficult, since votes for La Follette cannot be automatically equated with "progressive" votes.

Nevertheless, careful analysis of selected primary elections in the six cities reveals an unmistakable pattern of the middle- and upper-class rejection of progressive candidates roughly parallel to the defections of 1904 and 1912. Four elections were selected for analysis: the two aforementioned contests involving La Follette plus the Republican gubernatorial primaries of 1910 and 1914. In 1910 there were three major candidates in the five-way race: McGovern, the unofficially endorsed candidate of the La Follette progressives; Edward T. Fairchild, a stalwart state senator; and auto manufacturer William M. Lewis, who adopted a progressive label but whose major appeal was his support of county option prohibition. In 1914, the failure of the progressives to agree on a gubernatorial nominee led to a six-way race. The progressives largely divided between Andrew H. Dahl and William H. Hatton, who also appealed to some moderates. The stalwart choice was Emanuel L. Philipp, whose victory in the primary with only 35 per cent of the vote and subsequent election brought an end to progressive dominance in Wisconsin.

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[33] A possible explanation of why ethnicity was more influential in Milwaukee is that while Anglo-American middle-class voters rejected La Follette, middle-class Germans supported him.

[34] In order to obtain relatively reliable indicators of stalwart bolting, the vote for Roosevelt and Taft was added together to get an approximation of the true Republican vote in 1912, and McGovern's vote was then subtracted from it; a similar comparison was made by subtracting Wilson's vote from Karel's.

The Wisconsin political situation had changed drastically between 1904 and 1910. The contest of 1904 had been characterized by a politics of factionalism. Although La Follette always framed his campaigns in terms of combating "the interests," the stalwarts were not so much opposed to reform in 1904 as they were opposed to La Follette himself. By 1910, however, the basis of division between stalwart and progressive was no longer factional but ideological, based not on opposition to one man but on left-right divisions over concrete issues such as the personal income tax and pro-labor legislation. Even the names of the contesting factions changed. In 1904, the word "progressive" was not in use, even by La Follette supporters; it was the "stalwarts" versus the "half-breeds." By 1910, the contest was between "progressives" and "standpatters," terms with definite ideological connotations. As the politics of factionalism was replaced by a politics of ideology, class factors took on more and more importance in voting behavior, in many cases equaling or surpassing cultural divisions. This was particularly true in primary elections, where party identification—the foremost determinant of voter choice, usually the product of cultural identities—was meaningless.[35]

La Follette swept the state with huge majorities in both the 1910 and 1912 primaries, winning 78 and 73 per cent, respectively. La Follette won easily in every ward in the six cities in 1910, failing to get at least 60 per cent in only three wards. His victory was not so spectacular in 1912, when he lost five wards in three cities, but he still posted a victory margin of at least 63 per cent in each city.

Despite La Follette's great success in all categories of wards in 1910 and 1912, there were nevertheless clearly distinguishable patterns of greater or lesser support. As in 1904, La Follette's opponents invariably ran strongest in middle- and upper-class areas. In Madison, Taft ran best in the three wards with the highest percentage of upper- and middle-class residents, and S. A. Cook—La Follette's opponent in 1910—posted his highest total in the most prosperous ward. In Oshkosh, La Follette's smallest percentage in both 1910 and 1912 came in Ward 7, the top upper- plus middle-class ward. In contrast, he received at least 90 per cent in three wards in 1910 (two of which also gave him 90 per cent or better in 1912); these wards ranked one, two, and four in percentage of normally Democratic-leaning ethnic groups (each had a majority of Germans and one also had large numbers of Poles and Russians), and one, two, and five in percentage of unskilled workers. In Kenosha, both Taft and Cook ran best in the most prosperous ward.

Purely local factors entered into the multi-candidate primaries (such as extraordinary support for a candidate who resided nearby) and make meaningful analysis of those elections in Kenosha and Oshkosh virtually impossible.[36] In Madison and Sheboygan, however, middle-class voters continued to reject progressives. In Sheboygan, the progressive McGovern ran poorest in 1910, and the stalwart Philipp ran best in 1914 in Ward 1, which ranked highest in percentage of middle and upper middle classes. In Madison, Fairchild won two of the three most middle-class wards in 1910, and in 1914, the only ward Philipp captured was the most prosperous one.

The cities of Milwaukee and La Crosse, where statistical analysis can be applied to the data, provide the best examples of the middle-class rejection of progressive candidates in Republican primaries. Because of wholesale ward boundary changes, the analysis of Milwaukee is limited to 1910. In the senatorial primary, La Follette won more than 62 per cent in every ward, failing to get 75 per cent in only five wards. Although middle- and upper-class voters gave La Follette a solid majority of their votes, La Follette won greater support from other voters. As seen in Table 6, La Follette received a mean percentage of 72.2

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[35] This theme is developed more fully in Wyman, "Voting Behavior," chap. 7.

[36] In Kenosha, for example, in 1910 Lewis, from nearby Racine, won three of the city's eight wards, severely undercutting McGovern in that city. In Oshkosh, the presence of local candidates in both 1910 and 1914 complicates any analysis, but nothing occurred to contradict the general pattern of weak middle-class support for progressive candidates.

Table 6. Percentage of Total Vote for Candidates in Primary Elections, 1910–1914, by Wards with Particular Ethnic and Occupational Characteristics

	Upper+ Middle Class	Anglo-American	Working Class	German Stock	Democratic Ethnic Grps.
Milwaukee					
La Follette, 1910	72.2	69.3	85.5	85.3	85.6
McGovern, 1910	42.2	39.9	50.2	52.4	53.1
Fairchild, 1910	45.3	45.4	39.1	37.9	37.7
Lewis, 1910	11.1	13.4	8.4	7.9	7.0
La Crosse					
La Follette, 1910	68.7	74.8	80.2	83.4	83.2
La Follette, 1912	59.3	73.1	82.0	81.1	80.8
McGovern, 1910	53.3	54.1	53.9	54.2	55.3
Fairchild, 1910	31.6	24.5	28.3	30.6	29.4
Dahl, 1914	30.0	44.7	43.6	35.7	34.8
Philipp, 1914	54.8	39.3	42.9	51.9	51.5

in the city's eight most prosperous wards, compared to at least 85 per cent in both working-class and German wards.

In the 1910 gubernatorial race, McGovern amassed a city-wide percentage of 46.4 in capturing 16 wards to Fairchild's seven. The antipathy of Milwaukee's upper and middle classes to the city's former crusading district attorney was striking. Fairchild won six of the eight most upper- plus middle-class wards and four of the five most Anglo-American wards. In sharp contrast, McGovern won 14 of the 15 most working-class wards, including all of the top ten. McGovern also ran well in German areas, polling a majority of votes cast in the six most German wards, and winning ten of the twelve wards with a majority of German families.

Correlation and regression analysis of the Milwaukee data, summarized in Table 7, corroborates and illuminates the patterns described above. A solid negative relationship appears between the two progressive candidates —La Follette and McGovern—and percentage upper plus middle class. At the same time, Fairchild's stalwart candidacy registers a strong positive relationship with the same variable. An interesting anomaly appears in the results of the regression analysis, however. An examination of the betas and the partial r's suggests

Table 7. Summary of Regression Analysis, Ethnic and Occupational Data with Candidates in Republican Primary Election in Milwaukee, 1910

Dependent Variable	Independent Variable	Simple r	Beta	Signif. level of b $P=<$	Signif. level of equation $P=<$	R^2
La Follette	Anglo-American	−.756	−.579	.0005	.0001	.667
	Upper+Middle Class	−.644	−.355	.025		
	Working Class	.791	.599	.0005	.0001	.686
	Democ. Ethnic Groups	.682	.311	.05		
McGovern	Anglo-American	−.833	−.730	.0001	.0001	.726
	Upper+Middle Class	−.570	−.206	n.s.		
	Democ. Ethnic Groups	.809	.601	.0001	.0001	.724
	Working Class	.708	.336	.025		
Fairchild	Upper+Middle Class	.738	.584	.005	.001	.616
	Anglo-American	.599	.308	n.s.		

n.s. = not significant at the .05 level.

Table 8. Summary of Regression Analysis, Ethnic and Occupational Data with Candidates in Republican Primary Elections in La Crosse, 1910–1914

Dependent Variable	Independent Variable	Simple r	Beta	Signif. level of b $P=<$	Signif. level of equation $P=<$	R^2
La Follette, 1910	Upper+Middle Class	−.749	−.629	.0005	.0005	.603
	Anglo-American	−.556	−.237	n.s.		
	Working Class	.550	.471	.01	.01	.398
	Democ. Ethnic Groups	.436	.319	n.s.		
La Follette, 1912	Upper+Middle Class	−.915	−.903	.0001	.0001	.838
	Anglo-American	−.482	−.024	n.s.		
	Working Class	.722	.677	.0001	.001	.552
	Democ. Ethnic Groups	.349	.181	n.s.		
Fairchild, 1910	Upper+Middle Class	.401	.617	.005	.01	.431
	Repub. Ethnic Groups	−.326	−.563	.005		
Dahl, 1914	Upper+Middle Class	−.554	−.800	.0005	.005	.481
	Anglo-American	.079	.484	.025		
	Working Class	.553	.684	.0001	.0005	.568
	Democ. Ethnic Groups	−.359	−.529	.001		
Philipp, 1914	Repub. Ethnic Groups	−.442	−.699	.0001	.001	.578
	Upper+Middle Class	.401	.669	.0001		
	Democ. Ethnic Groups	.416	.537	.005	.01	.396
	Working Class	−.355	−.488	.01		

n.s. = not significant at the .05 level.

that ethnicity was more influential in producing the opposition to La Follette and McGovern but that class factors were more important in determining stalwart support for Fairchild. A probable explanation is that middle-class Germans may have been attracted to McGovern and La Follette, and thus the core of stalwart strength may have resided among Anglo-American members of the upper and middle classes. The table also shows once again the support given to the progressives by workers and normally Democratic ethnic groups, on both class and ethnic grounds.

The more complete data for La Crosse, presented in Tables 6 and 8, amplify the findings of middle- and upper-class rejection of the progressives, but with one distinct difference from Milwaukee: Class factors seem to have been much more important than cultural forces in determining voting behavior in primary elections. In both 1910 and 1912, La Follette ran the poorest in the same four wards, three of which he lost to Taft in 1912; they ranked two through five in percentage of upper- plus middle-class residents. In 1910, La Follette posted his highest percentages in normally Democratic wards, but in 1912 the pattern was not so uniform: although the Senator received his highest percentages in two normally Democratic wards, two solidly Republican wards were also among those giving him at least 80 per cent. In 1912, La Follette polled a mean percentage of only 59.3 in La Crosse's seven upper- plus middle-class wards compared to 73.1 per cent in the four Anglo-American wards, 82.0 per cent in the 11 working-class wards, and 81.1 per cent in the seven German wards.

That the opposition to La Follette in La Crosse followed class lines, especially in 1912, is clear from Table 8. In each election, the class variables were much more influential than the ethnic variables. In 1912, both the simple r and the beta coefficient between La Follette's vote and upper plus middle class was in excess of −.9!

In the Republican gubernatorial primaries of 1910 and 1914, both ethnicity and class were important factors. Except for McGovern's candidacy, which produced no statistically significant relationships,[37] all other stalwart and progressive candidacies produced statistically significant relationships with ethnic and class variables. Class factors seemed particularly important in determining support for Fairchild in 1910: his top four wards ranked two through five in per cent upper plus middle class, and the beta coefficient between the two variables was .617.

Both ethnicity and class seemed extremely strong in 1914. In La Crosse, the six-way race was strictly between Philipp, who won 11 wards, and Dahl, who won the remaining ten. Each had a strong ethnic appeal: Dahl ran strongest among his fellow Scandinavians, and Philipp did very well among German voters. Despite these special ethnic attractions, the pattern of upper- and middle-class rejection of progressivism persisted. Philipp ran the strongest in the three banner stalwart wards that had voted for Taft in 1912, and the top five upper-plus middle-class wards were among those where Philipp won a majority. As in the other elections, percentage upper plus middle class showed a high positive correlation with the stalwart candidate and a strong negative correlation with the progressive.

Conclusions

The preceding analysis clearly demonstrates that progressivism in Wisconsin was far from being a middle-class or upper middle-class movement. Middle- and upper-class voters consistently refused to provide strong electoral support for progressive candidates in either general or primary elections. Instead, these prosperous citizens constituted the major source of support for anti-progressive candidates. In urban areas, stalwart Republicans invariably ran best in middle- and upper-class wards, and progressive nominees such as La Follette or McGovern often ran farthest behind the rest of the Republican ticket in the same areas.

In rejecting progressive reform at the polls, middle- and upper-class voters were motivated by both ethnocultural and class factors. In general, ethnocultural factors remained as the primary determinants of voter choice in general elections during the Progressive Era, although class factors became increasingly important during the period. In primary elections, however, the results for the two cities examined in greatest detail varied: In Milwaukee, ethnocultural forces seemed to predominate, while in La Crosse, class considerations seemed to outweigh cultural factors. Regardless of whether prosperous voters in these Wisconsin cities were motivated primarily by class or cultural factors or by the interaction of both, the result was the same; the middle and upper classes did not support progressivism.

Since the data used in this study deal only with Wisconsin, I cannot claim with authority that these findings can be extended to other states. The work of Rogin and Shover, however, suggests a roughly similar basis of support for progressivism in California, at least after 1912. Furthermore, there is no reason to think that the pattern which occurred in Wisconsin did not happen elsewhere. The same type of factionalism rent the Republican party in Iowa, Ohio, Indiana, and in other parts of the nation, including California and New York. Similar voting analyses for other states will probably produce comparable results.

The findings of this study raise two important questions: Just who *did* support the progressives? If progressivism was not a middle-class movement, what kind of movement was it?

A comprehensive analysis of the electoral support for progressivism in Wisconsin lies beyond the scope of this paper, and the data contained herein—dealing only with urban voters—can only offer partial answers to the questions posed above. Nevertheless, some basic generalizations can be offered, based upon data which are part of a more comprehensive study. First of all, Wisconsin progressivism had an extremely large core of rural support. The progressives were strongest in the poorer western and northern counties, especially those with significant Scandinavian populations. Second, in general elections, progressive candidates—running as duly nominated Republicans—drew upon traditional Republican sources of support, including many middle- and upper-class voters. It is significant to note that the two statewide independent candidacies supported by the progressive leadership (for attorney general in 1910 and for governor in 1914) failed miserably, with victory in each case going to the only stalwart on an otherwise progessive Republican dominated ticket. In primary elections, the rural and Scandinavian components were aided significantly by the thousands of "fair-minded Democrats" who invaded the Republican primary to vote for progressive Republicans. In urban areas, this extra measure of support came largely from German and working-class voters. In addition, progressives received the votes of a sizable minority of middle- and upper-class voters.

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[37] McGovern won easily in La Crosse, capturing 20 of 21 wards. There were no distinguishable ethnic or class patterns in his vote, and analysis of it is omitted from Table 8.

Progressivism, in Wisconsin and throughout the United States, was a very complex and amorphous phenomenon. Its basis of support at the polls was constantly shifting, as was the mainstream of progressivism itself. Victory at the polls for a given progressive candidate or referendum proposition was often the product of a voting coalition that differed markedly from coalitions put together in previous or subsequent campaigns; these coalitions often varied sharply in their ethnocultural and economic composition.[38] If progressivism can indeed be termed a "movement," it was one which transcends simple classification along either class or cultural lines.

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[38] Buenker, "The Progressive Era," pp. 184–192.

On the Size of Winning Coalitions*

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On the Size of Winning Coalitions

In a recent note Robert Butterworth[1] argues that the size principle, William Riker's theorem about the size of winning coalitions in *n*-person zero-sum games with side-payments, is in error. He asserts in its place his maximum positive gainers principle, thereby demonstrating that winning coalitions need not be of minimum winning size. In this paper I elaborate on Butterworth's critique, correcting what I believe to be an important theoretical omission. In so doing I am able to conclude on a somewhat more sanguine note than Butterworth about the viability of the size principle. Butterworth, however, raises several important objections which I endeavor to make explicit below.

The discussion proceeds as follows. After presenting Butterworth's challenge to the size principle, I introduce several additional theoretical considerations which place Butterworth's critique in a slightly different light. To this point the argument is carried entirely by example. A somewhat more formal (though not terribly rigorous) game-theoretic analysis is then presented, primarily to raise hitherto suppressed issues in solution theory upon which a truly general theory of coalitions must ultimately rest. Of primary concern here are alternative solution concepts, most notably the von Neumann-Morgenstern *stable set*[2] and the Aumann-Maschler *bargaining set*,[3] and the implications they hold for the size of winning coalitions. I conclude with some observations on the prospects for generalization.

The Two Principles Stated

A decade ago William Riker asserted and proved his famous size principle:

> In *n*-person, zero-sum games, where side-payments are permitted, where players are rational, and where they have perfect information, only minimum winning coalitions occur.[4]

More recently he has provided us with a shortened and more compact proof of this theorem. The result is limited to those games whose characteristic function has nonpositive slope in the range of winning coalitions.[5]

Butterworth, on the other hand, argues, for the conditions outlined in the size principle, that the size of the eventual winning coalition is indeterminate. Although it is possible to observe winning coalitions of minimal size—where minimal size is specified in the rules of the game—it is also quite conceivable that larger winning coalitions form. In effect, then, Butterworth challenges the *necessity* of minimum winning coalitions for the conditions specified. In so challenging the size principle he offers us an alternative principle—the maximum positive gainers (MPG) principle—which formalizes some of the same intuition possessed by the size principle, but which places no upper bound on the size of winning coalitions:

> In *n*-person, zero-sum games with a nonpositively sloped characteristic function, where side-payments are permitted, where players are rational, and where they have perfect information, the largest possible number of *positive gainers* that can occur is the number of players necessary to comprise a minimum winning coalition.[6]

Note that the MPG principle restricts only the number of positive gainers, i.e., the number of players with positive payoffs, not the size of winning coalitions. As is illustrated below, Butterworth allows for the possibility that additional players may "bribe" their way into a minimal winning coalition, rendering it nonminimal winning.

To further elucidate the MPG principle and to illustrate the challenge it poses to the size principle, it is useful to examine the example on which

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* The author would like to acknowledge the helpful criticisms of an earlier draft of this paper by Steven Brams and William Riker, as well as those of several anonymous referees. Although considerably revised, this current version may still have failed to accommodate all of their remarks. The critics, therefore, are not responsible for what is contained herein. Financial support by the National Science Foundation, under Grant GS-33053, is also appreciatively acknowledged.

[1] Robert Butterworth, "A Research Note on the Size of Winning Coalitions," *American Political Science Review*, 65 (September, 1971), 741–745.

[2] John von Neumann and Oskar Morgenstern, *The Theory of Games and Economic Behavior*, science edition (New York: John Wiley, 1964).

[3] Robert Aumann and Michael Maschler, "The Bargaining Set for Cooperative Games," in *Advances in Game Theory*, ed. Melvin Dresher, L. S. Shapley, and A. W. Tucker (Princeton: Princeton University Press, 1964), pp. 443–476.

[4] William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), p. 32.

[5] William H. Riker, "A New Proof of the Size Principle," in *Mathematical Applications in Political Science, II*, ed. Joseph L. Bernd (Dallas: Southern Methodist University Press, 1966), pp. 167–174. For the moment, technical definitions and notation are avoided. Thus, the definition of the characteristic function will remain vague until a more formal analysis is employed. Suffice it to say that the characteristic function of a game specifies the worth or value of a coalition.

[6] Butterworth, "A Research Note," p. 744.

Butterworth relies. Notational matters, however, must first be disposed of.

Some Notation and Definitions

A *game* $G(n, v)$ is represented by a set of n players $I=\{1, 2, \ldots, n\}$ and a characteristic function v.[7] If we let S represent the set of all possible coalitions of players in I (that is, S is the power set of I) and let B and C be typical elements of S, then the *characteristic function* v is a real-valued set function defined on all $B, C \in S$ such that

(1.1) $v(\varnothing)=0$, where $\varnothing$ is the set containing no players; and

(1.2) $v(B \cup C) \geq v(B)+v(C)$, where B and C are disjoint. If $G(n, v)$ is a *zero-sum game*, then an additional property of v is

(1.3) $v(B)=-v(B')$, where B' is the complement of B. Finally, if the value of a coalition B depends only on the number of players in B, written $|B|$, then $G(n, v)$ is said to be a *symmetric game:*

(1.4) $v(B)=f(|B|)$.

Property (1.1) is included for technical completeness. Property (1.2)—*superadditivity*—underscores the profitability of cooperation by requiring that coalition formation never be positively harmful. Thus a coalition between the players in B and those in C is worth at least as much as the sum of the individual worths of B and C. If the equality sign holds for all $B, C \in S$, then $G(n, v)$ is said to be *inessential.* The game is *essential* if the inequality sign holds for some $B, C \in S$. Since essential games provide a setting in which coalition formation is profitable, throughout I assume that the inequality sign in (1.2) holds for some coalitions.

Property (1.3)—the *zero-sum condition*—requires that the winners win what the losers lose or, alternatively put, that the totality of winnings in a play of the game sums to zero. An immediate consequence of (1.3), letting $B=I$ and recalling (1.1) since $B'=\varnothing$, is that the coalition-of-the-whole is worthless: $v(I)=0$.

Properties (1.1)–(1.3) completely describe an n-person zero-sum game in characteristic function form. I shall additionally impose the symmetry property (1.4) without severely jeopardizing the argument. In effect, (1.4) is an anonymity condition—the labels identifying the players in a coalition are not relevant to a determination of a coalition's worth; this depends only on the *number* of players it contains.[8]

Armed with some notation and common language we may now inquire into the conflict between the size principle and the MPG principle.

An Illustration

Butterworth, in his critique of the size principle, considers a five-person zero-sum game characterized as follows:

$v(B)=$	whenever $\lvert B \rvert=$
0	0
−20	1
−30	2
30	3
20	4
0	5

Given the symmetric nature of the game, Butterworth focuses on outcomes in which the spoils of victory are shared symmetrically by winners; losses are similarly borne by the losers. Thus a typical imputation for three-, four-, and five-member winning coalitions is:[9]

Table 1. Symmetric Imputations of a Five-Person Zero-Sum Game

Number of Players in Winning Coalition	Individual Payoffs to Players				
	a	*b*	*c*	*d*	*e*
5	0	0	0	0	0
4	5	5	5	5	−20
3	10	10	10	−15	−15

Butterworth notes that the prediction of the size principle is one of ten combinations of the last row of Table 1. He argues, however, that one of two possibilities may well materialize. An oversized coalition may exist during the course of the game which has no incentive, contrary to the size principle, to pare off some of its members. Or, alternatively, a minimal winning coalition may find it profitable—and hence rational—to add members, again contrary to the size principle, and despite the nonpositively sloped characteristic function. In each case his argument rests on the existence of an individually rational bribe.

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[7] Not all games are adequately represented by a characteristic function. In *Competition, Welfare, and the Theory of Games* (unpublished manuscript), L. S. Shapley and Martin Shubik suggest a number of deficiencies in a characteristic function representation to which the interested reader is referred. For additional formal considerations see Robert W. Rosenthal, "Cooperative Games in Effectiveness Form," *Journal of Economic Theory,* 5 (August, 1972), 88–101, and Robert Wilson, "Stable Coalition Proposals in Majority-Rule Voting," *Journal of Economic Theory,* 3 (September, 1971), 254–271. Throughout I shall focus exclusively on games in characteristic function form—what Shapley and Subik call *c-games.*

[8] It is clear that many political games are *not* symmetric. In a legislature, for example, a majority coalition that does not contain a majority of members on a relevant committee and/or subcommittee is usually not winning. The status of a coalition, in this case, does depend on the labels or roles which identify its members.

[9] It is, of course, the case that players $a, b, \ldots, e$ may be permuted among the payoffs for each coalition size. There are $\binom{5}{3}=10$ three-member coalitions and $\binom{5}{4}=5$ four-member coalitions.

In the game characterized above Butterworth shows that one of the players in a maximally losing coalition, say d, has incentives to bribe $\{a, b, c\}$ in order to effect the oversized coalition $\{a, b, c, d\}$. In particular, if d offers six "points" to each of the members of the three-member coalition, then the resulting imputation dominates the imputation in row 3 of Table 1 vis-à-vis the effective set $\{a, b, c, d\}$. To see this we may partition the payoffs to players in the configuration resulting in an oversized coalition according to their sources: (i) payoffs resulting from the spoils of victory (defeat), and (ii) side-payments (bribes):

Table 2. An Individually Rational Bribe

Source of Payoff	Players				
	a	*b*	*c*	*d*	*e*
Spoils of victory	5	5	5	5	−20
Side-payments (bribes)	6	6	6	−18	0
Total payoff	11	11	11	−13	−20

Notice that the imputation (11, 11, 11, −13, −20) dominates (10, 10, 10, −15, −15) vis-à-vis the effective set $\{a, b, c, d\}$. As a result Butterworth claims to have shown that

> the payoffs as given by the rules of the three-person winning coalition dominate those given to the four-person winning coalition [compare lines 2 and 3 of Table 1], as Riker says; *but* the payoffs of the four-person winning coalition with bribing in operation dominate those of the three-person coalition without bribing.[10]

But certainly Mr. e is not going to stand by and let the transformation resulting from d's bribe take place. Butterworth argues, however, that e's behavior does not affect his principal result, i.e., the indeterminateness of the size of winning coalitions and hence the invalidity of the size principle. His argument proceeds as follows. Inspection of the two relevant imputations, (11, 11, 11, −13, −20) and (10, 10, 10, −15, −15), indicates that Mr. e stands to lose an additional five points if the bribe is tendered by d and accepted by $\{a, b, c\}$. Mr. e, therefore, would find it individually rational to offer d up to 5 points to remain in the losing coalition $\{d, e\}$. But then, Mr. d could revise his bribe to $\{a, b, c\}$ downward to 15 points (5 points each). The net result is that $\{a, b, c\}$ is indifferent between including and excluding d, d is indifferent between membership in $\{a, b, c, d\}$ and $\{d, e\}$, and e is indifferent between being in a one-man and being in a two-man losing coalition. The "equilibrium" imputation, in other words, is (10, 10, 10, −10, −20), and the coalition structures consistent with this imputation include both three-member and four-member winning coalitions. The result is an indeterminacy in the size of winning coalitions and, consequently, a rejection of the size principle.

Butterworth's rejection of the size principle derives from a fuller exploitation of the assumption of permissible side-payments than had been employed by Riker. N-person games with side-payments place no restrictions on *who* may make them. Riker, in his initial work, emphasized a sociologically specific notion of side-payment activity—side-payments, for Riker, are contingent payments made by *leaders*. The concept of leadership, however, is ambiguous in the context of n-person games, and the rules of the game do not make *a priori* designations. Butterworth's observation that "nonleaders" may also engage in the making of side-payments (what he calls "bribes") is germane to this point. Unfortunately, Butterworth has erred in his development of this point, thereby undercutting his conclusion about the size principle.

Butterworth's error, in his otherwise interesting note on a more general interpretation of side-payments, lies in his arbitrarily narrow restriction on the set of strategic alternatives available to the members of a maximally losing coalition. In his example presented above, he permits Mr. e, who stands to lose if Mr. d's bribe is accepted by $\{a, b, c\}$, only to attempt to bribe d back into a coalition with him. Mr. e, however, has additional (and, in my opinion, more efficacious) alternatives available. Notice that in Butterworth's "equilibrium" imputation (10, 10, 10, −10, −20), e stands to lose 20 points, whatever the coalition structure. Were he included in a four-member coalition with $\{a, b, c\}$, in which case his share of the spoils is five points, he could rationally afford to expend up to 25 points in bribe payments and still be at least as well off as in the alleged "equilibrium" imputation. *That is, there are very real incentives for e to bid up the price of bribery.* Suppose, for example, that e responded to d's initial bribe offer of 6 points to each member of $\{a, b, c\}$ (second line in Table 2) with his own offer of 7 points to each of $\{a, b, c\}$:

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[10] Butterworth, "A Research Note," p. 742.

Table 3. A Bribe Response

Source of Payoff	Players				
	a	*b*	*c*	*d*	*e*
Spoils of victory	5	5	5	−20	5
Side-payments (bribes)	7	7	7	0	−21
Total payoff	12	12	12	−20	−16

Now the imputation (12, 12, 12, −20, −16) dominates the imputation (11, 11, 11, −13, −20)—the imputation resulting from *d*'s initial bribe—and it dominates the alleged "equilibrium" imputation, (10, 10, 10, −10, −20), in both cases vis-à-vis the effective set $\{a, b, c, e\}$. *Therefore, the latter imputation can hardly qualify as an "equilibrium" point.* By this stage of the process, although Mr. *d* may continue to compete with *e* for entry into an oversized winning coalition, it is clearly in the interest of both *d* and *e* to revert to their original maximal losing coalition. That is, the imputation (10, 10, 10, −15, −15) dominates (12, 12, 12, −20, −16), as well as any other bribe response *d* might make, vis-à-vis the effective set $\{d, e\}$. But this is simply to reassert the size principle.[11]

But is it? Given that (10, 10, 10, −15, −15) is preferred to (12, 12, 12, −20, −16) by $\{d, e\}$, and that $\{d, e\}$ is effective for that imputation, do these facts reassert the size principle? After the process has come full circle there is nothing preventing its recurrence. That is, after experiencing a series of bribes and counter-bribes with the consequent bidding up of the price of bribery, and after the reassertion of the maximal losing coalition $\{d, e\}$, there is nothing inherent in the structure of the game to prevent *d* (or *e*) from beginning the process again. Indeed, there is a very real incentive to desert.

There is clearly a problem here, which may be traced to two distinct sources. The first source is one we can do very little about—zero-sum essential games are inherently unstable. I shall have more to say about this shortly. The second source of our problem is one of theoretical language. Throughout the previous discussion I have purposely enclosed the word "equilibrium" in quotation marks. To speak of equilibrium in a game $G(n, v)$ is to have some *solution* in mind, the elements of which possess certain properties of stability. Having specified a solution concept, and having assured ourselves that the game $G(n, v)$ possesses a solution, it is then (and probably only then) appropriate to ask whether anything general may be said about the size of winning coalitions. Neither Riker nor Butterworth tie their analysis explicitly to a *formal solution* to $G(n, v)$, and this, I believe, has been a source of some confusion.[12] In the following section, solution considerations are introduced in order to clarify the status of the size principle.

Solution Theory and the Size Principle

There are a number of solution concepts in the theory of games, but a discussion of many would require extended detail inappropriate here.[13] I focus, primarily, on two: the von Neumann-

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[11] Riker, it seems, is very close to making this observation when he notes the tension of a coalition that receives more than it is objectively worth. See William H. Riker, "Comment on Butterworth, 'A Research Note on the Size of Winning Coalitions,'" *American Political Science Review*, 65 (September, 1971), 745–747.

[12] Riker, in *Theory of Political Coalitions*, pp. 38–39, is quite explicit in his recognition of the central role played by solution theory in the analysis of *n*-person games. He was, at that time, quite dissatisfied with the progress made in determining what constituted a solution to a game:

> The net effect of a study of these attempts to limit the number of admissible imputations is to leave one with a sense of dissatisfaction. . . . It may be, of course, that the reasonable outcomes in an *n*-person model or an *n*-person real situation are in fact so numerous and diverse that systematic analysis and prediction is impossible. *But it may also be that game theorists have not asked the questions most useful to social scientists and that by exclusive emphasis on the attempt to delimit admissible imputations they have overlooked the possibility of delimiting coalition structures directly* (emphasis added).

[13] An excellent, readable discussion of solution theory is found in Shapley and Shubik, *Competition, Welfare, and the Theory of Games.* Other important treatments include Robert Aumann, "A Survey of Cooperative Games Without Sidepayments," in *Essays in Mathematical Economics in Honor of Oskar Morgenstern,* ed. Martin Shubik (Princeton: Princeton University Press, 1967), pp. 3–27; Aumann and Maschler, "The Bargaining Set"; B. R. Gelbaum, "Symmetric Zero-Sum *n*-Person Games," in *Contributions to the Theory of Games, IV,* ed. A. W. Tucker and R. D. Luce (Princeton: Princeton University Press, 1959), pp. 95–109; R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1957); L. S. Shapley, "Simple Games: An Outline of the Descriptive Theory," *Behavioral Science,* 7 (January, 1962), 59–66; L. S. Shapley, "Solutions of Compound Simple Games," in *Advances in Game Theory,* ed. Melvin Dresher, L. S. Shapley and A. W. Tucker (Princeton: Princeton University Press, 1964), pp. 267–306; L. S. Shapley, "Compound Simple Games, III," RM-5438-PR (Santa Monica: RAND Corporation, 1967); William Vickrey, "Self-Policing Properties of Certain Imputation Sets," in *Contribution to the Theory of Games, IV,* ed. A. W. Tucker and R. D. Luce (Princeton: Princeton University Press, 1959), pp. 213–246; Robert Wilson, "A Class of Solutions for Voting Games," Working Paper #156, Stanford Business School, 1968; and Robert Wilson, "Stable Coalition Proposals." Of course, von Neumann and Morgenstern, *The Theory of Games,* is an historically important primary source. It might also be added that Shapley has contributed a very large number of significant results, only a small number of which are cited here.

Morgenstern *stable set* and the Aumann-Maschler *bargaining set.*

Most solution concepts rely directly on the characteristic function. Indeed, for *inessential* games, where the equality sign holds for all $B, C \in S$ in (1.2) above, the characteristic function itself provides a solution: the payoff vector $(v\{i\}, \cdots, v\{i\})$ tells us the amount each player can expect from optimal, independent behavior (independent because coalition formation is unprofitable in inessential games). It therefore constitutes a solution to the game. Though we shall not concern ourselves here with inessential games, we focus exclusively on games that are "adequately" represented by a characteristic function. C-games or "games of consent" are identified by a special sort of privacy: a coalition cannot affect the *distribution* of gains or losses among the members of other coalitions.[14] The import of this fact for Butterworth's five-person game will become apparent below.

Solutions to cooperative n-person games are ordinarily cast in terms of a single payoff vector or a collection of payoff vectors which satisfy certain (usually behavioral) criteria of stability. The *core* and the von Neumann-Morgenstern *stable set* are solution concepts of this sort. In addition, a solution concept may specify a coalition structure that sustains a given payoff vector. Thus, in the Aumann-Maschler *bargaining set* concept, a solution is a set of elements $(x; K)$, where x is a payoff vector $(x_1, x_2, \cdots, x_n)$, x_i is the payoff to the ith player, and K is a coalition structure $(K_1, K_2, \cdots, K_m)$ which sustains x.[15]

Imputations

Although there are significant differences of detail in the various solution concepts, most begin with the idea of an *individually rational*, *feasible*, *Pareto optimal* payoff vector, $(x_1, x_2, \cdots, x_n)$. Such vectors, called *imputations*, have the following formal characteristics:

$$(2.1) \qquad x_i \geq v(\{i\}) \qquad \text{(individual rationality)}$$

$$(2.2) \qquad \sum_{i \in I} x_i \leq v(I) \qquad \text{(feasibility)}$$

$$(2.3) \qquad \sum_{i \in I} x_i \geq v(I) \qquad \text{(Pareto optimality)}$$

Characteristics (2.2) and (2.3) together imply:

$$\sum_{i \in I} x_i = v(I),$$

and the zero-sum condition (1.3) allows us further to write:

$$\sum_{i \in I} x_i = 0.^{16}$$

Inequalities (2.1) and (2.2) are quite straightforward. The former precludes a payoff vector any component of which gives a player less than he could obtain under the worst of circumstances (namely, when all the other players in I coalesce against him). The latter restricts consideration to feasible payoff vectors as determined by the rules of the game. Thus, in zero-sum games, redistribution is the sole activity of coalition formation (since the sum of all payoffs is zero). Pareto optimality is somewhat more restrictive, but is reasonable nonetheless for a first cut. Inequality (2.3), in effect, requires that all "gains from trade" be exhausted by the coalition-of-the-whole. Suppose the vector $x = (x_1, x_2, \cdots, x_n)$ were such that

$$v(I) - \sum_{i \in I} x_i = \epsilon (\epsilon > 0),$$

a contradiction of (2.3). Then one could construct a new, feasible payoff arrangement $y = (y_1, y_2, \cdots, y_n)$, where $y_i = x_i + \epsilon/n$. Since $\epsilon > 0$ everyone is better off with y than with x. With (2.3) it is assumed that the additional gains to be had—namely ϵ—will be distributed among the $i \in I$.

The *imputation space*, A, is the set of vectors satisfying (2.1) through (2.3). Conceivably we could call the collection A a solution to $G(n, v)$. There are, however, two objections to this possibility. First, the imputation space is not a very sharp or discriminating notion of solution. It lets in too much and, as a result, is not very informative. Second, little use is made of known coalitional potentials as provided in the characteristic function, v. Information about extreme coalitions —singleton coalitions and the grand coalition—is employed in (2.1) through (2.3), but additional information provided by v is left unused. A more discriminating solution concept will isolate, on the basis of additional information, specific subsets of A. The von Neumann-Morgenstern stable set is just such a concept. Before turning to it, it is necessary to introduce the notion of *dominance*, a concept implicitly used above in defense of characteristic (2.3). It is also convenient to examine one other solution concept, one that eminently satisfies the objections to the imputation space as solution.

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[14] In a zero-sum game, for example, a coalition C does determine the *payoff* for C', namely $-v(C)$, but it cannot affect the way in which the $i \in C'$ distribute $-v(C)$ among themselves.

[15] Recall note 12 above. Riker sought to bypass altogether the notion of solution as an "acceptable" distribution of gains and losses, instead focusing exclusively on coalition structures.

[16] Technical note: These conditions apply to the case of transferable utility. An analogous set of conditions exists for the nontransferable case. See Aumann, "A Survey of Cooperative Games."

Dominance and the Core[17]

Consider two imputations, x, $y \in A$, where $x=(x_1, x_2, \cdots, x_n)$ and $y=(y_1, y_2, \cdots, y_n)$. (Recall that x_i is the payoff to player $i \in I$ if x is the outcome to $G(n, v)$.) Also consider a coalition $C \in S$. (Again recall that S is the set of all possible coalitions.) The coalition C is *effective* for x if

$$(3.1) \qquad \sum_{i \in C} x_i \leq v(C).^{18}$$

That is, a coalition C is effective for an imputation x if the claim on winnings made by its members does not exceed the value of the coalition as specified by v. A coalition of players effective for a given imputation is called an *effective set*.

Let R_i be the ordinary complete and transitive binary preference relation for $i \in I$. Thus xR_iy means that i strictly prefers x to y or is indifferent between them.[19] Similarly, P_i and I_i are the strict preference and indifference relations for i, respectively, i.e., $R_i \equiv P_i \cup I_i$. The idea of *coalitional preference* now may be specified. A coalition C strictly prefers x to y if and only if xP_iy for all $i \in C$:

$$(3.2) \qquad xP_Cy \Leftrightarrow xP_iy \qquad \forall i \in C.$$

The imputation x is said to *dominate* y, written xD_Cy, if there exists an effective set C for which (3.2) is satisfied:

$$(3.3) \quad xD_Cy \Leftrightarrow \sum_{i \in C} x_i \leq v(C) \quad \text{and} \quad xP_Cy.$$

With the idea of dominance it is possible to inspect the set A of imputations in light of coalitional capabilities and preferences. In particular, we may ask whether there are $x \in A$ which are undominated.

For any $x \in A$ define the *dominion* of x as the set of $y \in A$ dominated by x:

$$(3.4) \qquad \begin{aligned} &\text{dom }(x) \\ &\quad = \{y \in A / xD_Cy \text{ for some } C \in S\}. \end{aligned}$$

(3.4) may be generalized so that we may refer to the dominion of a *set* of imputations. If X is a subset of the imputation space, i.e., $X \subseteq A$, then

$$(3.5) \qquad \begin{aligned} \text{dom }(X) = \{&y \in A / xD_Cy \\ &\text{for some } x \in X \text{ and } C \in S\}. \end{aligned}$$

Thus dom (X) is the set of imputations dominated by some $x \in X$.

The *core* of $G(n, v)$ is the collection of $x \in A$ that are not dominated by any other imputation:

$$(3.6) \quad \text{Core } [G(n, v)] = A - \text{dom } (A)$$

where dom (A) is defined as in (3.5). The core is the set of $x \in A$ that remain after all dominated imputations are removed.

The core is an extremely strong solution concept. This may be seen by re-examining (3.1) through (3.5) in order to reinterpret (3.6). The result of that exercise is an equivalent definition of the core that highlights its strict nature:

The core is the set of $x \in A$ for which

$$(3.7) \qquad \begin{aligned} &x_i \geq v(\{i\}) \\ &\qquad\qquad \text{(individual rationality)} \\ &\text{and } \sum_{i \in C} x_i \geq v(C) \qquad \text{for all } C \in S \\ &\qquad\qquad \text{(coalitional rationality).} \end{aligned}$$

Referring to (2.1) through (2.3) it may be seen that whereas the imputation space is the collection of individually rational payoff vectors *which are coalitionally rational for the grand coalition, I*, the core is that subset of A which is coalitionally rational for *all* $C \in S$. It is clearly a highly desirable solution concept that isolates those imputations which are, in a particular sense, stable: once adopted, an element of the core cannot be upset by any cooperative-collusive behavior.

Given the obvious virtue of the core as a solution concept, it becomes important to determine whether or not it exists for $G(n, v)$. And here we run afoul, for it may be demonstrated that, for the class of zero-sum games, the core exists if and only if $G(n, v)$ is inessential (in which case $[v(\{i\}), \cdots, v(\{i\})]$ is in the core); otherwise the core is empty. That is, zero-sum essential games are inherently unstable—the dominion of the imputation space, dom (A), is the imputation space itself. No imputation is undominated.[20]

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[17] Much of this section follows Robert Wilson, "A Game-Theoretic Analysis of Social Choice," in *Social Choice*, ed. Bernhardt Lieberman (New York: Gordon and Breach, 1971), pp. 393–407, Wilson, "Stable Coalition Proposals," and Shapley and Shubik, *Competition, Welfare, and the Theory of Games*, although there have been some notational changes.

[18] For a generalization of *effectiveness* that does not rely on the characteristic function, see Rosenthal, "Cooperative Games," and Wilson, "Stable Coalition Proposals."

[19] Throughout I shall assume that xR_iy means $x_iR_iy_i$. Player i, that is, evaluates the imputations x and y solely according to his own payoffs.

[20] Other core-like solution concepts have been examined in the game theory literature. The ϵ-core, for example, is the set of imputations in which every coalition C receives within an ϵ of its value:

$$\begin{aligned} &\epsilon\text{-core } [G(n, v)] \\ &\qquad = \left\{x \in A \Big/ \sum_{i \in C} x_i \geq v(C) - \epsilon \text{ for all } C \in S\right\}. \end{aligned}$$

As solutions, the imputation space and the core are at opposite extremes. The former allows too much, the latter excludes too much. While we must be resigned to the fact that instances of social interaction that are zero-sum and essential in nature do not admit of the kind of stability reflected in the core, we can still improve upon the imputation space. A middle ground charted by von Neumann and Morgenstern will prove especially useful for our examination of coalition size. Structurally, their *stable set*, V, has some of the virtues of the core and is more determinate than the imputation space, A. In fact, core $[G(n, v)] \subseteq V \subseteq A$, so that V is something of an analytical compromise candidate.[21] There is still, however, a frustrating indeterminacy in stable sets—an indeterminacy that spills over into our concern about coalition size. To these issues we now turn.

Von Neumann-Morgenstern Stable Sets

The von Neumann-Morgenstern stable set, V, is a *collection* of imputations possessing two properties of stability. First, for every imputation outside of the stable set, there exists one in V that dominates it. As a result, the imputation space may be characterized as the union of two sets:

$$(4.1) \quad V \cup \text{dom}\ (V) = A \quad \text{(external stability)}$$

It is easy to see that the external stability of a set is very much a function of its size. If we make a set large enough, it is often possible to render it externally stable. Thus in a trivial sense the entire imputation space A is externally stable. To counterbalance this effect of size a second stability property is required by von Neumann and Morgenstern. A set V is said to be *internally stable* if no imputation in V is dominated by any other imputation in V. That is, the dominion of V is disjoint from V:

$$(4.2) \quad V \cap \text{dom}\ (V) = \varnothing \quad \text{(internal stability)}$$

Together, (4.1) and (4.2) imply

$$(4.3) \qquad V = A - \text{dom}\ (V).$$

Comparing (4.3) with the definition of the core (3.6), one notices that the "nondominance" property of the core is much more stringent than that of the stable set. An imputation in the core of a game is undominated by all $x \in A$; an imputation in a stable set of a game is simply undominated by all $x \in V$.

In the case of stable sets there are both existence and uniqueness problems. First, games have been discovered for which V is empty.[22] Second, and perhaps even more frustrating for our purposes, a game often has a multitude of stable sets. Precisely this fact led Riker to bypass solution theory altogether (see note 12 above). What is needed is some means of providing a social interpretation to various stable sets in a given social context. Indeed, von Neumann and Morgenstern interpret a stable set as a "standard of behavior." Which standard comes to govern a given game is, for them, an extra-game-theoretic, sociological question—a standard of behavior in their view reflects sociological conditions not contained in the formal rules of the game.

While a rigorous examination of the size principle in the context of solution theory must address the problems of existence and uniqueness observed above, it is nonetheless appropriate to begin a more modest inquiry. Consequently this task is begun below with consideration given to a heuristically interesting stable set.

Stable Sets and the Size Principle

Consider the five-person, symmetric, zero-sum game employed in Butterworth's note. For convenience, the characteristic function of that game is again provided:

$v(B) =$	whenever $\lvert B \rvert =$
0	0
−20	1
−30	2
30	3
20	4
0	5

The following theorem identifies the principal stable set of this game:

THEOREM: The set V of permutations of the vector $(10, 10, 10, x, -30 -x)$ with $-20 \leq x$

If the costs of coalition formation and negotiation exceed ϵ, then the elements of the ϵ-core may be quite stable, even if some coalitions receive less than their value. Moreover, the ϵ-core always exists for some ϵ. Of course, the ϵ-core and the core are identical for $\epsilon = 0$.

[21] It should be noted that one buys more than existence when imputations are joined to the core to produce V. The core lacks *external stability* (see (4.1) below) so that there may be some difficulty in getting into the core, even when it exists. That is, although the core contains imputations that are themselves undominated, elements of the core do not necessarily dominate imputations outside the core. Thus, from an initial point outside the core, an imputation in the core may be "unreachable" by the process of dominance. Some strands of exchange theory are concerned with the additional structure required to insure that final allocations of economic goods lie in the core of the economy. The interested reader should consult the excellent presentation in Peter Newman, *The Theory of Exchange* (Englewood Cliffs: Prentice-Hall, 1965), esp. chap. 5.

[22] See William F. Lucas, "The Proof That a Game May Not Have a Solution," *Transactions of the American Mathematical Society*, 137 (March, 1969), 219–229.

≤ -10 constitutes a stable set for the game characterized above.

Since the general five-person game is considered in some detail in the Appendix to this paper, the proof is omitted here.[23] Several things should be noted about this result. First, the stable set

$$V = \{\pi(10, 10, 10, x, -30 - x)/ -20 \leq x \leq -10\} \tag{5.1}$$

(where π indicates that the payoffs in the given vector are permuted among the players) is an infinite set. The structure of this game is such that the distribution of losses is determinate only up to an interval. The "winners," however, distribute their gains symmetrically. In the Appendix it is shown that in some instances the interval degenerates to a single point, in which case V is finite.

Second, V is consistent with Butterworth's MPG principle.[24] All $y \in V$ have exactly three positive gainers.

Third, the stable set solution specifies no coalition structure. Of course, the imputations that constitute V are determined in light of coalitional features of the game. The defining properties of a stable set—internal and external stability—depend on the dominance relation which, in turn, depends on the capabilities of coalitions as specified in the characteristic function. Nevertheless, there is no explicit mention of the coalition structure that generates some $y \in V$ as an outcome to the game.

But this is precisely what is of interest to us—a characterization of the coalition structures that can feasibly sustain a $y \in V$. In particular, is there any limitation to the size of the winning coalition when the outcome of $G(n, v)$ is an element of V? In Butterworth's game the answer appears to be no. Any coalition C, where $|C| \geq |MWC|$, can feasibly sustain an imputation in V. (By "feasibly sustain" I mean that the payoffs to the members of C do not, in sum, exceed the worth of C:

$$\sum_{i \in C} y_i \leq v(C).)$$

To demonstrate this point, winning coalitions of different size are considered one at a time. For a MWC ($|C| = 3$) it is easy to check that $x_a + x_b + x_c \leq v(\{a, b, c\})$ where $C = \{a, b, c\}$ is the set of positive gainers. Similarly, it may be confirmed that the grand coalition ($|C| = 5$) feasibly sustains any $y \in V$. This follows directly from the definition of imputation (see (2.2)).

To show that the $y \in V$ may be sustained by a four-person coalition, consider $C = \{a, b, c, d\}$ where $\{a, b, c\}$ is the set of positive gainers and d is the player whose payoff is x in (5.1). Suppose, to the contrary, that a four-person sustaining coalition is infeasible for $y \in V$:

$$10 + 10 + 10 + x > v(C)$$
$$30 + x > 20 \qquad (\text{since } |C| = 4)$$
$$x > -10.$$

But this exceeds the allowable range for x as given in (5.1). There, x is restricted to the closed interval $[-20, -10]$.

It is seen, then, that the imputations in V are sustainable by three-, four-, and five-person coalitions. The size of the winning coalition *cannot* be determined by a stipulation of the von Neumann-Morgenstern stable set. As a consequence, Butterworth's observations constitute a valid critique of the size principle, *to the extent that the size principle depends on a stable set concept of solution.*

A more complete examination must supplement feasibility considerations with motivational features. The relevant question is: Does a MWC (weakly) prefer an arrangement in which it is a proper subset of a winning coalition to the arrangement in which it—and it alone—constitutes itself as a winning coalition? If the answer is affirmative, then the size principle must be abandoned. If negative, the size principle is strongly sustained. As is demonstrated in the Appendix in a more general context, the answer in fact is ambiguous. For games like Butterworth's, however, the answer is unequivocally negative, and the size principle is sustained.

As I have noted, the stable set solution does not incorporate any coalition structure explicitly. To do so is to weigh both coalitional capabilities and coalitional intentions (preferences). In the examination of coalitional *capabilities* above, it was discovered that winning coalitions of *all* sizes are possible. To examine coalitional *intentions* the following behavioral constraints are imposed:

(1) the coalitions that in fact form must receive at least their values as specified by the characteristic function; and
(2) each player has complete strategic flexibility, constrained only by the characteristic function.

Constraint (1) has a simple justification. If

$$\sum_{i \in C} x_i < v(C),$$

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[23] In the Appendix some general results by Gelbaum are employed. See Gelbaum, "Symmetric, Zero-Sum, *n*-Person Games." The reader may also wish to consult von Neumann and Morgenstern, *Theory of Games and Economic Behavior*, pp. 330–338.

[24] It is of historical interest to note that an earlier version of the MPG principle, applied to a slightly different class of games, appears in the work of Vickrey. See his "Self-Policing Properties" and especially his Lemma 6 which applies to constant-sum simple games.

where C is one of the coalitions that actually forms and x is the payoff vector, then some or all members of C can improve their positions at no cost to their coalition partners. Put differently, (1) defines what may be called an *acceptable coalition structure* for the outcome x. It requires, for any $x \in V$, a partition of I, $K = \{K_1, K_2, \ldots, K_m\}$, that satisfies:

$$(5.2) \qquad \sum_{i \in K_j} x_i \geq v(K_j) \qquad j = 1, \cdots, m.$$

With the zero-sum condition (1.3) the m expressions in (5.2) must all be equalities.

Inequality (5.2) is to be sharply distinguished from (3.7)—the definition of the core. In the core,

$$\sum_{i \in C} x_i \geq v(C) \qquad \textit{for all } C \in S,$$

actual or potential; (5.2), on the other hand, applies to a *given* coalition structure K and requires only that *extant* coalitions $K_j \in K$ receive at least their value.[25]

Constraint (2) is essentially a sociologically neutral specification of side-payment activity. Riker, in his initial work, emphasized side-payments in the context of leadership: leaders make contingent payments out of anticipated winnings in an effort to render non-winning protocoalitions winning.[26] There are, however, two problems with this specification. First, a sociologically specific conception of side-payment activity makes demands on the theory of symmetric n-person games that cannot be fulfilled. The theory of games lacks a sociological basis—a fact acknowledged by von Neumann and Morgenstern and especially emphasized by more recent defenders.[27] Leadership, an important element of social conflict and cooperation, has not as yet been formally incorporated in game-theoretic formulations. Second (and I think this is the real value of Butterworth's note), side-payment activity is unrestricted—it may be employed by "nonleaders" in individually rational ways as well. In the absence of sociological restrictions to the contrary, and so long as any player has the coin in which side-payments are made and finds it individually rational to tender offers, there is no reason to restrict the form side-payments may take. Whether the empirical form taken by this activity is that of contingent promises (as in Riker's work) or that of contingent bribes (as in Butterworth's note) is not germane.

Constraint (2), however, cuts two ways. While it forbids arbitrary distinctions of the sort Butterworth accused Riker of making, it also forbids the restriction found in Butterworth's own analysis. Thus, in reference to Table 3 and the discussion pertaining to it, (2) permits Mr. *e* to bid up the price of bribery if it is in his self-interest.

Consider, now, the stable set V given in (5.1). We have observed above that all $y \in V$ may be sustained by a coalition structure $K = [K_1, K_2]$, where $3 \leq |K_1| \leq 5$ (from this point I ignore the coalition-of-the-whole). Let V^* be the subset of V with

$$(5.3) \qquad V^* = \{\pi(10, 10, 10, -10, -20)\}.$$

It is easily demonstrated that a coalition structure K that includes a larger than minimum winning coalition is *not* an *acceptable coalition structure* for any $y \in V - V^*$. For all $y \in V - V^*$ the undetermined payoffs to the "losers," x and $(-30-x)$, lie in the *open* interval $(-20, -10)$. That is,

$$(5.4) \qquad V - V^* = \{\pi(10, 10, 10, x, -30 - x) / -20 < x < -10\}.$$

It is readily checked that any coalition C, where $|C| = 4$, cannot obtain its value, i.e.,

$$\sum_{i \in C} x_i < v(C),$$

contrary to (5.2). *Therefore, if the outcome of the game is in $V - V^*$, only coalition structures which include a MWC are acceptable.* Moreover, it may be shown that for imputations in V^*—and only V^*—a coalition structure with a larger than minimum winning coalition is acceptable. In particular, for $|C| = 4$,

$$\sum_{i \in C} x_i \geq v(C)$$

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[25] Inequality (5.2) bears a slight resemblance to Aumann and Maschler's notion of a *coalitionally rational payoff configuration* (c.r.p.c.). In their early work on the bargaining set, however, they require (5.2) not only for extant coalitions, but also for *subsets* of extant coalitions:

$$(5.2') \qquad \sum_{i \in k} x_i \geq v(k) \qquad \text{for all } k \subseteq K_j,\ K_j \in \{K_1, \cdots, K_m\}.$$

That is, they additionally "assume that a coalition will not form if *some of its members* can obtain more by themselves forming a permissible coalition (emphasis added)." See Aumann and Maschler, "The Bargaining Set," p. 445. Wilson, too, assumes (5.2′) by restricting consideration to MWC's. See Robert Wilson, "Stable Coalition Proposals." In more recent work on the bargaining set, this condition is relaxed. The reader may consult Morton Davis and Michael Maschler, "Existence of Stable Payoff Configurations for Cooperative Games," in *Essays in Mathematical Economics in Honor of Oskar Morgenstern*, ed. Martin Shubik (Princeton: Princeton University Press, 1967), pp. 39–52; and Bazalel Peleg, "Existence Theorem for the Bargaining Set $M_1^{(i)}$," in *Essays in Mathematical Economics*, ed. Shubik, pp. 53–56.

[26] Riker, *Theory of Political Coalitions*, pp. 102–103.

[27] See, for example, Luce and Raiffa, *Games and Decisions*, chap. 9.

if and only if the undetermined parameter x in (5.1) is one of the extreme points of the stated interval, i.e., if and only if the payoff vector is an element of V^* as given in (5.3).

The size principle will be strongly sustained in this game if there is some reason to exclude the $y \in V^*$ as likely outcomes. A careful specification of constraint (2) will permit us to conclude that the outcome of the game is among the elements of a subset of $V - V^*$. Hence, the only acceptable coalition structures are those that contain a *MWC*.[28]

For a given game $G(n, v)$ an outcome is represented by a *payoff configuration* (p.c.) consisting of a vector of payoffs and a coalition structure:

$$(5.5)\qquad (x; K) \equiv (x_1, x_2, \cdots, x_n; K_1, K_2, \cdots, K_m)$$

where the K_j partition the set I of players. If (2.1) is satisfied by the x_i in (5.5) then $(x; K)$ is an *individually rational payoff configuration* (i.r.p.c.). If (5.2) is satisfied by the K_j in (5.5) then $(x; K)$ is said to be an *acceptable payoff configuration* (a.p.c.).[29] Thus any $y \in V^*$, with the coalition structure $K = [K_1, K_2]$ and K_1 containing the three positive gainers and the one "loser" whose payoff is -10, is both an i.r.p.c. and an a.p.c. ($y_i \geq -20$ for all i implies $(y; K)$ an i.r.p.c.;

$$\sum_{i \in K_1} y_i = 10 + 10 + 10 + (-10) = 20 = v(K_1)$$

and

$$\sum_{i \in K_2} y_i = -20 = v(K_2)$$

imply $(y; K)$ an a.p.c. Similarly any $y \in V$ with a coalition structure in which K_1 contains only the three positive gainers is both an i.r.p.c. and an a.p.c.

Consider an i.r.p.c. $(x; K)$ and let s and t be distinct members of I. Member s will be said to have an *objection* against t if there exists a coalition B with $s \in B$ and $t \notin B$, and a payoff vector y, such that

$$(5.6)\qquad \sum_{i \in B} y_i = v(B)$$

$$(5.7)\qquad y_s > x_s$$

$$(5.8)\qquad y_i \geq x_i \qquad i \in B$$

That is, s has an objection against t if a coalition exists which includes s but not t and which can feasibly distribute its winnings so as to render player s better off than, and all other players in the coalition B at least as well off as, in $(x; K)$. In a sense the payoff vector y "weakly dominates" x vis-à-vis the effective set B.

Writing the objection of s against t as $[y^s(t); B]$, player t is said to have a *counterobjection* to $[y^s(t); B]$ if there exists a coalition C, with $t \in C$ and $s \notin C$, and a distribution of spoils z such that

$$(5.9)\qquad \sum_{i \in C} z_i = v(C)$$

$$(5.10)\qquad z_i \geq x_i \qquad i \in C$$

$$(5.11)\qquad z_i \geq y_i \qquad i \in B \cap C.$$

The requirements for a counterobjection are somewhat weaker than those for an objection.In particular, a counterobjector must demonstrate only that there exists one response to $[y^s(t); B]$ that leaves his coalition partners and him *at least as* well off as under $(x; K)$ and $[y^s(t); B]$—(5.10) and (5.11)—the latter only if $i \in B$. An objector, on the other hand, must propose an alternative which accures *strictly* to his benefit, while leaving his coalition partners no worse off than under $(x; K)$—(5.7) and (5.8). If to every objection to an i.r.p.c. $(x; K)$ there is a counterobjection, $(x; K)$ is said to be *efficacious*.[30]

The whole process of objection and counterobjection may be taken as an operationization of constraint (2). In effect, it permits various kinds of side-payment activity, broadly construed, to upset a given coalition structure and distribution of spoils. I am particularly interested in the survivors (if any) of this process—the collection $\{(x; K)\}$ of *acceptable* (constraint (1)) and *efficacious* (constraint (2)) i.r.p.c.'s.

I have shown earlier that the payoff vectors in V^* can be sustained by an acceptable coalition structure containing a coalition of larger than minimum winning size. It can be shown, however, that none of these payoff configurations is efficacious. Consider the i.r.p.c. composed of the payoff vector $x \in V^*$, with $x = (x_a, x_b, x_c, x_d, x_e) = (10, 10, 10, -10, -20)$, and the coalition structure $K = [K_1, K_2] = [\{a, b, c, d\}, \{e\}]$. Mr. e has the following objection against d:

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[28] The demonstration to follow has been heavily influenced by the literature on the bargaining set. However, inasmuch as there are several important differences between my analysis and those listed above (one of which is alluded to in note 25), existence results pertaining to the bargaining set may not apply here. In this paper I do not examine existence problems.

[29] Recall the distinction made in note 25 between an a.p.c. and a c.r.p.c. The former is defined by (5.2), the latter by (5.2′).

[30] The collection of efficacious payoff configurations is very similar to the bargaining set. Indeed, the "objection" and "counterobjection" terms come directly from that literature. There is, however, an important difference (in addition to the one mentioned in note 25). In the bargaining set literature, the objector s and the objectee t must belong to the same $K_j \in K$. This requirement is not imposed here.

$$[y^e(d);\ \{a, b, e\}] \tag{5.12}$$
$$= [(25^-, 25^-, y, -30 - y, -20^+);\ \{a, b, e\}, \{c, d\}].$$[31]

Notice that (5.6) is satisfied:

$$\sum_{i \in \{a,b,e\}} y_i = (25^-) + (25^-) + (-20^+)$$
$$= 30 = v(\{a, b, e\});$$

(5.7) is satisfied:

$$y_e = -20^+ > -20 = x_e;$$

and (5.8) is satisfied:

$$y_i \geq x_i \qquad i \in \{a, b, e\}.$$

Equation (5.12), then, is a legitimate objection to $(x; K)$. Moreover, there is no way for Mr. d to counterobject. That is, there is no i.r.p.c. which excludes the objector e, allows d to keep at least -10 (his payoff in $(x; K)$), and simultaneously provides any set of coalition partners with at least as much as in $(x; K)$ and (if applicable) in $[y^e(d); \{a, b, e\}]$. Since this argument holds for any permutation of x among the players, it follows that no payoff vector in V^* is efficacious. All are subject to objections of the sort given in (5.12).

In addition to eliminating all $x \in V^*$, the principle of efficacy weeds out a number of payoff vectors in $V - V^*$. I shall spare the reader details, but simply observe that *the only i.r.p.c.'s in V that are both acceptable and efficacious are those in which both gains and losses are symmetrically shared by a minimum winning and a maximum losing coalition, respectively*, i.e., the set $[\pi(10, 10, 10, -15, -15); \{a, b, c\}, \{d, e\}]$.[32] Having begun with a symmetric zero-sum game in the first place, this is not really so surprising after all.

Prospects for Generalization

The major intention of this note has been to suggest a deeper justification of the size principle and to correct an error of omission in an earlier essay by Butterworth. In pursuing these goals I have attempted to tie assertions about coalition structure to a theory of solutions of n-person games. While the analysis has been none too rigorous, one is still left with a favorable impression of the size principle's validity. The prospects for generalization, however, are somewhat less promising.

The principal problems are those of existence and uniqueness. I have earlier referred to this problem as it relates to stable sets and have observed that this difficulty initially led Riker to bypass solution theory altogether. Although existence results have been provided for stable sets in an impressive variety of games, the results, to a non-game theorist, appear highly incomplete and idiosyncratic.[33]

Turning from stable sets to the acceptable and efficacious payoff configuration concept, I believe the prospects for generalization are better (whenever a stable set exists), though I cannot provide any existence theorem at this time. Existence results for the bargaining set, to which acceptable and efficacious payoff configurations seem closely related, may be of some utility on this score.[34]

One message, in any event, is clear. It is unlikely that much can be said with confidence about coalition structure in the absence of analytically viable (and perhaps sociologically rich) solution concepts. From Butterworth's initial research and the analysis presented in the first three sections of this paper, it appears that minimum winning coalitions constitute *unstable* equilibrium points in n-person zero-sum games. That is, there appear to be forces in the coalition formation process that drive winning coalitions toward minimal size, but fail to keep them at that point. If, however, the usual assumptions about n-person zero-sum coalition processes are supplemented with assumptions about coalition intentions and capabilities, there are good reasons to expect minimum winning coalitions in all but the most extreme instances. This proposition has been the focus of the latter part of this paper and is treated more fully in the Appendix.

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[31] By 25^- I mean slightly less than 25, and similarly for -20^+. Also y is an undetermined parameter in the closed interval $[-20, -10]$.

[32] It must be assumed throughout that payoffs are completely divisible.

[33] On quota games, see L. S. Shapley, "Quota Solutions to n-Person Games," in *Contributions to the Theory of Games, II*, ed. H. W. Kuhn and A. W. Tucker (Princeton: Princeton University Press, 1953), pp. 343–359. On (n, k)-majority games, see Raoul Bott, "Symmetric Solutions to Majority Games," in *Contributions to the Theory of Games, II*, ed., A. W. Kuhn and A. W. Tucker (Princeton: Princeton University Press, 1953), pp. 319–329, and D. B. Gillies, "Discriminatory and Bargaining Solutions to a Class of Symmetric n-Person Games," in *Contributions to the Theory of Games, II*, ed. H. W. Kuhn and A. W. Tucker (Princeton: Princeton University Press, 1953), pp. 325–342. On symmetric, zero-sum games, see Gelbaum, "Symmetric, Zero-Sum, n-Person Games."

[34] In addition to the articles cited in note 25, the reader may consult Michael Maschler, "Stable Payoff Configurations for Quota Games," in *Advances in Game Theory*, ed. Melvin Dresher, L. S. Shapley, and A. W. Tucker (Princeton: Princeton University Press, 1964), pp. 477–500. As well, Wilson's important work on the relationship between stable sets (when they exist) and bargaining sets of various kinds ("Stable Coalition Proposals") is especially enlightening. In this 1971 paper, Wilson provides several subtle criticisms of the bargaining set as originally formulated by Aumann and Maschler and charts some interesting directions for accommodating these criticisms.

One last comment is in order. It is extremely unfortunate that much of the research on and criticism of the size principle is empirical in nature. There are many *theoretical* subtleties yet to be explored.[35] Butterworth's note is a healthy sign that debate on theoretical issues will continue.

Appendix

In this appendix we investigate stable-set properties and coalition structure for the general five-person zero-sum game characterized as follows:

Table 4. A General Five-Person Zero-Sum Game

$v(C) =$	whenever $\|C\| =$
0	0
$-\beta$	1
$-\alpha$	2
α	3
β	4
0	5

It is convenient to convert this game to the standard $[-1, 0]$ normalization:

Table 5. The General Game Normalized

$v(C) =$	whenever $\|C\| =$
0	0
-1	1
$-\gamma$	2
γ	3
1	4
0	5 where $\gamma = \alpha/\beta$

An early analysis of this game is found in von Neumann and Morgenstern.[36] Discussion here is organized into five sections, depending on the value of the free parameter γ:

(a) $\gamma > 2$

(b) $\gamma = 2$

(c) $3/4 < \gamma < 2$

(d) $\gamma = 3/4$

(e) $\gamma < 3/4$

Before beginning, some notation and results from Gelbaum are needed.

Since $G(n, v)$ is a symmetric game, $v(C)$ depends only on $|C|$. If $|C| = q$, I write $v(C)$ as v_q, i.e., v_q is the value of a coalition of size q. Define

$$a = \sup \{v_q/q : q = 1, 2, \cdots, n\};$$

a is the largest pro-rata share provided by any coalition. Let p be the smallest coalition that provides the largest pro-rata share, a:

$$p = \inf \{q : v_q/q = a\}.$$

Finally, let

$$n = mp + r \qquad 0 \leq r < p,$$

where n is the number of players in the game and m is (for our purposes) an integer.

Two of Gelbaum's results, his Theorems 3.1 and 3.2, are germane.[37]

THEOREM 1: *If $r = p-1$ and $v_{p-1} = -(p-1)$, then the set of permutations of the vector*

$$\left(a, a, \cdots, a, \frac{-pma}{r}, \cdots, \frac{-pma}{r}\right),$$

where pm components equal a and r components equal $-pma/r$, is a finite, symmetric stable set of $G(n, v)$.

THEOREM 2: *If $r = p-1$ and if $v_{p-2} = -(p-2)$, then the set of permutations of the vector $(a, a, \cdots, a, x_1, x_2, \cdots, x_r)$, $\Sigma x_i = -pma$ and $x_i \geq -1$, where pm components equal a, is a symmetric (not necessarily finite) stable set of $G(n, v)$.*

(a) $\gamma > 2$

This case constitutes a violation of the superadditivity of v. Superadditivity requires, for all disjoint coalitions B and C, that

$$v(B \cup C) \geq v(B) + v(C).$$

For $|B| = 3$ and $|C| = 1$, this becomes (see Table 5)

$$1 \geq \gamma - 1$$

or

$$\gamma \leq 2,$$

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[35] For example, little interest has been expressed in the question of coalition structure in non-zero-sum games (although Riker's work is often criticized for his zero-sum assumption). A natural starting place is a consideration of the class of *simple games*—games in which a coalition is either winning or losing depending on whether its size exceeds some minimal number, k:

$$\begin{aligned} v(C) &= 1 \text{ if } |C| \geq k \\ &= 0 \text{ otherwise.} \end{aligned}$$

See the work of Shapley and von Neumann and Morgenstern cited above for expositions on this class of games.

[36] *Theory of Games and Economic Behavior*, pp. 330–338.

[37] Gelbaum, "Symmetric, Zero-Sum, *n*-Person Games," pp. 96–100.

contrary to supposition.

(b) $\gamma=2$

Referring to the definitions of a, p, and n, and consulting Table 5, it is seen that

$$a = \gamma/3 = 2/3$$
$$p = 3$$

and, since $n=5$ and $n=mp+r$,

$$m = 1, \quad \text{and}$$
$$r = 2.$$

Moreover, $r=p-1$ and $v_{p-1}=-(p-1)$. The hypotheses of Theorem 1 are satisfied. Thus,

$$\begin{aligned} W &= \{\pi(\gamma/3,\ \gamma/3,\ \gamma/3,\ -\gamma/2,\ -\gamma/2)\} \\ &= \{\pi(2/3,\ 2/3,\ 2/3,\ -1,\ -1)\} \\ &\qquad (\text{since } \gamma = 2) \end{aligned}$$

is a finite, symmetric stable set.

Acceptability: It appears that coalition structures containing three-player and four-player winning coalitions are acceptable. In the four-player case, for instance, with the three positive gainers and one of the "losers,"

$$\begin{aligned} \sum x_i &= 2/3 + 2/3 + 2/3 + (-1) \\ &= 1 \\ &= v_4 \end{aligned}$$

in accord with (5.2).

Efficacy: Every i.r.p.c., with payoff vector in W and coalition structure including winning coalitions of size three or four, is efficacious. That is, for all payoff vectors in W any objection may be countered, even if the winning coalition is larger than necessary. In this case, then, the size principle does not follow. It should be noted, however, that this case is extremely degenerate—all two-person coalitions are *flat* since $v_2=2v_1$. In a coalition structure with a four-man winning coalition, the excluded player is not additionally penalized for "going it alone." His situation would not be improved by participating in a maximum losing coalition.

(c) $3/4<\gamma<2$

This is the class in which Butterworth's game falls (where $\gamma=3/2$ when the game is normalized). Classes (b) and (c) are cases in which the MWC is the unique per-capita wealthiest coalition. Referring to the definitions of a, p, and n, and again consulting Table 5, we have

$$a = \gamma/3$$
$$p = 3 \qquad [\text{since } v_3/3 = \gamma/3 > 1/4 = v_4/4]$$
$$m = 1$$
$$r = 2.$$

Here $r=p-1$ and $v_{p-2}=-(p-2)$ so that the hypotheses of Theorem 2 are satisfied. Hence the set

$$V = \{\pi(\gamma/3,\ \gamma/3,\ \gamma/3,\ x,\ -\gamma - x)/ \\ -1 \le x \le 1 - \gamma\}$$

is a symmetric stable set. Since $\gamma<2$, the interval for x, $[-1, 1-\gamma]$, is nondegenerate; thus, V is infinite.

Acceptability: While all $y \in V$ with a *MWC* in the coalition structure are acceptable, only a subset of V, $V^*=\{\pi(\gamma/3, \gamma/3, \gamma/3, 1-\gamma, -1)\}$, is acceptable in the presence of a four-person winning coalition.

Efficacy: With an analysis directly parallel to that in the text for Butterworth's game, it is demonstrable that no i.r.p.c., with $y \in V^*$ and a coalition structure containing an overlarge winning coalition, is efficacious. The only acceptable and efficacious i.r.p.c.'s are those in which a minimal winning and maximal losing coalition symmetrically share gains and losses, respectively:

$$\{\pi(\gamma/3,\ \gamma/3,\ \gamma/3,\ -\gamma/2,\ -\gamma/2)\}.$$

In this case, only *MWC*'s are likely to be formed.

(d) $\gamma=3/4$

In this class three- and four-person coalitions are equally wealthy on a per-capita basis. In terms of Gelbaum's notation,

$$a = 1/4$$
$$p = 3$$
$$m = 1$$
$$r = 2.$$

Again the hypotheses of Theorem 2 are satisfied, and

$$V = \{\pi(1/4,\ 1/4,\ 1/4,\ x,\ -3/4 - x)/ \\ -1 \le x \le 1/4\}$$

is a stable set.

Acceptability: All $y \in V$ are acceptable in the presence of a *MWC*. In the presence of a four-player winning coalition, the only acceptable payoff vectors are in $V^* \subset V$ where $x=-1$ or $1/4$:

$$V^* = \{\pi(1/4,\ 1/4,\ 1/4,\ 1/4,\ -1)\}.$$

Efficacy: For a $y \in V^*$, say $y = (1/4, 1/4, 1/4, 1/4, -1)$, and the coalition structure $K = [\{a, b, c, d\}, \{e\}]$, consider the objection of e against c:

$$[y^e(c); B] = [(7/8^-, 7/8^-, -3/4 - y, y, -1^+); \{a, b, e\}].$$

No counterobjection exists which excludes e. Hence, no $y \in V^*$ is efficacious when the winning coalition is larger than necessary.

Now consider another subset of V, V^{**}, in which gains and losses are symmetrically shared, i.e., $V^{**} = \{\pi(1/4, 1/4, 1/4, -3/8, -3/8)\}$. A coalition structure containing a MWC consisting of the positive gainers is acceptable. Is it efficacious? The answer appears to be yes. Any objection by, say, d may be countered by e so long as the payoffs are completely divisible. Thus the size principle is sustained for this category of zero-sum symmetric games.

(e) $\gamma < 3/4$

In this case four-person coalitions are per-capita wealthiest, i.e. $(1/4 =) v_4/4 > v_3/3 (< 1/4)$. I shall not pursue this possibility here, though it seems unlikely that MWC's form. This observation, however, is not inconsistent with Riker's derivation of the size principle, since $\gamma < 3/4$ implies a positively sloped characteristic function.[38]

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[38] See Riker, *Theory of Political Coalitions*, Appendix I. Notice that in case (d) above, the characteristic function may be positively sloped with the size principle still obtaining.

Comment on Shepsle's "On the Size of Winning Coalitions"

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Professor Shepsle has contributed a lucid and stimulating discussion of coalitions and solutions, but his analysis cannot be used to resolve the problem of the size of winning coalitions. His conclusion that "there are good reasons to expect minimum winning coalitions in all but the most extreme instances" is therefore unwarranted, although his comment that "there are many *theoretical* subtleties yet to be explored" is fully substantiated.

Shepsle begins by ably summarizing my critique of the size principle. In *n*-person, zero-sum games with perfect information, there are situations in which a player who does not belong to a minimum winning coalition will try to bribe his way into that coalition. He will find it rational to use side-payments to increase the payoffs of each member of that minimum winning coalition in return for his joining them. Since each member of the minimum winning coalition would gain by accepting that deal, they would agree to expand their group to a larger-than-minimum size. If we allow only one of the players who does not belong to the minimum winning coalition to engage in side-payments, that is the end of the story; a larger-than-minimum winning coalition results. With competitive bribery, however, the result is simply indeterminate. Maybe the winning coalition will be of minimum size; maybe it will be larger than minimum. All we can know for sure is that the number of players whose payoffs will be greater than zero will be at most the number of players required to form a minimum winning coalition.

A quick word about that competitive bribery: Shepsle correctly notes that "there are very real incentives for *e* to bid up the price of bribery." Yes, there are, but they have limits; and those limits restrict *e*'s bribe function to the same range that *d*'s has. That range, in the game we keep using, is 20. It cannot be 25, as Shepsle would have it, because there are no imputations dominating those associated with a minimum winning coalition/maximum losing coalition situation that provide *e* with that many points to spend. Shepsle's suggested imputation (12, 12, 12, −20, −16) (see his Table 3, "A Bribe Response") would do so, *if* it indeed reflected *e*'s only option. It does not do so, of course, since as Shepsle immediately points out it is strategically irrelevant to *e*. That is because the coalition $\{d, e\}$ is effective for (10, 10, 10, −15, −15), which dominates the suggested payoff vector. The strategic analysis of *d*'s situation must be, *mutatis mutandis*, the strategic analysis of *e*'s.

Shepsle's main thrust, however, is that neither Professor Riker nor myself ties our analyses explicitly to "a *formal solution*." Doing so would certainly improve our theoretical leverage on coalition behavior. Because he recognizes some critical difficulties with rigorous solution concepts, he cannot yet remedy this shortcoming, and so undertakes "a more modest inquiry" into an "heuristically interesting stable set" (p. 511).

That inquiry, however, is too restricted to achieve his intentions. He begins by identifying a solution (in the sense of a von Neumann-Morgenstern stable set) to our five-person game, namely, "the set *V* of permutations of the vector (10, 10, 10, x, $-30-x$) with $-20 \leq x \leq -10$." He then asks whether there is "any limitation to the size of the winning coalition when the outcome of [the game] is an element of *V*," and concludes that the answer is No; "the size of the winning coalition *cannot* be determined by a stipulation of the von Neumann-Morgenstern stable set" (p. 512).

That conclusion, he notes, rests upon "feasibility considerations." Does it change when some "motivational features" are specified? He concludes that "in a more general context, the answer in fact is ambiguous. For games like Butterworth's, however, the answer is unequivocally negative, and the size principle is sustained" (p. 512). But he can reach that conclusion only because his analysis misses a crucial point of the MPG principle.

He first looks at a coalition structure's "acceptability." Since he confines his examination to *V*, he is concerned only with bargaining by players outside the minimum winning coalition *when the payoffs to the positive gainers are fixed*, and fixed *a priori*. No wonder, then, that he finds only one extreme case in which those gainers would shift from a situation in which the size of the winning coalition = the number of positive gainers = a minimum winning coalition: From the losers' point of view, the game is rigged. When the winnings of the positive gainers are specified, that is, so is the coalition structure, *if* we also specify the distribution of losses. If, as in Shepsle's example, we have not done the latter, then there is only one possible winning coalition that is larger than a minimum one—the case in which *x* (*d*'s payoff) takes an extreme value. As Shepsle's discussion (equation (5.4)) shows clearly, this is because the value of a winning coalition will have to be achieved by adding x to the value of a minimum coalition. The value of x must therefore reflect precisely the difference in value between a minimum winning coalition and a larger-than-minimum one. (And that, of course, is just how we derive the range for x.)

The analysis of "acceptability," then, depends on fixing the payoffs to the positive gainers. The analysis of the MPG principle depends on being able to alter them. But this exercise was, after all, to be of heuristic value; on, then, to the examination of "efficacy." Shepsle contends that this criterion "will permit us to conclude that the outcome of the game is among the elements of a subset of [imputations in which x does not take an extreme value]. Hence, the only acceptable coalition structures are those that contain a [minimum winning coalition]" (p. 514).

But here we again find a "fix," not in the game itself this time but in the specification of "efficacy." This fix takes the form of a curious double standard in the definitions of "objection" and "counterobjection." Shepsle sets up his criteria such that "a counterobjector must demonstrate only that there exists one response [to an objection] that leaves his coalition partners and him *at least as* well off as under [the 'current' payoff configuration, $(x; K)$] and [the situation proposed by the objector]—(5.10) and (5.11)—the latter only if [an actor would be a member of the coalition proposed by the objection]. An objector, on the other hand, must propose an alternative which accrues *strictly* to his benefit, while leaving his coalition partners no worse off than under [the 'current' imputation]—(5.7) and (5.8)" (p. 514).

But who are "his coalition partners"? For the counterobjector they cannot include the objector, and for the objector they cannot include the counterobjector. These criteria, that is, replace the traditional game theoretic assumption of symmetry among the players with restrictions on who can join with whom. The effect of this restriction is to confine *e*'s strategic alternatives to a choice between -20 and -20 plus a little bit. He either stays the sole loser or uses the limit of his bribe function to get two winners to include him in a minimum winning coalition. The central strategic problem is, in a real sense, lost when *e*'s other alternatives (such as cooperating with *b*, *c*, and *d*, or even just with *d*) cannot be simultaneously evaluated.

The more critical restriction, however, concerns how objectors and counterobjectors calculate their bribes. An objector, *e*, is permitted to look to his promised gains, but a counterobjector, *d*, is not allowed to consider his threatened losses. Hence *e* can spend nearly 25 points, but *d* can take nothing from his pocket to forestall his approaching ruin. Shepsle holds that requiring a counterobjector to demonstrate a counterproposal that leaves him at least as well off is "somewhat weaker" than requiring an objector to demonstrate a proposal that makes him better off. Superficially that might be true; but in effect the constraints on counterobjecting are stronger than those on objecting. The objector, *e*, is receiving the minimum payoff possible in this game; he has no place to go but up. A counterobjector, on the other hand, always has something to lose; and yet his strategic behavior is somehow not supposed to reflect that fact. The effect of this restriction is to impose stability on the game by sharply restricting the range of rational bargaining behavior.

But even if we accept these asymmetric restrictions, it looks to me like *d* can counter *e*'s objection. Starting with the vector

$$\begin{aligned} x &= (x_a, x_b, x_c, x_d, x_e) \\ &= (10, 10, 10, -10, -20) \end{aligned}$$

and the coalition structure

$$K = [K_1, K_2] = [\{a, b, c, d\}, \{e\}]$$

Shepsle notes an objection by *e* against *d*:

$$\begin{aligned} (5.12) \quad & [Y^e(d); \{a, b, e\}] \\ &= [(25^-, 25^-, y, -30 - y, -20^+); \\ & \quad \{a, b, e\}, \{c, d\}]. \end{aligned}$$

"There is," he claims, "no way for Mr. *d* to counterobject. That is, there is no i.r.p.c. [individually rational payoff configuration] which excludes the objector *e*, allows *d* to keep at least -10 (his payoff in $(x; K)$), and simultaneously provides any set of coalition partners with at least as much as in $(x; K)$ and (if applicable) in $[Y^e(d); \{a, b, e\}]$" (p. 515). But it looks as though *d* could counter the objection with this:

$$\begin{aligned} & [Y^d[Y^e(d); \{a, b, e\}], \{a, c, d\}] \\ &= [(25, y, 15^-, -10^+, -30 - y); \\ & \qquad \{a, c, d\}, \{b, e\}]. \end{aligned}$$

There are two possible criticisms of this proposed counterobjection. The first is that in saying "a counterobjector must demonstrate only that there exists one response to [an objection] that leaves his coalition partners and him *at least as* well off . . . " Shepsle means "his coalition partners" to refer to *d*'s partners under the "current" (i.e., $(x; K)$) payoff configuration. In this case that would mean including *b*, which the proposed counterobjection does not do. This criticism cannot be correct, however, or Shepsle would not have claimed that *d* possesses no counterobjection that "provides *any set* of coalition partners with at least as much as in $(x; K)$ . . . " (emphasis added). If this criticism were correct, moreover, it could not be true that the set

$$\left\{\pi(10, 10, 10, -15, -15); \{a, b, c\}, \{d, e\}\right\}$$

is efficacious. If d objected to c, for example, proposing to bribe a and b to form

$$[(15^-, 15^-, y, -15^+, -15 - y);$$
$$\{a, b, d\}, \{c, e\}],$$

c could not counterobject in a way that gave a and b at least 15^- and himself at least 10.

The second criticism is that the proposed counterobjection itself rests upon a minimum winning coalition. But what of that? We find nothing in the criteria of "efficacy" specifying that a coalition is efficacious only if the counterobjections associated with it have the same coalition structure that it does. *Any* counterobjection in this situation would have to rest upon a three-person coalition, *owing to the restrictions on counterobjecting that we noted above.* Because d is prohibited from digging deeper into his pocket to compete with e's bribe, he must either find a way to redistribute the fixed capital of the four-person coalition or generate more revenue by paring its size. The only way he could redistribute the capital so as to make his counterobjection "weakly dominate" the objection would be to include e, and he is also prohibited from doing that. He is left, then, with the paring option. The effects of the restrictions on counterobjecting are clearly seen here; because he cannot take account of threatened losses and cannot offer to cooperate with an objector, a counterobjector finds himself in a strategic situation that differs greatly from that in which we first placed him. In this case, a five-person coalition game has been transformed into a four-person majority game.

Using "the acceptable and efficacious payoff configuration concept," then, does not determine for us the size of winning coalitions. In that respect, Professor Shepsle's paper leaves us where we were: knowing an upper bound on the number of positive gainers, but not knowing the size of the coalition in which they will be found. He has formalized many of the subtleties of the problem, however; and his focus on the bargaining dynamics of coalition formation appears to be the most promising road to grounding a theory of coalition size in formal solution theory.

Minimum Winning Coalitions Reconsidered: A Rejoinder to Butterworth's "Comment"

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The purpose of my note, "On the Size of Winning Coalitions," was two-fold. First, I wished to address the notion of *competitive bribery*, underscoring its conceptual relevance to a theory of side-payments and exploiting its theoretical power in a less arbitrary manner than had Butterworth in his "Research Note."[1] Since there is still some apparent confusion on this point, additional details are presented below. The second purpose of my note was to present a general strategy for dealing with some of the problems surrounding the determination of winning coalition size, as well as to argue in behalf of a specific tactic. The general strategy still appears appropriate; the specific tactic, however, has been subjected to some valid criticisms, I believe, in Butterworth's "Comment." It needs, therefore, to be re-examined.

Butterworth's original contribution in this debate is the discovery of the existence of an *individually rational bribe*—a form of side-payment which allows an apparent loser to attempt to bribe his way into an otherwise minimal winning coalition so as to minimize his own losses. This process occurs at no apparent expense to the members of the formerly minimal winning coalition (*MWC*), and may, if for some reason the other losers do not respond appropriately, redound to their positive benefit. In the particular illustration employed by Butterworth (the characteristic function of this game appears in my paper elsewhere in this volume), in which a three-man winning coalition $\{abc\}$ and a two-man losing coalition $\{de\}$ are organized so as to receive the payoff

$$(1)\quad (x_a, x_b, x_c, x_d, x_e) = (10, 10, 10, x, -30-x)$$

(where $-10 \geq x \geq -20$), it is indeed possible for one of the losers, say d, to offer an amount δ to $\{abc\}$ so that

$$(2)\quad \left(5+\frac{\delta}{3},\ 5+\frac{\delta}{3},\ 5+\frac{\delta}{3},\ 5-\delta,\ -20\right)$$

dominates (see my definition (3.3)) (1) vis-à-vis the effective set $\{abcd\}$. Clearly $\{abcd\}$ is effective since the sum of the payoffs to its members just equals $v(\{abcd\})$. To establish dominance, then, coalitional preference must be demonstrated. Each of a, b, and c (weakly) prefers (2) to (1) if $5+(\delta/3)\geq 10$. Thus, dominance requires $\delta \geq 15$. Similarly, d (weakly) prefers (2) to (1) if $5-\delta \geq x$. But this simply requires $\delta \leq 5-x$. Combining these two requirements, (2) dominates (1) vis-à-vis $\{abcd\}$ if and only if $15 \leq \delta \leq 5-x$. In order for a δ with these properties to exist, it must be the case that $x \leq -10$, which is precisely what (1) requires. Thus, an individually rational bribe exists.

The existence of an individually rational bribe for d (or, for that matter, for e—the argument holds for him as well) suggests an incentive for d to break up an extant payoff configuration $[(x_a, x_b, x_c, x_d, x_e);\ \{abc\},\ \{de\}]$. But surely this prospect must send terror through the heart of e. As Butterworth notes, e may attempt to lure d back into the original maximal losing coalition $\{de\}$. According to Butterworth's line of reasoning, "equilibrium" obtains with the imputation $(10, 10, 10, -10, -20)$ and an *indeterminate* coalition structure. With this imputation all actors are indifferent between either of two coalition structures: $[\{abc\},\ \{de\}]$ and $[\{abcd\},\ \{e\}]$. As a consequence Butterworth claims to have *demonstrated* the invalidity of the size principle: *MWC*s do not, of necessity, form.

My principal quarrel is not so much with the substance of the conclusion (though my agitation with this is expressed shortly) as it is with the claim of demonstration. I argued in my paper that Butterworth imposes a curious sort of asymmetry in his use of the bribery notion: he permits one of the losers to attempt to bribe his way into a *MWC*, allowing the other loser only the option of trying to attract his former partner back into a maximal losing coalition. But remember now, e (in the example above) stands to lose $v(\{i\}) = -20$, the worst of all possible worlds, in the event that any of d's bribe offers are tendered and accepted by the *MWC*, $\{abc\}$. He will, therefore, desperately try anything the rules permit to avoid this prospect. *And because he is faced with the worst of all possible worlds, his rational options are indeed greater than d's.* Contrary to Butterworth's remarks in the third paragraph of his "Comment", while d and e have the same "feasible set" of strategies, i.e. the game is symmetric, because of their different strategic positions *after* d makes his bribe, the *rational* subsets of their respective feasible sets differ.

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[1] Robert Lyle Butterworth, "A Research Note on the Size of Winning Coalitions," *Americal Political Science Review*, 65 (September, 1971), 741–745.

In particular, Mr. *e* may *rationally* expend up to 25 "points" in an effort to outbid *d* for a place in a previously *MWC* (20 he would otherwise lose if *d* succeeds in bribing his way into the *MWC* plus 5 he would win if he successfully outbids *d*).[2] Any expenditure in that range leaves *e* at least as well off as the situation in which *d* succeeds in bribing his way into the *MWC*. As I demonstrated in my earlier paper, consideration of this prospect, and the concomitant bidding war, leads to the conclusion that attempts by a loser to bribe his way into a *MWC* can only redound to the positive *advantage* of the previous *MWC* and to the positive *disadvantage* of the previous maximally losing coalition. In other words, bidding wars turn out to be quite unprofitable for the bidders who, eventually realizing this, revert to their original maximally losing coalition.

This is the truth of the size principle—that in zero-sum, perfect information games with side-payments, there are contextual forces that drive winning coalitions to minimal winning size (all other things equal).[3] Butterworth's "demonstration" does nothing to contradict this *part* of the size principle.

There is a second part of the size principle, however, that Butterworth's analysis does challenge. In fact, Butterworth's is the first analysis I know of that accounts for the instability of zero-sum games in the context of coalition theory (as opposed to solution theory). It is this feature of his analysis that I attempted to explicate in my paper. As I showed, after the series of bribes and counterbribes (under my more general conception of bribery as side-payment activity), resulting in a resurrection of minimal winning and maximal losing coalitions, there are no contextual forces to halt the process at that point.

It is well-known in solution theory that *n*-person zero-sum games are inherently unstable: their cores are empty. Hence no payoff vector is invulnerable to strategic assault. Butterworth's analysis and my alteration, then, cast the size principle in a slightly different light. They suggest that moving the focus of one's theory from payoff vectors to coalition configurations—Riker's initial research strategy (see footnote 12 in my earlier paper)—buys some insight about equilibrium; the purchase, however, is less complete and satisfactory than one might originally have anticipated. Coalition configurations involving *MWC*s are *unstable equilibrium points* in a coalition theory of conflict phenomena.

The notion of competitive bribery was a beginning point of my earlier analysis. It satisfactorily establishes, I believe, the thesis that Butterworth only hints at, *viz*., that *MWC*s constitute unstable equilibria. While my analysis does not, in any sense, disprove the size principle, it does, unfortunately, raise questions about its empirical utility. If, as I suggested above, contextual forces exist that drive winning coalitions to minimal winning size but fail to hold them there, then the usefulness of the size principle as a predictive tool related to outcomes in empirical settings is substantially limited.[4]

In the second part of my paper I suggest a general approach to the problem of coalitional stability in *n*-person games. The approach amounts to asking whether there are additional features in the payoff structure that allow one to draw inferences about coalition size. My specific tactic was to impose two *a priori* restrictions on coalitional divisions of the spoils. First, each *extant* coalition must receive at least its value. This I called *acceptability* (see (5.2) and the discussion following) with which there are no apparent difficulties. The second *a priori* restriction, called *efficacy* (see (5.5) through (5.11)), requires that no coalition structures to which uncountered objections exist survive. As Butterworth correctly observes in his "Comment", efficacy does not serve to further restrict the set of acceptable payoff configurations,

$$\left[(x;\,K)\colon \sum_{i\in K_j} x_i \geq v(K_j)\right].$$

Let us see, then, what acceptability alone does.

From the von Neumann-Morgenstern solution of the game that Butterworth employs, the following stable set emerges:

$$V = \{\pi(10,\,10,\,10,\,x,\,-30-x)\colon -20 \leq x \leq -10\} \tag{3}$$

The following three sets of payoff configurations are both acceptable and compatible with (3):

$$A = [\pi(10,\,10,\,10,\,x,\,-30-x);\,(K_1,\,K_2)\colon |K_1| = 3,\ |K_2| = 2,\ -20 < x < -10]$$

$$B = [\pi(10,\,10,\,10,\,-10,\,-20);\,(K_1,\,K_2)\colon |K_1| = 3,\ |K_2| = 2]$$

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[2] Although I did not consider the prospect in my paper, Mr. *e* has yet another strategic alternative, namely to attempt to convince two of the previous winners to form a new *MWC*. In this case he has 30 points with which to play—20 he would otherwise lose plus 10 he would receive as a member of a *MWC*. More on this shortly.

[3] It must be noted, of course, that the *ceteris paribus* assumption may be inappropriate. Thus, despite the existence of forces that produce *MWC*s, these forces and their effects may well pose empirical problems.

[4] Insights from the size principle about the *process* by which conflicts are resolved, however, may be quite rich.

$$C = [\pi(10, 10, 10, -10, -20); (K_1, K_2): |K_1| = 4, |K_2| = 1]$$

A is the set of solution imputations in which a maximally losing coalition forms with its members sharing the benefits of their coalition (i.e., the "benefits" of a superadditive characteristic function) in some *nondegenerate* fashion. In *B*, on the other hand, one of the losers reaps all of the gains from coalescing with the other loser. In *C*, finally, one of the losers buys his way into the *MWC* à la Butterworth. In the context of solution theory, the size principle predicts some $[x; K] \in A \cup B$.[5] Butterworth, on the other hand, suggests an $[x; K] \in B \cup C$, while acceptability implies only an $[x; K] \in A \cup B \cup C$.

As I have pointed out via my competitive bribery argument, there is no sound reason for accepting Butterworth's *argument*. There may, however, be good reason to accept his *conclusion*—that the size of winning coalitions is still indeterminate. Disregarding *B*, which is bizarre (though consistent with the size principle), acceptability implies an element of $A \cup C$. In order to endorse the size principle, one must have formal justification for ignoring the elements of *C*. Efficacy, as Butterworth demonstrates, does not qualify as a compelling justification. But are there others? I suggest two, though I do not provide accompanying formalizations.[6] Each suggests a different class of empirical situations in which *MWC*s are to be expected.

Any *MWC*, {*abc*}, may expect *at least* $v(\{abc\}) = 30$, to continue with the example we have been using. Should players *d* and *e* fail to coalesce, then

$$\sum_{i \in \{abc\}} x_i > 30.$$

Unless a briber can guarantee {*abc*} more than it is objectively worth—a contingency shown to be impossible because of competitive bribery—it stands to reason that {*abc*} will follow a course of action that at least admits the possibility that *d* and *e* will not band together. Such seems to have been the course followed by the British in colonial Africa. On this basis, it appears there are some costs (disincentives) to admitting bribers to a *MWC*. The elements of *C*, then, are unlikely to occur if the premise is accepted.

A second empirical contingency in which only the elements of *A* are to be expected requires only a modicum of risk aversion. In games with minimal self-policing properties,[7] a *MWC* may correctly fear the appeals of a briber, for they provide both opportunity and motive for the losers left out in the cold to attempt to form a new *MWC* (see footnote 2). Indeed, experimental evidence suggests that players in a winning coalition often are willing to "shave their quotas" in order to prevent just such a circumstance.[8]

Each of these contingencies suggests that there often is additional structure in empirical situations to produce a stronger implication of the size principle—that *MWC*s are *stable* outcomes of *n*-person zero-sum games. Butterworth's initial essay is substantially correct in its conclusion that there is not quite enough analytical closure in *n*-person game theory to yield the size principle. By failing to incorporate a more general notion of bribery, however, his argument fails to detect the important kernel of truth contained in the size principle. Payoff configurations containing a *MWC* are unstable equilibrium points of *n*-person games; additional analytical structure appears to render those points stable.[9]

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[5] We can probably eliminate *B* entirely for it is highly improbable that any loser would willingly permit a coalition partner to secure all the "gains from trade."

[6] There is a third, less compelling, probabilistic justification for expecting an element of *A* to emerge. Barring unusual probability distributions, all of the probability measure on $A \cup B \cup C$ falls on *A*. *B* and *C*, being finite sets, are sets of measure zero.

[7] See William Vickrey, "Self-Policing Properties of Certain Imputation Sets," in *Contributions to the Theory of Games, IV,* ed. A. W. Tucker and R. D. Luce (Princeton: Princeton University Press, 1959), pp. 213–246.

[8] See William H. Riker, "Bargaining in a Three-Person Game," *American Political Science Review,* 61 (September, 1967), 642–656. Relevant discussion is found at pp. 651–53.

[9] It follows from my comments in footnote 25 of my previous essay that if condition (5.2′) is imposed, instead of (5.2), in which case all extant coalitions and *all subsets of those coalitions* receive their value, then the size principle is an immediate consequence. Professor Richard McKelvey has recently called this fact to my attention and plans to report a formal demonstration of this result.

The Paradox of Not Voting: A Decision Theoretic Analysis*

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Introduction

During the past decade students of the electoral process have made increasing use of rational choice models in their research.[1] But whatever their ability to account for such general phenomena as candidate strategy and electoral outcomes, many believe that rational choice models do not provide a satisfactory explanation for the most basic political decision in a democratic system: to vote or not to vote. The purpose of this paper is to show one means of rescuing rational choice theorists from this embarrassing predicament.

In his seminal work, Anthony Downs argued that a citizen would vote rather than abstain if his vote value exceeds zero, where the vote value equals the utility difference between the two candidates discounted by the citizen's probability of affecting the outcome.[2] As Downs remarked, the latter probability is small, so the vote value generally could not be expected to exceed any costs associated with voting. Thus, Downs left the decision to vote somewhat up in the air, resorting to extra theoretic factors to explain widespread voting.[3] Gordon Tullock further popularized the notion that the decision to vote appeared irrational on the face of it.[4]

Understandably, some political scientists find the Downs-Tullock argument disturbing. Professors Riker and Ordeshook comment that "much recent theorizing about the utility of voting concludes that voting is an irrational act . . . it is certainly no explanation to assign a sizeable part of politics to the mysterious and inexplicable world of the irrational."[5] But the Riker and Ordeshook formulation has been criticized for appearing to do just that. They reconceptualize the probability that a citizen's vote makes a difference as a function of his subjective perception of the closeness of the election. And they suggest that in many cases this judgment is inflated. More important, Riker and Ordeshook introduce into the voting calculus a direct utility gain, D, from the act of voting. Among other things, D incorporates what political scientists know as a "sense of citizen duty." Thus, the decision to vote is rendered rational by recognizing individual errors on the one hand and postulating cathartic or psychic rewards on the other.

How satisfactory is the Riker-Ordeshook reformulation? First of all, their data indicate that most of the action is in the D term. Consider Table 1 which is the Riker-Ordeshook data in collapsed form.[6] Although the relationships all are in the predicted direction, P appears to make rather little difference, B somewhat more, and D the most. Thus, one asks, how satisfactory is the introduction of D? No doubt many political scientists would share Brian Barry's judgment of the D term.

> "Riker [and Ordeshook say] . . . that people vote because they derive satisfaction from voting for reasons entirely divorced from the hope that it will bring about desired results. This may well be true but it does not leave any scope for an economic model to come between the premises and the phenomenon to be explained. Instead, the question shifts back to: 'Why do some people have this kind of motivation more strongly than others'?"[7]

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* We should like to acknowledge the encouragement of Charles Plott, the critical acumen of John Benton, James Quirk, Kenneth Shepsle, Peter Aranson, Peter Ordeshook, and Duff Spafford, and, finally, the prescience of David Seidman.

[1] For a summary of theoretical work in this area see Otto Davis, Melvin Hinich, and Peter Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June 1970), 426–448. For attempts to interpret data within the framework of rational choice models, see two papers presented at the 1972 Public Choice Convention: John Jackson, "The Importance of Issues and Issue Importance in Presidential Elections: A Test of a 'Rational' Model"; George Rabinowitz, "A Spatial Look at U. S. Politics." We should remark that throughout this paper we use the expression "rational behavior" in a nontechnical sense denoting purposeful behavior.

[2] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), pp. 260–276.

[3] *Ibid.*, chapter 14.

[4] Tullock, *Toward a Mathematics of Politics* (Ann Arbor, Michigan: University of Michigan Press, 1967), chapter 7.

[5] William Riker and Peter Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March 1968), 25.

[6] Brian Barry draws this table from Riker's and Ordeshook's Table 3. See Barry, *Sociologists, Economists and Democracy* (London: Collier-Macmillan Ltd., 1970), p. 17. Riker and Ordeshook, Table 3, p. 38.

[7] Barry, p. 16.

Table 1. Empirical Test of the Riker-Ordeshook Calculus of Voting

Expected Closeness of Election Results (P)	'Citizen Duty' Score (D)					
	High		Medium		Low	
	High B	Low B	High B	Low B	High B	Low B
Close	91%	83%	85%	71%	63%	44%
Not close	86%	74%	77%	71%	62%	39%

B = party differential, i.e., how much difference the respondent thought it made which side won. The percentages give the proportion who voted in each category.

To elaborate, Downs and Tullock model the voting decision as an investment decision. For the prospect of some uncertain future benefit (PB) one accepts costs (C) with certainty in the present. Addition of the D term to the voting calculus, however, changes the model to one of consumption decision. Just as one buys tomatoes because the utility of eating tomatoes and incurring their selling price exceeds that of forgoing the tomatoes, so one votes because the psychic pleasure of pulling the lever (D) exceeds the costs (C) of doing so. Thus, one needs only to explain why some citizens get a big bang out of pulling the lever while others don't.

The question is then whether the principal explanation of the voting act is found in its investment aspect or its consumption aspect. If it is in the former, then one type of rational choice model is appropriate. If in the latter, another theory—the theory of consumer choice—is relevant. Barry is wrong when he concludes that if the voting act is a consumption decision, economic models have nothing to say about it. Part of the theory of consumer choice investigates the structure of individual preferences: substitutes, complements, inferior goods, and the like. And, there are many empirical studies of voting behavior to which such theory is appropriate. Why, for example, are high income people more likely to vote? One way of asking this question is to investigate complementaries in preference structures.

Where, then, does the problem lie—with a ubiquitous behavior that cannot be explained as an investment decision, or with the analyses of contemporary theorists? We believe that the latter is the case. In the remainder of this paper we shall argue the following proposition: under rather general circumstances it is rational for many citizens to vote even if they neither distort their individual impact nor place a direct value on the act of voting. The argument involves a comparison of alternative decision-making rules voters could employ. And although the argument is rather abstract, several testable implications are derived.

Before proceeding let us agree on some basic terminology. Assume that each voter has preferences over the set of possible candidate-party-issue packages represented by a utility function, U. Let C_1 and C_2 be the two candidates (who may be viewed as bundles of personal, party and issue characteristics). The gain to a voter if his candidate wins is $|U(C_1)-U(C_2)|$. For notational simplicity assume that the voter prefers candidate 1 to candidate 2 and that his utility function is normalized so that $U(C_1)=1$, $U(C_2)=0$. Let c be the cost of voting expressed on the same utility scale. In the case of tie votes, assume a fair coin is flipped so that each voter expects to receive

$$\tfrac{1}{2}U(C_1) + \tfrac{1}{2}U(C_2) = \tfrac{1}{2}.$$

The decision problem of the citizen takes the form of a $3\times 3^{(N-1)}$ table, where the voter has a choice of voting for C_1, C_2, or abstaining (strategies V_1, V_2, A), and the states of nature are configurations of the same 3 choices by the other $(N-1)$ citizens. This table reduces to a 3×5 table by collapsing all of the identical columns. The five mutually exclusive and collectively exhaustive states of nature are as follows (where n_i equals the number of votes for C_i *exclusive* of the citizen under consideration):

States	*Definition*	*Description*
S_1	$n_1>n_2+1$	C_1 wins by more than one vote regardless of the citizen's vote
S_2	$n_1=n_2+1$	C_1 wins by exactly one vote without the citizen's vote
S_3	$n_1=n_2$	C_1 ties C_2 without the citizen's vote
S_4	$n_1=n_2-1$	C_1 loses by exactly one vote without the citizen's vote
S_5	$n_1<n_2-1$	C_1 loses by more than one vote without the citizen's vote

Thus, the decision problem appears as Table 2, where the cell entries represent the payoffs forthcoming from choice of a particular action under a particular state of nature.

Table 2. Payoff Matrix for Two-Candidate Plurality Election

Acts	S_1	S_2	S_3	S_4	S_5
V_1	$1-c$	$1-c$	$1-c$	$\frac{1}{2}-c$	$-c$
V_2	$1-c$	$\frac{1}{2}-c$	$-c$	$-c$	$-c$
A	1	1	$\frac{1}{2}$	0	0

Thus, if S_3 holds, the citizen can give either candidate the victory by voting for him. If he votes for C_1, his payoff is $1-c$; if he votes for C_2 his payoff is $-c$; if he abstains, his payoff is $\frac{1}{2}$, the expected utility of the coin flip.

Two-Candidate Contests

The Conventional Analysis: Voting as Decision Making under Risk

If the citizen is an expected utility maximizer, we assume he has certain beliefs about what his fellow citizens are likely to do. These beliefs are summarized by a vector of subjective probabilities

$$p = (p_1, p_2, p_3, p_4, p_5),\ p_j \geq 0,\ \sum_{j=1}^{5} p_j = 1,$$

where p_1 is the citizen's subjective probability that S_1 will hold and the other p_j are defined analogously. The assumption of expected utility maximizing postulates that the citizen chooses the act whose associated expected utility is greatest. Applying this decision rule to Table 1 yields the conventional analysis.

One sees first that voting for one's lesser preferred candidate need not be considered: strategy V_2 is dominated by both V_1 and A.[8] Between V_1 and A, then, the citizen-as-expected-utility-maximizer votes rather than abstains if $EU(V_1) > EU(A)$, or

$$\begin{aligned}&[p_1(1-c) + p_2(1-c) + p_3(1-c)\\ &+ p_4(\tfrac{1}{2}-c) + (1-p_1-p_2-p_3-p_4)(-c)]\\ &\quad > [p_1(1) + p_2(1) + p_3(\tfrac{1}{2}) + p_4(0)\\ &\qquad + (1-p_1-p_2-p_3-p_4)0].\end{aligned}$$

which simplifies to the condition

$$p_3 + p_4 > 2c. \tag{1}$$

That is, if the citizen's subjective probability that a vote for his preferred candidate will break or create a tie exceeds twice his relative costs of voting, he rationally decides to vote. Two implications are immediate. First, if $c > \frac{1}{2}$, the citizen will never vote, no matter what his beliefs about his fellow citizens. Second, the smaller c is, the larger the set of probability beliefs which allow rational voting. (Bear in mind that c may be small either because voting costs truly are negligible or because candidate preference is intense, so that costs are small relative to the unit of the utility scale determined by preferences for candidates.)

The preceding analysis is basically that carried out by Downs and Tullock, albeit with a somewhat more precise specification of the problem. The citizen is assumed to engage in decision making under *risk*, i.e., he assigns a probability distribution to the states of nature. The conclusion one draws is that unless costs are negligible, or the utility difference between the candidates so enormous that voting costs are insignificant by comparison, (1) is very unlikely to be satisfied unless subjective estimates of p_3 and p_4 are grossly inflated.

An Alternative Analysis: Voting as Decision Making under Uncertainty

If a citizen calculates according to the conventional analysis, he will decide to abstain. But all citizens will arrive at the same decision; therefore, a smart citizen would vote and singlehandedly decide the election. And yet, other citizens would also follow this strategy, so maybe he should abstain after all. But if other citizens reason similarly, maybe . . . and so forth. Clearly we have a highly complex situation. The strategic interdependence of voter decisions makes it difficult to conceive of how the citizen could confidently assign probabilities to the states of nature. In other words, the assumption that the citizen is making a decision under risk may be inappropriate to the context of the voting decision.

Classical decision theorists differentiate between decision making under *risk* and decision making under *uncertainty*. Under risk, probabilities can be assigned to the states of nature; under uncertainty, state probabilities are unknown or unknowable.[9] (For an example of the latter, consider the question, "What is the probability that there is an afterlife?") Classical theorists treat the conditions of risk and uncertainty differently, developing decision criteria for the latter case which do not depend on state probability estimates. In this section, we will demonstrate that one well-known classical procedure yields a quite weak condition for voting *vis-à-vis* abstaining.[10]

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[8] A strategy *i* dominates a strategy *j* if *i*'s consequences are at least as good as *j*'s consequences (and at least one is better), for every conceivable state of nature.

[9] Some scholars would deny the usefulness of this distinction. In particular, if one holds a subjectivist view of probability, no state probability necessarily is unknown or unknowable. But we still believe the distinction makes some intuitive sense. For an excellent discussion of the issues surrounding the risk-uncertainty distinction see Kenneth Shepsle, "Essays on Risky Choice in Electoral Competition" (Ph.D. dissertation, University of Rochester, 1970), chapter 1.

Consider Savage's minimax regret criterion.[11] Rather than operate on a matrix of outcomes, the Savage criterion operates on a matrix of "regrets." The regret, r_{ij}, accruing to an act a_i if state S_j comes to pass is defined as the difference between what the decision maker could have attained had he known the true state of nature before he chose his action and what he actually gets by choosing a_i. In the voting decision problem, suppose the behavior of the other citizens is such that if the *i*th citizen does not vote, the election will end in a tie. Then the "regret" of the *i*th citizen because of not having voted is (from Table 2) $(1-c-\frac{1}{2})=\frac{1}{2}-c$. Had he voted, he would have received $\frac{1}{2}-c$ more than he got. Using this method of computing regrets, we can transform Table 2 into a regret matrix (Table 3).

Now, the minimax regret criterion specifies that the citizen should choose the act which minimizes his maximum regret. The maximum regrets of V_1, V_2 and A are c, 1, and $\frac{1}{2}-c$, respectively. Clearly, V_2 is not a viable strategy. But the citizen would vote for his preferred candidate rather than abstain if max reg $V_1<$max reg A, i.e.,

$$c < \tfrac{1}{2} - c \Rightarrow c < \tfrac{1}{4}. \tag{2}$$

(If the utility scale were not normalized, (2) would be transformed accordingly, i.e., if $U(C_1)=4.4$, $U(C_2)=0$, then (2) would become $c<1.1$.) Thus, the following proposition is established: In a two-candidate plurality winner contest, a citizen following the minimax regret decision rule votes for his preferred candidate rather than abstains if the utility gain from the election of his preferred candidate exceeds four times the utility loss of the voting act.

Table 3. Regret Matrix for Two-Candidate Election

Acts	S_1	S_2	S_3	S_4	S_5
V_1	c	c	0	0	c
V_2	c	$\frac{1}{2}+c$	1	$\frac{1}{2}$	c
A	0	0	$\frac{1}{2}-c$	$\frac{1}{2}-c$	0

Assumption: $\frac{1}{2}>c$.[12]

This result shows that analysts must specify exactly what they mean by "rational" voting. If a citizen does not make (does not act as if he makes) expected utility calculations, do we condemn his behavior as irrational? Clearly, there is an important distinction between expected utility maximizers and minimax regret decision makers. Minimax regretters vote if $\frac{1}{4}>c$, while expected utility maximizers vote only if $p_3+p_4>2c$. The former condition probably is satisfied for many voters; the latter condition probably is not. And in deciding between criteria, we must remember that 60–70 per cent of the electorate typically does vote in a presidential election, a fact the expected utility analysis cannot accommodate without postulating a D term. Notice, too, that the empirical hypothesis implied by the conventional analysis follows just as well from the minimax regret analysis. Namely, as c decreases (either in the costs composing it or relative to the unit of the candidate preference scale) the condition for voting is met more easily. We believe that the implications of these alternative decision criteria for voter psychology are fascinating and important questions. After the analysis of three candidate elections, we shall venture some tentative answers.

Three-Candidate Elections

Two questions concern us in this section. First, is it again the case that minimax regret and expected utility maximizing rationality criteria imply significantly different predictions about the voting decision? Second, given an enlarged set of available voting strategies, does a citizen still make his decision between only two of them: abstaining, or voting for his most preferred candidate? We answer the first question positively. The answer to the second question provides a clear test for distinguishing empirically between expected utility maximizers and minimax regretters.

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[10] There are several major classical decision rules. These are surveyed on an elementary level in R. Duncan Luce and Howard Raiffa, *Games and Decisions: Introduction & Critical Survey* (New York: Wiley, 1957), chapter 13, and on a more advanced level in D. J. White, *Decision Theory* (Chicago: Aldine, 1969), chapter 2.

[11] See the description of this criterion in Luce and Raiffa, pp. 280–282; White, pp. 28–29.

[12] In computing regrets we assume $c < ½$, i.e., the utility cost of voting is less than ½ the utility difference between having one's preferred candidate in office rather than his opponent. The assumption is not very restrictive in our view. Essentially we are analyzing average voters, those who have a preference for C_1 or C_2, and for whom the costs of voting are the typical costs in time and effort. Clearly our assumption may be violated for blind arthritics or those totally indifferent between the candidates. Such groups, however, normally will not constitute a very significant proportion of the electorate. The assumption does not let us get away with anything, because all conditions we derive for voting are even stronger. The assumption is necessary, of course, because from Table 2 one sees that unless it is satisfied one never votes: A would dominate V_1 as well as V_2. Analogous considerations lead us to make the same assumption in the three-candidate case. If it did not hold, A would dominate V_1 for all states except S_{16} and would not fail to dominate then if k were large.

In a three-candidate election the citizen's decision problem takes the form of a $4 \times 4^{N-1}$ table, with the rows signifying votes for C_1, C_2, C_3, or abstention, and the columns representing all possible combinations of the same choices by the $N-1$ other members of the electorate. The myriad states of nature reduce to the following set of 19 mutually exclusive and collectively exhaustive states:

State	*Definition*	*Description*
1	$n_1 > n_j + 1,\ j = 2,3$	A clear winner exists; one vote cannot change the outcome
2	$n_2 > n_j + 1,\ j = 1,3$	
3	$n_3 > n_j + 1,\ j = 1,2$	
4	$n_1 = n_2 + 1 > n_3 + 1$	A narrow winner, C_1, exists; one vote for a just losing candidate produces a tie
5	$n_1 = n_3 + 1 > n_2 + 1$	
6	$n_1 = n_2 + 1 = n_3 + 1$	
7	$n_2 = n_1 + 1 > n_3 + 1$	Same, with C_2 being the narrow winner
8	$n_2 = n_3 + 1 > n_1 + 1$	
9	$n_2 = n_3 + 1 = n_1 + 1$	
10	$n_3 = n_1 + 1 > n_2 + 1$	Same, with C_3 being the narrow winner
11	$n_3 = n_2 + 1 > n_1 + 1$	
12	$n_3 = n_1 + 1 = n_2 + 1$	
13	$n_1 = n_2 > n_3 + 1$	A two-way tie for first place exists; one vote could break tie
14	$n_1 = n_3 > n_2 + 1$	
15	$n_2 = n_3 > n_1 + 1$	
16	$n_1 = n_2 = n_3$	A three-way tie exists; one vote could break tie
17	$n_1 = n_2 = n_3 + 1$	A two-way tie for first place exists, but now one vote could break this tie or produce a three-way tie
18	$n_1 = n_3 = n_2 + 1$	
19	$n_2 = n_3 = n_1 + 1$	

where n_i = number of votes for C_i *exclusive* of the voter under consideration. Assume the voter prefers C_1 first, C_2 second and C_3 third (relabeling candidates if necessary) and his utility function is normalized, so that $U(C_1) = 1$, $U(C_2) = k$, $U(C_3) = 0$. For ease of analysis we assume strict preference so that $1 > k > 0$. As previously, we assume that tie votes yield payoffs to the citizen which are the averages of the utilities of the tied candidates. The decision problem then appears as Table 4.

The Conventional Analysis: Voting as Decision Making under Risk[13]

In this subsection we will consider two questions. First, what conditions justify voting at all? Second, if the citizen votes, for whom does he vote? The most parsimonious approach involves answering the second question first.

A quick perusal of Table 4 makes one fact evident. Voting for one's least preferred candidate is a dominated strategy. Thus, we need only examine strategies V_1 and V_2 in deciding for whom a citizen votes given that he has decided to vote.

Strategies V_1 and V_2 lead to different payoffs in states 4, 6, 7 and 9–19. Notice, too, that only in states 11, 15 and perhaps 19 is the payoff from voting for one's second choice greater than that from voting for one's first choice. In general, voting for one's second choice rather than for one's first choice is rational for an expected utility maximizer if $EU(C_2) - EU(C_1) > 0$, or (see below)

$$\Bigg[p_4\left(\frac{1+k}{2} - c\right) + p_6\left(\frac{1+k}{2} - c\right) + p_7(k - c) + p_9(k - c) + p_{10}(-c) + p_{11}\left(\frac{k}{2} - c\right)$$
$$+ p_{12}\left(\frac{k}{2} - c\right) + p_{13}(k - c) + p_{14}\left(\frac{1}{2} - c\right) + p_{15}(k - c) + p_{16}(k - c) + p_{17}(k - c)$$
$$+ p_{18}\left(\frac{1+k}{3} - c\right) + p_{19}(k - c)\Bigg] - \Bigg[p_4(1 - c) + p_6(1 - c) + p_7\left(\frac{1+k}{2} - c\right)$$
$$+ p_9\left(\frac{1+k}{2} - c\right) + p_{10}\left(\frac{1}{2} - c\right) + p_{11}(-c) + p_{12}\left(\frac{1}{2} - c\right) + p_{13}(1 - c) + p_{14}(1 - c)$$
$$+ p_{15}\left(\frac{k}{2} - c\right) + p_{16}(1 - c) + p_{17}(1 - c) + p_{18}(1 - c) + p_{19}\left(\frac{1+k}{3} - c\right)\Bigg] > 0.$$

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[13] For a more extensive analysis of the voting behavior of expected utility maximizers in multicandidate contests see Richard McKelvey and Peter Ordeshook, "A General Theory of the Calculus of Voting," in *Mathematical Applications in Political Science,* VI, ed. James Herndon and Joseph Bernd (Charlottesville, Va.: The University of Virginia, 1972). Our conclusions from this brief analysis seem consonant with theirs, although we pay more attention to the possibility of voting for one's second choice.

Subtracting and combining terms, one sees that a citizen votes for his second choice only if

$$\left[\frac{k}{2}(p_{11}+p_{15})+\left(\frac{2k}{3}-\frac{1}{3}\right)p_{19}\right]>\left[\frac{1}{2}(p_{10}+p_{14})-(k-1)(p_{13}+p_{16}+p_{17})\right.$$
$$\left.-\left(\frac{k}{2}-\frac{1}{2}\right)(p_4+p_6+p_7+p_9+p_{12})-p_{18}\left(\frac{k}{3}-\frac{2}{3}\right)\right] \tag{3}$$

Were the inequality reversed, he would vote for his first choice. If equality held, he would be indifferent.

For reasons we shall discuss immediately, it is very important to show that (3) can in principle be satisfied. That is, circumstances are conceivable in which rational citizens vote for their second choice rather than their most preferred candidate. Assume $k=.9$, $(p_{11}+p_{15})=.6$, $p_{19}=0$. Then no allocation of the remaining probability to the

Table 4. Decision Problem for Three-Candidate Contest

Act	States																		
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
V_1	$1-c$	$k-c$	$-c$	$1-c$	$1-c$	$1-c$	$\frac{1+k}{2}-c$	$k-c$	$\frac{1+k}{2}-c$	$\frac{1}{2}-c$	$-c$	$\frac{1}{2}-c$	$1-c$	$1-c$	$\frac{k}{2}-c$	$1-c$	$1-c$	$1-c$	$\frac{1+k}{3}-c$
V_2	$1-c$	$k-c$	$-c$	$\frac{1+k}{2}-c$	$1-c$	$\frac{1+k}{2}-c$	$k-c$	$k-c$	$k-c$	$-c$	$\frac{k}{2}-c$	$\frac{k}{2}-c$	$k-c$	$\frac{1}{2}-c$	$k-c$	$k-c$	$k-c$	$\frac{1+k}{3}-c$	$k-c$
V_3	$1-c$	$k-c$	$-c$	$1-c$	$\frac{1}{2}-c$	$\frac{1}{2}-c$	$k-c$	$\frac{k}{2}-c$	$\frac{k}{2}-c$	$-c$	$-c$	$-c$	$\frac{1+k}{2}-c$	$-c$	$-c$	$-c$	$\frac{1+k}{3}-c$	$-c$	$-c$
A	1	k	0	1	1	1	k	k	k	0	0	0	$\frac{1+k}{2}$	$\frac{1}{2}$	$\frac{k}{2}$	$\frac{1+k}{3}$	$\frac{1+k}{2}$	$\frac{1}{2}$	$\frac{k}{2}$

Notation	*Assumptions*
$U(C_1)=1$	$1>k>0$
$U(C_2)=k$	$\frac{1}{2}>c>0$
$U(C_3)=0$	

terms on the right hand side of (3) can reverse the inequality, i.e., make voting for one's first choice rational.

The practical importance of showing that condition (3) can hold stems from the fact that a controversy on just this question has occupied space in five recent issues of the *American Political Science Review*.[14] In *An Economic Theory of Democracy*, Downs argued impressionistically that in multi-party contests voters might rationally vote for candidates other than their first choice.[15] The assertion seems plausible enough. If one's favorite has no chance of winning but one's second choice is running neck and neck with a candidate lower on one's scale of values, voting for one's second choice appears reasonable. Certainly, many have explained defections away from Wallace in the closing days of the 1968 campaign in such terms. Similarly, during the 1970 New York senatorial race, late polls showed Goodell trailing badly, fueling speculation that Goodell supporters might switch to Ottinger in an attempt to defeat Buckley. Sources in both the Goodell and the Ottinger camps took the possibility seriously, although they viewed it in considerably different lights. (The frequency of such behavior is not at issue, only its rationality.)

Thus, many were surprised by Thomas Casstevens's "A Theorem about Voting," in which he purported to prove that in multiparty contests, rational citizens always vote for their most preferred candidate.[16] Gerald Kramer quickly pointed out that Casstevens's proof was technically incorrect because of his consideration of tie votes.[17] Even more important, Kramer noted, Casstevens proved his theorem by throwing away the problem. Casstevens employed a "reduction postulate" which asserted in effect that if a citizen switched his vote from C_2 to C_3, the probability that C_1 would win remained unchanged. Naturally, the rationale for vote switching is *precisely* the possibility of altering the candidates' probabilities of election. Without the reduction postulate Casstevens's theorem cannot be proved; with it the theorem is substantively irrelevant. Although the controversy seems closed now, our formulation makes the issues involved intuitively clear to those who might have felt unclear about the technical details of the argument.

By examining Table 4 one sees immediately that there are conditions which justify voting for one's second choice. If states 11 or 15 hold, V_2 is optimal. Depending on k, V_2 may also be optimal if state 19 holds. But obviously one's vote changes the probabilities of various candidates winning. Under state 11 or 15 if the citizen chooses V_1, C_3 wins or ties, respectively. But should the citizen choose V_2, he reduces the probability that C_3 wins to .5 or 0, because he either creates a tie or puts C_2 over the top. Finally, if state 19 holds, the citizen must decide whether to create a 3-way tie or make C_2 the winner. Thus, our payoff matrix confirms Downs and Kramer, while illustrating clearly the effects of Casstevens's assumptions and the relevance of Kramer's criticisms.

Now, we must turn to the question of when an expected utility maximizer decides to vote at all. If (3) is *not* satisfied, he votes for his *most preferred candidate rather than abstain* if $EU(C_1)-EU(A)>0$, i.e.,

$$\left[-c\left(\sum_{i=1}^{19} p_i\right)+\frac{1}{2}(p_7+p_9+p_{10}+p_{12}+p_{13}+p_{14}+p_{17}+p_{18})+\frac{2}{3}p_{16}+\frac{1}{3}p_{19}-\frac{k}{2}(p_7+p_9+p_{13}+p_{17})-\frac{k}{3}p_{16}-\frac{k}{6}p_{19}\right]>0$$

or, alternatively, $EU(C_1)>EU(A)$ if

$$\left[\frac{1}{2}(p_7+p_9+p_{10}+p_{12}+p_{13}+p_{14}+p_{17}+p_{18})+\frac{2}{3}p_{16}+\frac{1}{3}p_{19}\right]>\left[c+\frac{k}{2}(p_7+p_9+p_{13}+p_{17})+\frac{k}{3}p_{16}+\frac{k}{6}p_{19}\right] \qquad (4)$$

Can one say anything reasonably simple about (4)? Well, for any k, a sufficient (*not* necessary) condition for voting is simply

$$\left[\tfrac{1}{2}(p_{10}+p_{12}+p_{14}+p_{18})+\tfrac{1}{3}p_{16}+\tfrac{1}{6}p_{19}\right]\geq c \qquad (5)$$

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[14] Thomas Casstevens, "A Theorem about Voting," *American Political Science Review*, 62 (March 1968), 205–207. Gerald Kramer, Comment, *APSR*, 62 (September 1968), 955–956. Casstevens, Reply; Kramer, Rebuttal, *APSR*, 65 (March 1971), 187–189. Lawrence Mayer, Comment, *APSR*, 65 (September 1971), 779. Kramer, Rebuttal, *APSR*, 66 (March 1972), 183.

[15] Downs, pp. 146–150.

[16] Casstevens, footnote 14.

[17] Kramer, footnote 14.

Table 5. Regret Matrix for Three-Candidate Contest[a]

Acts	States									
	1	2	3	4	5	6	7	8	9	10
V_1	c	c	c	c	c	c	0 $\frac{k}{2}-\frac{1}{2}+c$	c	0 $\frac{k}{2}-\frac{1}{2}+c$	0
V_2	c	c	c	$\frac{1}{2}-\frac{k}{2}+c$	c	$\frac{1}{2}-\frac{k}{2}+c$	$\frac{1}{2}-\frac{k}{2}$ c	c	$\frac{1}{2}-\frac{k}{2}$ c	$\frac{1}{2}$
V_3	c	c	c	c	$\frac{1}{2}+c$	$\frac{1}{2}+c$	$\frac{1}{2}-\frac{k}{2}$ c	$\frac{k}{2}+c$	$\frac{1}{2}$ $\frac{k}{2}+c$	$\frac{1}{2}$
A	0	0	0	0	0	0	$\frac{1}{2}-\frac{k}{2}-c$ 0	0	$\frac{1}{2}-\frac{k}{2}-c$ 0	$\frac{1}{2}-c$

Acts	11	12	13	14	15	16	17	18	19
V_1	$\frac{k}{2}$ c	0	$\frac{k}{2}-\frac{1}{2}+c$ 0	0	c c	$\frac{k}{3}-\frac{2}{3}+c$ 0	0 $\frac{k}{2}-\frac{1}{2}+c$	0	0 $\frac{k}{6}-\frac{1}{3}+c$
V_2	0 $c-\frac{k}{2}$	$\frac{1}{2}-\frac{k}{2}$	$\frac{1}{2}-\frac{k}{2}+c$ $1-k$	$\frac{1}{2}$	0 $-\frac{k}{2}+c$	$\frac{1}{3}-\frac{2k}{3}+c$ $1-k$	$1-k$ $\frac{1}{2}-\frac{k}{2}+c$	$\frac{2}{3}-\frac{k}{3}$	$\frac{1}{3}-\frac{2k}{3}$ $c-\frac{k}{2}$
V_3	$\frac{k}{2}$ c	$\frac{1}{2}$	c $\frac{1}{2}-\frac{k}{2}$	1	k $\frac{k}{2}+c$	$\frac{1}{3}+\frac{k}{3}+c$ 1	$\frac{2}{3}-\frac{k}{3}$ $\frac{1}{6}-\frac{k}{6}+c$	1	$\frac{1+k}{3}$ $\frac{k}{2}+c$
A	$\frac{k}{2}-c$ 0	$\frac{1}{2}-c$	0 $\frac{1}{2}-\frac{k}{2}-c$	$\frac{1}{2}-c$	$\frac{k}{2}-c$ 0	0 $\frac{2}{3}-\frac{k}{3}-c$	$\frac{1}{2}-\frac{k}{2}-c$ 0	$\frac{1}{2}-c$	$\frac{1}{3}-\frac{k}{6}-c$ 0

Thus, if $\frac{1}{2}$ the probability of making or breaking a tie between C_1 and C_3 plus $\frac{1}{3}$ the probability of breaking a three-way tie plus $\frac{1}{6}$ the probability of bringing C_1 into a three way tie exceeds the cost of voting, one certainly votes for C_1 although he might also vote if (5) is not satisfied but (4) is.

Assume now that (3) *is* satisfied. Under what conditions does a citizen vote for his *second choice rather than abstain?* That condition is given by (6).

$$\left[\frac{k}{2}(p_4+p_6+p_{11}+p_{12}+p_{13}+p_{15}+p_{17}+p_{19}) + \frac{2k}{3}p_{16}+\frac{k}{3}p_{18}\right] > \quad (6)$$

$$\left[c+\tfrac{1}{2}(p_4+p_6+p_{13}+p_{17})+\tfrac{1}{3}p_{16}+\tfrac{1}{6}p_{18}\right]$$

Both (4) and (6) imply reasonable hypotheses. Either condition will be more difficult to satisfy the higher c is. In the case of (4), the smaller k is, the larger the set of probability beliefs that will allow voting for C_1 rather than abstaining. Conversely, in the case of (6), the larger k is, the larger the set of beliefs which justify voting for one's second choice rather than abstaining.

At the same time, one must recognize the fact that the probability terms in (4) and (6) all concern the probabilities of ties and one vote victories. In an electorate of any size, with subjective probability estimates which are at all realistic, it seems very unlikely that the left hand side of either (4) or (6) will exceed the cost of voting. As in the two-candidate case, the expected utility maximizer in the three-candidate case appears to be most rational by going fishing on election day.

An Alternative Analysis: Voting as Decision Making under Uncertainty

The minimax regret analysis of the three-candidate case is perfectly straightforward although somewhat more involved than in the two-candidate case. The additional complexity arises because of non-unique maximum regrets for strategies V_1, V_2 and A. Table 5 is the matrix of regrets.

Consider V_1. If $\frac{k}{2} \geq c$, then $\frac{k}{2}$ can be identified as the maximum regret of V_1, while if the inequality is reversed, c must be chosen.

For V_2 there are more possibilities: four, to be exact. The maximum regret of V_2 is

$$\frac{1}{2} \text{ if } (1)\ \frac{1}{2} \geq 1-k \qquad \Rightarrow k \geq \frac{1}{2}$$

and

$$(2)\ \frac{1}{2} \geq \frac{1}{2}-\frac{k}{2}+c \qquad \Rightarrow \frac{k}{2} \geq c$$

and

$$(3)\ \frac{1}{2} \geq \frac{2}{3}-\frac{k}{3} \qquad \Rightarrow k \geq \frac{1}{2}$$

$$1-k \text{ if } (1)\ 1-k \geq \frac{1}{2} \qquad \Rightarrow k \leq \frac{1}{2}$$

and

$$(2)\ 1-k \geq \frac{1}{2}-\frac{k}{2}+c \qquad \Rightarrow c \leq \frac{1}{2}-\frac{k}{2}$$

and

$$(3)\ 1-k \geq \frac{2}{3}-\frac{k}{3} \qquad \Rightarrow k \leq \frac{1}{2}$$

$$\frac{1}{2}-\frac{k}{2}+c \text{ if } (1)\ \frac{1}{2}-\frac{k}{2}+c \geq \frac{1}{2} \qquad \Rightarrow c \geq \frac{k}{2}$$

and

$$(2)\ \frac{1}{2}-\frac{k}{2}+c \geq 1-k \qquad \Rightarrow c \geq \frac{1}{2}-\frac{k}{2}$$

and

$$(3)\ \frac{1}{2}-\frac{k}{2}+c \geq \frac{2}{3}-\frac{k}{3} \qquad \Rightarrow k \leq 6c-1$$

$$\frac{2}{3}-\frac{k}{3} \text{ if } (1)\ \frac{2}{3}-\frac{k}{3} \geq \frac{1}{2} \qquad \Rightarrow k \leq \frac{1}{2}$$

and

$$(2)\ \frac{2}{3}-\frac{k}{3} \geq 1-k \qquad \Rightarrow k \geq \frac{1}{2}$$

and

$$(3)\ \frac{2}{3}-\frac{k}{3} \geq \frac{1}{2}-\frac{k}{2}+c \qquad \Rightarrow k \geq 6c-1$$

Notice that $\frac{2}{3}-\frac{k}{3}$ can be the maximum regret of V_2 only if $k=.5$, in which case $(\frac{2}{3}-\frac{k}{3})=\frac{1}{2}$. Thus the four possibilities for V_2 reduce to three.

The maximum regret of V_3 is unity.

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[a] Notice that some columns of the matrix have two entries for each cell. This occurs because the maximum payoff cannot be specified uniquely in some columns. When this occurs we have computed regrets based on both of the possible maximum entries. In S_7 for example, the top entry in each cell is the regret computed assuming cell (1, 7) of Table 4 contains the maximum payoff for S_7, while the bottom entry in each cell is the regret computed assuming cell (4, 7) contains the maximum payoff. In S_{19} there are actually three possibilities (V_1, V_2, A), although we have only included two since the third proves irrelevant to the analysis. The non-uniqueness is fully considered in the analysis which follows.

For A there are two possibilities. The maximum regret of A is

$$\frac{1}{2}-c \text{ if } \frac{1}{2}-c \geq \frac{2}{3}-\frac{k}{3}-c \qquad \Rightarrow k \geq \frac{1}{2}$$

$$\frac{2}{3}-\frac{k}{3}-c \text{ if } \frac{2}{3}-\frac{k}{3}-c \geq \frac{1}{2}-c \qquad \Rightarrow k \leq \frac{1}{2}$$

Let us proceed to check for minimax regret strategies by a process of elimination. Clearly, 1 is greater than $\frac{k}{2}$, c, $\frac{1}{2}$, $1-k$, and $\frac{1}{2}-\frac{k}{2}+c$, given the particular conditions under which each of these five numbers is maximum regret for its respective strategy. Thus, our first conclusion is an obvious and plausible one: Voting for one's least preferred candidate is never minimax regret optimal.

Now consider strategies V_1 and V_2. If $\frac{k}{2} \geq c$, the maximum regret of V_1 is $\frac{k}{2}$, while the maximum regret of V_2 is either $\frac{1}{2}$ or $1-k$. The maximum regret of V_1 will be less than the maximum regret of V_2 iff

$$\frac{k}{2}<\frac{1}{2} \qquad \Rightarrow k<1$$

and

$$\frac{k}{2}<1-k \qquad \Rightarrow k<\frac{2}{3}$$

By assumption $k<1$, so the first condition always is true. The second condition always holds under the conditions ($k \leq \frac{1}{2}$) in which $(1-k)$ can be the maximum regret of V_2. Thus, we see that when $\frac{k}{2} \geq c$ the maximum regret of V_1 always is less than that of V_2.

If $\frac{k}{2} \leq c$, then c is the maximum regret of V_1 while either $(1-k)$ or $(\frac{1}{2}-\frac{k}{2}+c)$ must be the maximum regret of V_2. Maximum regret of $V_1<$ maximum regret of V_2 iff

$$c<1-k \qquad \Rightarrow c+k<1$$

and

$$c<\frac{1}{2}-\frac{k}{2}+c \qquad \Rightarrow k<1$$

Again, the second condition always is true since by assumption $k<1$. The first also always is true because by assumption $c<\frac{1}{2}$ and $(1-k)$ is the maximum regret of V_2 only if $k \leq \frac{1}{2}$. Thus, when $\frac{k}{2} \leq c$ the maximum regret of V_1 always is less than that of V_2. Putting the two cases together, we arrive at a second, rather more surprising, conclusion: *Voting for one's second choice is never minimax regret optimal.* This conclusion affords the possibility of a critical test between minimax regret and maximizing expected utility rationality criteria. Any citizen who votes for his second choice while believing $1>k>0$ and $\frac{1}{2}>c$ cannot be behaving according to a minimax regret decision rule.

Thus far we have specified only the minimax regret strategy among the three voting strategies. Now we must determine whether there exist conditions under which voting for one's first choice carries a smaller regret than abstaining. Two cases must be considered.

1) If $\frac{k}{2} \geq c$, $\frac{k}{2}$ is the maximum regret of V_1, while either $(\frac{1}{2}-c)$ or $(\frac{2}{3}-\frac{k}{3}-c)$ is the maximum regret of A. Thus, max regret of $V_1<$ max regret of A iff

$$\begin{gathered}\frac{k}{2}<\frac{1}{2}-c \qquad \Rightarrow c<\frac{1}{2}-\frac{k}{2} \\ \text{and} \\ \frac{k}{2}<\frac{2}{3}-\frac{k}{3}-c \qquad \Rightarrow c<\frac{2}{3}-\frac{5}{6}k\end{gathered} \tag{7}$$

The two conditions are equivalent iff $k=\frac{1}{2}$. If $k>\frac{1}{2}$, the first condition governs, while if $k<\frac{1}{2}$, the second condition governs.

2) If $c \geq \frac{k}{2}$, c is the maximum regret of V_1, while $(\frac{1}{2}-c)$ or $(\frac{2}{3}-\frac{k}{3}-c)$ is the maximum regret of A. Thus, max regret of $V_1<$ max regret of A iff

$$\begin{gathered}c<\frac{1}{2}-c \qquad \Rightarrow \quad c<\tfrac{1}{4} \\ \text{and} \\ c<\frac{2}{3}-\frac{k}{3}-c \qquad \Rightarrow 2c<\frac{2}{3}-\frac{k}{3}\end{gathered} \tag{8}$$

The two conditions are equivalent iff $k=\frac{1}{2}$. If $k>\frac{1}{2}$, the first condition governs. If $k<\frac{1}{2}$, the second governs.

After examining (7) and (8), we draw our third conclusion: *Minimax-regret decision makers find it rational to vote for their most-preferred candidate rather than abstain under relatively weak conditions.* Under case (1), for example, if $k=.4$, any c in the interval $(0 \leq c \leq .2)$ would allow voting. Alternatively, if $k=.6$, any $c<.2$ would admit a rational vote. Under case (2), if $k=.2$, any c in the interval $(0 \leq c<.3)$ would allow voting.

Two additional conclusions, one obvious and one nonobvious, follow from the analysis of cases (1) and (2). As our fourth conclusion we note that *a high cost of voting constricts the range of preferences which satisfy a condition and thereby justify voting*, i.e., a high cost of voting increases the "likelihood" (in Fisher's sense) that abstention will be one's minimax regret strategy. The fifth conclusion is more surprising: *a relatively high utility for one's second choice makes the conditions more difficult to satisfy*, i.e., reduces the "likelihood" that voting will be one's minimax regret strategy.

Discussion

This paper makes a simple but often overlooked point: the concept of rational behavior is more ambiguous than many of us take it to be. We have shown that the usual arguments about the irrationality of participating in mass elections are more assailable than they appear. And in doing so we have shown that one need not postulate catch all D terms to rescue the rationality of the decision to vote. Rational choice theorists are guilty of equating the notion of rational behavior with the rule of maximizing expected utility. Alternatively, we usually define the first as the second. Yet although the rule of maximizing expected utility is the most widely known, widely used and widely accepted rationality criterion, it is not the only one. Our comparison of expected utility maximization and minimax regret decision criteria shows that the behavior of decision makers using the alternative rules differs considerably. Specifically, expected utility maximizers go to the polls only under the most restrictive conditions, whereas minimax regret decision makers need little incentive to participate. And one should remember that most people do vote.

Earlier we raised the question of what these alternative rationality criteria might suggest about voter psychology. The time has come to address that question, although we emphasize that the following comments are largely speculative. An expected utility maximizer behaves as if he estimates the probabilities of events and utilities of outcomes.[18] Each action is viewed as a lottery with choice dependent on the expected utility of each action-as-lottery. The minimax regret decision maker uses a simpler rule. He imagines himself in each possible future state of the world and looks at how much in error each of his available actions could be, given that state. Then he chooses that action whose maximum error over the states of nature is least. If asked why he voted, a minimax regret decision maker might reply "My God, what if I didn't vote and my preferred candidate lost by one vote? I'd feel like killing myself." Notice that for the expected utility maximizer the probability of such an event is very important, whereas for the minimax regret decision maker, the mere logical possibility of such an event is enough.

To expand this discussion somewhat, consider another decision criterion: the maximin rule. This rule states that the decision maker should ascertain the worst possible payoff associated with each strategy and choose the strategy whose worst payoff is best. (Maximin differs from minimax regret in that it is applied to the payoff matrix rather than the regret matrix, i.e., Tables 2 and 4 as opposed to Tables 3 and 5.) For both the two-candidate and three-candidate cases, abstention is the maximin strategy.[19] Maximin decision makers do not vote under any conditions.

We think it would be interesting to study the possibility of some association between different rationality criteria and the standard socioeconomic divisions of the population. Demographic variables typically show weak but significant relationships with voting turnout.[20] The present analysis suggests that (other things being equal) maximin decision makers would turn out less than expected utility maximizers, who would in turn vote less frequently than minimax regret decision makers. Could use of these alternative criteria somehow imperfectly parallel educational or occupational hierarchies? For example, the maximin rule is considered an extremely pessimistic decision rule. Maximin decision makers never vote. Aren't the poor and the culturally deprived more likely to be maximin decision makers than the rich and the educated? Only utility maximizers ever vote for their second choice. Isn't it rather more likely that both such sophisticated behavior and use of the most demanding rationality criterion would be concentrated among the well educated? Of course, variations in the cost of voting no doubt explain some of the variations among socioeconomic categories, but perhaps deeper relationships like the ones suggested underlie participation differences among demographic groups.

To engage in a study like that outlined, we obviously need ways of distinguishing among people who use different rationality criteria. Several are contained in this paper. For example, one could contrive experimentally a situation in which (3), (6) and (7) are satisfied. If the subject then abstains we might classify him as a maximin decision maker; if he votes for C_1, as a minimax regret decision maker; if he votes for C_2, as an expected utility maximizer.[21] After identifying people we could then check for associations between rationality criterion used, and age, occupation, education, sense of political efficacy, etc.

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[18] Actually, we describe here subjective expected utility maximizers. One can also axiomatize utility maximizing with given objective probabilities.

[19] In Table 2, the worst outcomes for V_1, V_2, and A are $-c$, $-c$, and 0, respectively. The worst outcome for A—0—is the best of the worst. Similarly, in Table 4 the worst outcomes for V_1, V_2, V_3 and A are $-c$, $-c$, $-c$, and 0, respectively. Again, the worst outcome of A—0—is the best of the worst.

[20] Lester Milbrath, *Political Participation* (Chicago: Rand McNally, 1965).

In sum, the formulation in this paper might not only provide a theoretical explanation of voting turnout; it might also provide a means of organizing and explaining the many empirical correlations which exist. And that, of course, is the value of theory.

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[21] Strictly speaking we say only that an individual behaves *as if* he were using a particular rationality criterion.

Paradoxes of Voting*

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Introduction

Given a context in which simple majority seems reasonable as a method for selecting a social alternative in a two-alternative contest, there remains to this day sizable disagreement on the best method for selecting an alternative when more than two alternatives are in contention and the social choice is to be based on preferences of the voters.[1] Two basic positions on this matter are often associated with the names of Borda and Condorcet.[2] The Borda viewpoint is a positional approach. It assigns points to each alternative for each voter according to the position of the alternative in the voter's preference order. The winner is the alternative with the greatest point total. The Condorcet philosophy is a binary-comparison viewpoint which only indirectly notes the specific position of each alternative in each preference order. Two selection methods which satisfy Condorcet's criterion—that an alternative with a strict simple majority over every other alternative should be the social choice when such an alternative exists—are (1) Copeland's method,[3] in which an alternative's score equals the number of alternatives it defeats by simple majority minus the number that defeat it, and, assuming linear preference orders for voters, (2) the method under which an alternative's score equals the minimum number of voters who prefer it to any other alternative. Elsewhere[4] I have compared the Borda and Copeland methods in a variety of ways and refer the reader to this work for these comparisons.

The present paper does not attempt to resolve the conflict between competing positions. Its main purpose is to emphasize in the strongest way possible some of the peculiar consequences that can arise under different selection procedures, thereby dramatizing potential shortcomings of these procedures. The analysis will cover a series of voting paradoxes—i.e., factual results that seem opposed to common sense (at least on first glance). I shall not examine Condorcet's cyclical-majority paradox in detail since it has received so much attention elsewhere.[5]

To counterbalance the primary emphasis on extreme possibilities, we shall present data from computer simulations designed to assess the likelihood of the several paradoxical occurrences. As might be expected, the general conclusion from these data is that the more extreme forms of the paradoxes are exceedingly rare in practice.[6]

For simplicity it will be assumed throughout that each voter's preference order is linear, so that he is never indifferent between distinct alternatives. The set A of social alternatives will be assumed to be finite. A summary of the extreme results follows.

1. *The dominated-winner paradox.* In sequential-elimination simple-majority voting the winner may be Pareto dominated by another alternative. Let $p(D)$ be the probability, given a profile D of individual preference orders on A, that the winner under sequential-elimination simple-majority voting will be Pareto dominated when each of the $|A|!$ orders of introduction for voting on the

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* I wish to thank Bruce McCarl for his assistance in writing the computer program used in the simulation analyses of this paper.

[1] This seems especially true when no alternative has a simple majority over every other alternative (Condorcet's famous paradox of voting). I shall argue later, however, that, even when there is a simple-majority winner, some other alternative might seem more attractive as the social choice.

[2] Jean-Charles de Borda, "Mémoire sur les élections au scrutin," *Histoire de l'Académie Royale des Sciences*, 1781 (translated, with commentary, by Alfred de Grazia, "Mathematical Derivation of an Election System," *Isis*, 44 [June 1953], 42–51); Marquis de Condorcet, *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix*, Paris, 1785. See Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1958) for discussion of these and other historical figures.

[3] A. H. Copeland, "A 'Reasonable' Social Welfare Function," mimeographed, University of Michigan Seminar on Applications of Mathematics to the Social Sciences, 1951.

[4] Peter C. Fishburn, "A Comparative Analysis of Group Decision Methods," *Behavioral Science*, 16 (Nov. 1971), 538–544; *The Theory of Social Choice* (Princeton: Princeton University Press, 1973); "Subset Choice Conditions and the Computation of Social Choice Sets," *Quarterly Journal of Economics*, forthcoming.

[5] Some key references that will lead to others are: William H. Riker, "Voting and the Summation of Preferences: An Interpretive Bibliographical Review of Selected Developments During the Last Decade," *American Political Science Review*, 55 (Dec. 1961), 900–911; Robert M. May, "Some Mathematical Remarks on the Paradox of Voting," *Behavioral Science*, 16 (March 1971), 143–151; and Part 3 in *Probability Models of Collective Decision Making*, ed. Richard G. Niemi and Herbert F. Weisberg (Columbus, Ohio: Charles E. Merrill, 1972).

[6] It is tempting to suggest that the more surprising the paradox, the less likely it is to occur in practice. This of course is a matter of personal judgment that the reader may wish to assess for himself.

alternatives is equally likely to be used. By appropriate choice of D and A, $p(D)$ can be made arbitrarily close to unity.

2. *The inverted-order paradox.* If the alternatives are linearly ordered by Borda point totals, and if the winner in A is removed and the process is repeated for the remaining alternatives, these alternatives may be linearly ordered by revised point totals in the exact opposite order from their original point-total order.

3. *The winner-turns-loser paradox.* There are voter profiles in which the unique Borda point-total winner in A is also the point-total winner in one situation which involves it and exactly one other alternative, but is a point-total loser in every other situation that involves it and at least one, but not all, of the other alternatives.

4. *The truncated point-total paradox.* Let B_k denote the truncated Borda procedure where k points are given for a first-place vote, $k-1$ points are given for a second-place vote, $\cdots$, 1 point is given for a kth-place vote, and zero points are given for votes thereafter. (B_1 is the plurality method, and $B_{|A|-1}$ is the Borda method.) There are voter profiles in which each B_k has a unique winner ($k=1, 2, \cdots, |A|-1$), no two of which are identical. Thus, with $|A|=10$, any one of nine of the 10 alternatives can win depending on which the nine B_k procedures is used.

5. *Condorcet's other paradox.* There are voter profiles for which one alternative has a strict simple majority over every other alternative and all but one of these other alternatives beats the simple-majority winner on the basis of every modified Borda point-total scheme that assigns more points to a first-place vote than to a second-place vote, more points to a second-place vote than to a third-place vote, and so forth.

The cyclical-majorities paradox and the paradoxes presented in this paper suggest that any more-or-less reasonable procedure for aggregating individual preferences to arrive at a social choice must carry with it criticisms that at least some viewers will find to be damning. The question may then be which procedure or class of procedures is best able to withstand adverse criticism. It is hoped that this paper will stimulate further thinking along these lines.

Preliminary Notation and Definitions

A few conventions used in the paper will be noted here before we proceed with the first paradox. Other notations will be introduced as needed.

A linear order on $\{x_1, x_2, \cdots, x_m\}$ that has x_1 first (most preferred), x_2 second (next most preferred), $\cdots$, and x_m last (least preferred) will be written as $x_1x_2 \cdots x_m$. The *backward cyclic list of orders generated by* the linear order $x_1x_2 \cdots x_m$ is the m-tuple of orders

$$C(x_1x_2 \cdots x_m) = (x_1x_2 \cdots x_m,\ x_mx_1 \cdots x_{m-1},\ x_{m-1}x_m \cdots x_{m-2},\ \cdots,\ x_2x_3 \cdots x_1),$$

in which each order is obtained from its predecessor by moving the last element in the predecessor to first place. The *forward cyclic list of orders generated by* $x_1x_2 \cdots x_m$ is

$$C'(x_1x_2 \cdots x_m) = (x_1x_2 \cdots x_m,\ x_2x_3 \cdots x_1,\ x_3x_4 \cdots x_2,\ \cdots,\ x_mx_1 \cdots x_{m-1}),$$

in which each order is obtained from its predecessor by moving the first element in the predecessor to last place. These cyclic lists play key roles in the proofs of later theorems.[7]

In general we shall let n denote the number of voters. We shall sometimes use m to denote the number $|A|$ of alternatives, but often $|A|$ will be some function of m. For example, $|A|=m^3+m^2+m+1$ is used in the proof of Theorem 1, along with $n=3$. In other proofs, n will depend on m or $|A|$.

A *profile* (voter profile) consists of an n-tuple of linear orders on the set A of social alternatives, one order for each voter. For a given profile, $n(x, y)$ is the number of voters who prefer x to y. Clearly $n(x, x)=0$ and, by the linearity assumption, $n(x, y)+n(y, x)=n$ when $x\neq y$. xPy means that x has a *strict simple majority* over y, or $n(x, y)>n(y, x)$. $x\gg y$ means that all voters prefer x to y, or $n(x, y)=n$. When $x\gg y$, we say that x *Pareto dominates* y.

The Dominated-Winner Paradox

The first paradox that we shall examine arises from Condorcet's paradox of cyclical majorities in the context of sequential-elimination simple-majority voting. Along with the assumption of linear orders for voters we shall assume that the number of voters is odd, so that xPy or yPx whenever $x\neq y$. (We assume also that a voter votes for x over y if and only if he prefers x to y.)

With respect to a linear order L on A, sequential-elimination simple-majority voting proceeds as follows: A simple-majority vote is taken between the first two alternatives in L. The loser of this contest is eliminated and a second simple-majority vote is taken between the winner and the third alternative in L. Continuing in this fashion, the surviving winner of the first $|A|-2$ votes confronts the last alternative in L in the final simple-majority vote. The winner of this vote is the *overall winner*.

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[7] Note that in $C(x_1x_2 \cdots x_m)$, x_1 has an $m-1$ to 1 majority over x_2, x_2 has an $m-1$ to 1 majority over x_3, $\cdots$, and x_m has an $m-1$ to 1 majority over x_1. The same is true for C' since its orders are obtained from a permutation of the orders in C.

As is widely known, the overall winner may lose on the basis of simple majority to one or more alternatives that were eliminated during the process. Under the assumptions used here, this can happen only if there are cyclical majorities. And in this case it is readily seen that the overall winner depends on the order L in which alternatives are introduced into the voting.

A less widely known fact is that, if $|A|>3$, the overall winner can be Pareto dominated by another alternative, so that every voter prefers x to y with y the overall winner. The earliest example of this "dominated-winner paradox" that I am aware of (see next paragraph) is given by Harary, Norman, and Cartwright, who for some reason do not remark that the overall winner is in fact dominated, although they do note that it has no first-place votes.[8] Explicit mention of the paradox has been made by the author.[9]

The key to the dominated-winner paradox is illustrated by four alternatives x, y, a, and b, with $x \gg y$, aPx, bPa, and yPb. If $L=xaby$, then a eliminates the alternative x that Pareto dominates y; b then eliminates a; and y eliminates b to emerge as the overall winner. A three-voter profile that gives this result is

1. $xyba$
2. $axyb$
3. $baxy$

which consists of the backward cyclic list $C(xyba)$ without its final order $ybax$.

An Extreme Result

Although the dominated-winner paradox might be extremely unlikely to occur in an actual situation, it is possible to construct profiles under which the paradox is almost certain to arise. We demonstrate this under the assumption that each possible order L for sequential elimination is equally likely.

THEOREM 1. *For a given profile D, let* $p(D)$ *denote the proportion of all* $|A|!$ *orders-of-introduction L for which* $x \gg y$ *for some x in A when y is the overall winner under sequential-elimination simple-majority voting according to L. Then, for any* $\delta>0$, *there exists an A and a three-voter profile D on A such that* $p(D)>1-\delta$.

For expositional convenience, this theorem along with some others presented later will be proved in the Appendix.

It is important to note that the theorem does not say that the likelihood of a dominated winner will be large for most profiles of individuals' preferences. To the contrary, we would expect the general likelihood of a dominated winner to be rather small, and this expectation is supported by the simulation results presented below. What the theorem does say is that, by appropriate and deliberate construction, it is possible to fashion a sequence of profiles with three voters and increasing numbers of alternatives so that, for *this* sequence, the likelihood of a dominated winner approaches unity as we go further along in the sequence.

Computer Simulation Data

The simulation data reported in this and later sections are based on 1000 randomly generated profiles for each of 12 combinations of specified numbers of voters and alternatives. Thus 12,000 profiles were examined altogether. The preference orders in each profile were generated independently of one another, with equal probability $(1/|A|!)$ for each of the $|A|!$ possible orders. The number n of voters took one of the three values in $\{5, 11, 21\}$, and the number of alternatives took one of the four values in $\{4, 5, 6, 7\}$.

Among the 4,000 profiles with five voters (1000 for each $|A|$ in $\{4, 5, 6, 7\}$), 287 (7 per cent) had no simple-majority winner *and* a Pareto-dominated alternative. These conditions are necessary (but not sufficient) for the occurrence of the paradox. Using a randomly-chosen order of introduction for sequential voting, exactly one of the 287 profiles gave an overall winner that was Pareto dominated. Thus the dominated-winner paradox arose once in 4,000 cases with $n=5$.

For 11 voters, the 4,000 profiles included five with no simple-majority winner *and* a Pareto-dominated alternative. With a randomly-selected order of introduction, none of these five had an overall winner that was Pareto dominated. For 21, the 4,000 profiles included none with no simple-majority winner and a Pareto-dominated alternative.

Hence exactly 292 of the 12,000 profiles had no simple-majority winner *and* a Pareto-dominated alternative. The paradox was observed only once in the 12,000 cases.

The Inverted-Order Paradox

We now switch attention from simple-majority to point-count procedures. We shall consider subsets of A in this section and the next in line with the possibility that any one of a number of such subsets of the universal set A could arise as the actual set of nominees or feasible alternatives in the situation at hand.

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[8] Frank Harary, Robert Z. Norman and Dorwin Cartwright, *Structural Models: An Introduction to the Theory of Directed Graphs* (New York: Wiley, 1965), 314.

[9] Peter C. Fishburn, "The Irrationality of Transitivity in Social Choice," *Behavioral Science*, 15 (March, 1970), 119–123, esp. p. 122; Fishburn, "A Comparative Analysis of Group Decision Methods," esp. p. 542.

For any nonempty subset B of alternatives in A and a given profile of individuals' preferences, the Borda point total for x in B *with respect to* B is defined by

$$s(x, B) = \text{sum of the } n(x, y) \text{ over the } y \text{ contained in } B.$$

When $|B| = m'$, $s(x, B)$ is the point total received by x when, for each voter, points of $m'-1$, $m'-2, \cdots, 0$ are assigned respectively to his first, second, $\cdots$, last alternative within B.

For a given profile and a proper subset B of A, it should be clear that the point-total ranking of alternatives in B as determined by the order of the $s(x, A)$ for x in B may differ from the point-total ranking of alternatives in B as determined by the order of the $s(x, B)$. For example, with x and y in B, we may have x ahead of y, or $s(x, A) > s(y, A)$, within the context of A as the feasible set of alternatives; but when alternatives other than x and y are removed from A (by disqualification, withdrawal, etc.) to give B, and when the process is repeated for B, we can get y ahead of x, or $s(y, B) > s(x, B)$, within the context of B as the feasible set.

This section and the next present paradoxes which demonstrate extremes of what can happen to certain alternatives' point totals when other alternatives are removed. The first of these is related to Davidson and Odeh's study of changes in the order of initial losers when the initial unique winner is removed.[10]

To illustrate it, let $A = \{x, a, b, c\}$ with the following seven-voter profile:

1. *xcba*	5. *axcb*
2. *axcb*	6. *baxc*
3. *baxc*	7. *xcba*.
4. *xcba*	

The first three orders in this profile consist of the three orders in the backward cyclic list $C(cba)$ generated by *cba*, with x inserted immediately before c in each. The second three orders are an exact copy of the first three. The final order is the same as the first and fourth. As one can readily verify, $s(x, A) = 15$, $s(a, A) = 10$, $s(b, A) = 9$, and $s(c, A) = 8$, so that the alternatives are ranked as *xabc* in the context of A.

Suppose then that x is deleted. The reduced profile on $B = \{a, b, c\} = A - \{x\}$ becomes

1. *cba*	5. *acb*
2. *acb*	6. *bac*
3. *bac*	7. *cba*,
4. *cba*	

with $s(c, B) = 8$, $s(b, B) = 7$, and $s(a, B) = 6$. Hence the B ranking for a, b and c is *cba*, which is the exact reverse of the ranking of these three in the A context.

A straightforward generalization of this example yields the following theorem.

THEOREM 2. *Suppose* $|A| \geq 3$, *with* $A = \{x, a_1, \cdots, a_m\}$. *Then there exists a profile for which* $s(x, A) > s(a_1, A) > s(a_2, A) > \cdots > s(a_m, A)$ *and* $s(a_m, A - \{x\}) > s(a_{m-1}, A - \{x\}) > \cdots > s(a_1, A - \{x\})$.

This is referred to as the inverted-order paradox since it says that the point-total linear order of losers in the context of A is completely reversed or inverted when the original winner is removed. A more cumbersome label is "the first shall be last and the last shall be first" paradox, referring to the fact that the first runner-up in A becomes last in $A - \{x\}$, the second runner-up in A becomes next-to-last in $A - \{x\}, \cdots$, and the last runner-up in A becomes first in $A - \{x\}$.

Charles Plott and John Ferejohn have suggested a dual form of this paradox in which the point-total order of the first $m-1$ candidates is reversed when the last-place candidate is removed. To illustrate this reversal, suppose seven men are nominated for the office of president in an organization. A Borda count is to be used, with the top candidate to be elected. Under an appropriate voter preference profile, and based on the rank-order ballots, the election committee determines the following point total order of the seven candidates:

Andrews:	President Elect
Brown:	2nd
Carter:	3rd
Davis:	4th
Evans:	5th
Foster:	6th
Gardner:	7th.

Before these results can be announced, the election committee is informed that Gardner has just died. Thus, acting in accord with the election rules, Gardner's name is deleted from the ballots and new point totals are computed, giving the following results:

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[10] Roger R. Davidson and Robert E. Odeh, "Some Inconsistencies in Judging Problems," *Journal of Combinatorial Theory* (A), 13 (1972), 162–169.

Foster:	President Elect
Evans:	2nd
Davis:	3rd
Carter:	4th
Brown:	5th
Andrews:	6th.

Although Kenneth Arrow uses an example such as this to defend the reasonableness of his condition of the independence of irrelevant alternatives (arguing that the death of one candidate should not alter the social ordering that obtains over the survivors), the example in no way violates his independence condition since *different* feasible sets or "environments" are involved in the two parts of the example.[11] In fact, our example holds faithfully to Arrow's independence condition since the "winner" in each circumstance was based only on voter preferences over the feasible candidates in the circumstance. Given the independence condition, what the example does violate is the dictum that the social choice from a larger feasible set of alternatives ought to be determined from the potential choices from two-element subsets of the feasible set which, in the case at hand, would be determined by simple majority, since the Borda method reduces to simple majority when it is applied to a two-alternative set.[12]

Simulation Results

Since the probability of observing the occurrence specified by Theorem 2 is vanishingly small within the context of n in $\{5, 11, 21\}$ and $|A|$ in $\{4, 5, 6, 7\}$, we looked instead for the Davidson-Odeh case in which (1) there are unique first-place and second-place point-total winners in A and (2) the second-place alternative in A loses to some other alternative when the first-place winner in A is removed.

Of the 4,000 profiles with five voters, 2,729 (68.2 per cent) had unique first-place and second-place point-total winners. Removal of the winner caused the original second-place alternative to become a loser in 57 (2.1 per cent) of these profiles.

Of the 4,000 profiles for $n=11$, 3,047 (76.2 per cent) had unique first-place and second-place point-total winners. Removal of the winner caused the original second-place alternative to become a loser in 161 (5.3 per cent) of these profiles.

Of the 4,000 profiles for $n=21$, 3,315 (82.9 per cent) had unique first-place and second-place point-total winners. Removal of the winner caused the original second-place alternative to become a loser in 236 (7.1 per cent) of these profiles.

The clear trend here is that (with a fixed number of alternatives) the likelihood of occurrence of the Davidson-Odeh case increases as the number of voters increases.

The Winner-Turns-Loser Paradox

The second point-total paradox which involves removals shows what may happen to the original point-total winner when original losers are removed. The rather long proof of the following theorem is presented elsewhere.[13]

THEOREM 3. *Suppose* $|A| \geq 3$. *Then there exists a profile in which* $s(x, A) > s(a, A)$ *for all a in* $A - \{x\}$ *and, for every B that is a proper subset of A and contains x and has* $|B| \geq 2$, *except for one such B with* $|B| = 2$, *there exists a in B such that* $s(a, B) > s(x, B)$.

This says that the original unique winner x can be a loser within every proper subset of A that contains x and at least one other alternative, except that x is the winner in exactly one such subset that contains x and another alternative. If $A = \{x, a_1, \cdots, a_{10}\}$, there are $2^{10} - 2 = 1022$ proper subsets of A that contain x and at least one other alternative. The winner in A can be a loser in all but one of these 1022 subsets.

It seems reasonable to refer to the paradox identified by Theorem 3 as the winner-turns-loser paradox. One immediate corollary of the theorem says that if x is the unique point-total winner in A, all but one of the other alternatives can have simple majorities over A.

Simulation Results

To examine the effect on the original point-total winner when one or more original losers are removed, we looked at the cases of (1) removing one loser and (2) removing all but one loser.

The results for our 12,000 randomly-generated profiles when one loser is removed are shown in Table 1. For each profile with a unique point-total winner, we removed one loser in each of the $|A| - 1$ possible ways and recorded the number of these (r) in which the original winner became a loser. The resultant frequencies for r (expressed as percentages) are shown in Table 1. For example, of the 837 profiles with unique winners for

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[11] Kenneth J. Arrow, *Social Choice and Individual Values*, 2nd ed. (New York: Wiley, 1963), pp. 26–27.

[12] For further discussion on this point see Peter C. Fishburn, "The Irrationality of Transitivity in Social Choice," and "Should Social Choice be Based on Binary Comparisons?," *Journal of Mathematical Sociology*, 1 (Jan. 1971), 133–142.

[13] Peter C. Fishburn, "On the Sum-of-Ranks Winner when Losers are Removed," *Discrete Mathematics*, forthcoming.

Table 1. Frequency of Profiles for Winner-Turns-Loser with One Loser Removed

$\|A\|$	n	Number of Profiles (out of 1000) with Unique Point-Total Winner	Percentage (to two places) of Profiles in Preceding Column in which Unique Winner was a Loser in r of the $\|A\|$-1 Contests Obtained by Removing One Loser			
			$r=0$	$r=1$	$r=2$	$r=3$
4	5	837	96 (%)	4.3	0	0
	11	881	93	6.7	.45	0
	21	914	86	13	.88	0
5	5	869	96	4.0	0	0
	11	895	88	11	.89	0
	21	917	84	14	2.0	.11
6	5	866	94	5.8	.35	0
	11	895	86	13	1.6	0
	21	932	82	14	3.2	.32
7	5	860	93	6.3	.47	0
	11	910	86	11	2.6	.33
	21	931	80	16	3.3	.64

$|A|=4$ and $n=5$, 96 per cent (801) never had the original winner a loser after removal of one loser, and 4.3 per cent (36) had the original winner becoming a loser in exactly one of the three subcases resulting from the removal of one loser. The .64 in the lower right corner of the table shows that, in .64 per cent (6) of the 931 profiles with a unique winner for $|A|=7$ and $n=21$, the original winner became a loser in exactly three of the six subcases resulting from the removal of one loser.

To the extent shown by Table 1, the likelihood that an original winner becomes a loser when one loser is removed increases as the number of voters increases (with the number of alternatives fixed). It may also be noted that, in every profile with a unique winner, this alternative was a winner in at least one of the $|A|-1$ contests resulting from the removal of one loser. (Theorem 3 says that this need not always be the case.)

The results of removing all but one loser are shown in Table 2. For each profile with a unique point-total winner we determined the number (r) of other alternatives that had a strict simple majority over the point-total winner. The table displays the frequencies of the several r values for each (n, $|A|$) pair. Thus for (n, $|A|$) = (21, 7), the

Table 2. Frequency of Profiles for Winner-Turns-Loser with All But One Loser Removed

$\|A\|$	n	Number of Profiles (out of 1000) with Unique Point-Total Winner	Percentage (to two places) of Profiles in Preceding Column in which Unique Winner Lost to r of the other $\|A\|$-1 Alternatives on the Basis of Simple Majority			
			$r=0$	$r=1$	$r=2$	$r=3$
4	5	837	86 (%)	14	0	
	11	881	81	18	.34	
	21	914	78	22	.77	
5	5	869	79	20	.81	0
	11	895	69	29	1.5	.11
	21	917	68	30	2.1	0
6	5	866	70	28	2.3	0
	11	895	66	30	3.9	.22
	21	932	63	33	4.7	.11
7	5	860	66	29	4.8	.12
	11	910	61	32	5.9	.44
	21	931	59	33	6.4	.75

point-total winner was the simple-majority winner in 59 per cent (547) of the 931 profiles with a unique point-total winner; the point-total winner lost to one other alternative by simple majority in 33 per cent (317) of the 931 profiles; it lost to two other alternatives by simple majority in 6.4 per cent (60) of the 931 profiles; and it lost to three alternatives by simple majority in .75 per cent (7) of the 931 profiles. An increase in the number of voters on Table 2 shows an increase in the likelihood that a unique point-total winner will lose to one or more other alternatives by simple majority.

The maximum number of other alternatives that can have strict simple majorities over the unique point-total winner is $|A|-2$. This maximum was observed in 10 of the 2,632 profiles in Table 2 with $|A|=4$; it arose once in the 2,681 profiles with $|A|=5$; and it was never observed for profiles with $|A|>5$. For $|A|>5$, no profile arose in which more than three other alternatives beat the point-total winner on the basis of simple majority.

The Truncated Point-Total Paradox

In the two preceding sections we considered the effects of removing one or more alternatives from A. In this section we consider truncated Borda procedures which involve all alternatives in A but assign points only to the first k alternatives in each voter's preference order. As noted earlier, the truncated point-total procedure for k is denoted by B_k. Procedure B_k assigns k points to a first-place vote, $k-1$ points to a second-place vote, $\cdots$, 1 point to a kth-place vote, and zero points thereafter. For a given $|A|$ there are $|A|-1$ B_k procedures, for $k=1$ (plurality), $2, \cdots, |A|-1$ (Borda).

THEOREM 4. *Suppose* $|A|\geq 3$. *Then there exists a profile for which each* B_k *has a unique point-total winner, and no two of the* $|A|-1$ *winners for* $k=1, 2, \cdots, |A|-1$ *are identical.*

For example, with $A=\{a_1, a_2, a_3, a_4\}$, it may happen that a_1 is the unique B_1 (plurality) winner, a_2 is the unique B_2 winner, and a_3 is the unique B_3 (Borda) winner. One seven-voter profile with this result is

1. $a_1 \quad a_2 \quad a_3 \quad a_4$
2. $a_1 \quad a_2 \quad a_3 \quad a_4$
3. $a_1 \quad a_2 \quad a_3 \quad a_4$
4. $a_2 \quad a_3 \quad a_1 \quad a_4$
5. $a_2 \quad a_3 \quad a_4 \quad a_1$
6. $a_3 \quad a_4 \quad a_1 \quad a_2$
7. $a_3 \quad a_4 \quad a_1 \quad a_2$.

For obvious reasons we refer to this as the truncated point-total paradox.

Simulation Results

The simulation results for each of the 12 $(n, |A|)$ combinations are displayed in Table 3. For each profile with a unique B_k winner for $k=1, \cdots, |A|-1$ we determined the number r of distinct alternatives in the set of winners for the $|A|-1$ B_k procedures. For $(n, |A|)=(21, 4)$, 674 of the 1000 profiles had a unique winner for each of B_1, B_2, and B_3. Of these 674, 76 per cent

Table 3. Frequency of Profiles for Distinct Winners with Truncated Point-Total Procedures

$\|A\|$	n	Number of Profiles (out of 1000) with Unique B_k Winner for each k	Percentage (to two places) of Profiles in Preceding Column with r Distinct Winners in the $\|A\|$-1 B_k Procedures			
			$r=1$	$r=2$	$r=3$	$r=4$
4	5	521	89 (%)	11	0	
	11	611	84	16	.33	
	21	674	76	24	.45	
5	5	470	87	13	0	0
	11	530	78	22	.57	0
	21	590	67	30	3.1	0
6	5	395	86	14	0	0
	11	455	73	25	2.0	0
	21	565	65	32	2.8	.18
7	5	375	85	15	0	0
	11	394	73	25	1.8	0
	21	473	63	32	4.4	0

(510) had the same winner for the three procedures; 24 per cent (161) had two distinct alternatives in the set of winners; and .45 per cent (3) had a different winner for each of the three B_k procedures. As would be expected, an increase in the number of voters increases the chances for each B_k to have a unique winner, and an increase in the number of alternatives decreases the chances for each B_k to have a unique winner. Like Tables 1 and 2, Table 3 shows that as n increases the likelihood of getting a larger r-value increases. The percentage of profiles that have more than one distinct alternative in the set of B_k winners more than doubles in going from $n=5$ to $n=21$.

Exactly five of the profiles for Table 3 exhibited the behavior noted in Theorem 4. All five arose with $|A|=4$ and n in $\{11, 21\}$, giving a different unique winner for each of B_1, B_2, and B_3. We observed only one profile where more than three B_k had different unique winners. This occurred with $|A|=6$ and $n=21$.

Condorcet's Other Paradox

In extolling the idea that an alternative should be the social choice when it has a strict simple majority over every other alternative, Condorcet presented an example designed to discredit Borda's point-total method and any method similar to it which assigned α_1 points for each first-place vote, α_2 points for each second-place vote, $\cdots$, and α_m points for each last-place vote when $|A|=m$, with $\alpha_1>\alpha_2>\cdots>\alpha_m$. His original example, reproduced in Black,[14] gives a profile for $|A|=3$ in which one alternative has a strict simple majority over each of the others, with the simple-majority winner losing to another alternative under every point-count (α) scheme. A simpler example with $A=\{w, a, b\}$ and $n=7$ is given by the following profile:

1. *wab*
2. *wab*
3. *wab*
4. *abw*
5. *abw*
6. *awb*
7. *bwa*.

Alternative w has a 4-to-3 simple majority over each of a and b; since a has three first-place votes, three second-place votes, and one third, and w has three first-place votes, two second-place votes, and two thirds, the α-scores of a and w are respectively $3\alpha_1+3\alpha_2+\alpha_3$ and $3\alpha_1+2\alpha_2+2\alpha_3$. Since the first of these exceeds the second whenever $\alpha_2>\alpha_3$, a beats w under every point-count scheme which has $\alpha_2>\alpha_3$.

We shall say that *x Borda dominates y within A*, and write $x>_A y$, if and only if x beats y on a point-total basis for *every* point-count scheme $\alpha=(\alpha_1, \alpha_2, \cdots, \alpha_{|A|})$ which has $\alpha_1>\alpha_2>\cdots>\alpha_{|A|}$. It may be noted that Borda dominance is "stronger" than Pareto dominance since $x\gg y$ implies $x>_A y$, but not conversely.

More dramatic examples of Borda dominance than that just illustrated can be constructed. For example, it is possible to construct a 101-voter profile on nine alternatives which include a and w so that w has a strict simple majority (51 to 50) over each of the other eight alternatives (including a) and so that the distribution of first-place, second-place, . . . votes for a and w are as follows:

place	*votes for a*	*votes for w*
1st	50	0
2nd	0	30
3rd	30	0
4th	0	21
5th	21	0
6th	0	31
7th	0	0
8th	0	0
9th	0	19.

Condorcet's feelings to the contrary notwithstanding, examples like this suggest that some cases which have a simple-majority winner do *not* represent the most satisfactory social choice. For if a is chosen here, no voter gets worse than his fifth choice and nearly half the voters get their first choice, whereas the choice of w gives no voter his first choice and gives 19 voters their last choice.

The occurrence of Borda dominance $>_A$ is specified precisely by the following theorem, which notes also a mixed form of this type of dominance.[15]

THEOREM 5. *With respect to a given profile on A, for each x in A let x^i be the number of ith-place votes for x and let $x(i)=\sum_{j=1}^{i}x^j$, for $i=1,\cdots,m-1$, where $|A|=m$. Then $x>_A y$ if and only if $x(i)\geq y(i)$ for $i=1,\cdots,m-1$ and $x(i)>y(i)$ for some i. Moreover, alternative y will lose to some other alternative on a point-total basis for every α for which $\alpha_1>\alpha_2>\cdots>\alpha_m$ if and only if there exists a real-valued function λ on A with $\lambda(x)\geq 0$ for all x in A and $\sum_A\lambda(x)=1$ such that $\sum_A\lambda(x)x(i)\geq y(i)$ for $i=1,\cdots,m-1$ and $\sum_A\lambda(x)x(i)>y(i)$ for some i.*

Note here that $x(i)$ is the number of voters who have x in ith place or before. For the preceding example, $(a(1), a(2), \cdots, a(9))=(50, 50, 80, 80,$

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[14] Duncan Black, *Theory of Committees and Elections,* 176–177.

[15] For a proof of Theorem 5 see Theorems 6.1 and 7.1 in Peter C. Fishburn, *Decision and Value Theory* (New York: Wiley, 1964).

101, 101, 101, 101, 101) and $(w(1), w(2), \cdots, w(9)) = (0, 30, 30, 51, 51, 82, 82, 82, 101)$.

An extreme form of Condorcet's other paradox is expressed in the following theorem.

THEOREM 6. *Suppose* $|A| \geq 3$. *Then there exists a profile for which* wPa *for all* a *in* $A - \{w\}$, *and* $a >_A w$ *for all but one* a *in* $A - \{w\}$.

With $A = \{w, b, a_1, \cdots, a_m\}$ and $m \geq 1$, this says that there is a profile in which w has a strict simple majority over every other alternative, and $a_j >_A w$ for $j = 1, \cdots, m$. The first example in this section proves the theorem for $|A| = 3$. A constructive proof for $|A| = 4$ is left to the reader. The proof for $|A| \geq 5$ is in the Appendix.

Simulation Results

The simulation for Condorcet's other paradox was designed to assess the degree of agreement between the Condorcet criterion and the Borda method as well as to test for occurrence of the paradox.

Of the 4,000 profiles for five voters, 3113 (78 per cent) had a simple-majority winner. Of these, the simple-majority winner was also a point-total winner in 2887 (93 per cent); 58 (1.9 per cent) of the profiles with a simple-majority winner contained a point-total winner that Borda dominated the simple-majority winner. In one of the 3113 cases the simple-majority winner was Borda dominated by a point-total loser: $|A| = 6$ for this case.

Of the 4,000 profiles for $n = 11$, 2978 (74 per cent) had a simple-majority winner, and 2700 (91 per cent) of these included the simple-majority winner among the point-total winners. Fifty-four (1.8 per cent) of the profiles with a simple-majority winner contained a point-total winner that Borda dominated the simple-majority winner. No profile arose in which a point-total loser Borda dominated the simple-majority winner.

Of the 4,000 profiles for $n = 21$, 3093 (77 per cent) had a simple-majority winner, and 2598 (84 per cent) of these included the simple-majority winner among the point-total winners. Ninety-six (3.1 per cent) of the profiles with a simple-majority winner had a point-total winner that Borda dominated the simple-majority winner. In seven of the 3093 cases (three each for $|A| = 4$ and $|A| = 5$, one for $|A| = 6$) the simple-majority winner was Borda dominated by a point-total loser.

These data suggest that the degree of discordance between Condorcet's criterion and Borda's method increases as the number of voters increases. This correlation suggests in turn that the likelihood of Condorcet's other paradox increases as the number of voters increases, but this hypothesis is only partially supported by the data, as can be seen from the rows in Table 4. Looking at the table the other way, we see that the likelihood of the paradox decreases with an increase in the number of alternatives for 21 voters, but similar behavior is not evident for $n = 11$ or $n = 5$.

Conclusions

The underlying theme of this paper is the liability of procedures for arriving at a social choice among a number of candidates or social alternatives to criticisms of their overall reasonableness. Such criticisms have been offered in the form of paradoxes which, although they may be unexpected and rare in practice, do in fact point out potential problems in the various procedures. It is hoped that this work will stimulate re-examination of these and other procedures used in voting situations and aid in the further development of analyses designed to weigh the various merits and shortcomings of alternative voting schemes.

Appendix

This Appendix outlines proofs of Theorems 1, 2, 4, and 6. The definitions used are given in the text. The critical role of cyclic lists (section 2) may be noted in each case.

Proof of Theorem 1

Let m be a positive integer, let $|A| = m^3 + m^2 + m + 1$, and consider a profile on A for three voters as follows:

1. $xy_1y_2 \cdots y_{m^3}b_1b_2 \cdots b_{m^2}a_1a_2 \cdots a_m$
2. $a_1 \cdots a_mxy_1 \cdots y_{m^3}b_1 \cdots b_{m^2}$
3. $b_1 \cdots b_{m^2}a_1 \cdots a_mxy_1 \cdots y_{m^3}$.

For each i, j, and k, $x \gg y_i$, a_jPx, b_kPa_j, and y_iPb_k, along with xPb_k and a_jPy_i. It is easily checked that some y_i will be the overall winner if the order L has the following properties:

(1) the last element in L is a y_i,
(2) the last element from $\{a_1, \cdots, a_m, b_1, \cdots, b_{m^2}, x\}$ in L is one of the b_k,
(3) at least one a_i follows x in L.

Assuming that all $|A|!\,L$ are equally likely, the probability that (1) holds is $m^3/|A|$, the probability of (2) given (1) is $m^2/(m^2+m+1)$, and the probability of (3) given (1) and (2) is $m/(m+1)$. Hence the probability that some y_i wins is at least $m^6/(m^3+m^2+m+1)(m^2+m+1)(m+1)$, which approaches 1 as m gets large.

Proof of Theorem 2

With $A = \{x, a_1, \cdots, a_m\}$ and $m \geq 2$, we construct a profile of $2m+1$ orders as follows. The first m orders in the profile consist of the orders in the backward cyclic list $C(a_ma_{m-1} \cdots a_1)$ generated by $a_m a_{m-1} \cdots a_1$ with x inserted immediately in front of a_m in each of these. The second m orders are an exact copy of the first m. The final order is $xa_m \cdots a_1$. For the first m orders the point total for x is $m(m+1)/2$, and the point total for a_i is $m(m+1)/2-i$. Hence $s(x, A)=2[m(m+1)/2]+m=m(m+2)$ and $s(a_i, A)=2[m(m+1)/2-i]+(i-1)=m(m+1)-i-1$. Hence $s(x, A)>s(a_1, A)>s(a_2, A)>\cdots>s(a_m, A)$. When x is removed, $s(a_i, A-\{x\})=2[(m-1)m/2]+(i-1)=(m-1)m+i-1$, so that $s(a_m, A-\{x\})>s(a_{m-1}, A-\{x\})>\cdots>s(a_1, A-\{x\})$.

Proof of Theorem 4

With $A=\{a_1, a_2, \cdots, a_m\}$ we construct a profile from the orders in the forward cyclic list generated by $a_1a_2\cdots a_m$ as follows:

$a_1a_2 \cdots a_m$ is used $n_1=m^3-1$ times

$a_2a_3 \cdots a_1$ is used $n_2=m^3-2$ times

$a_3a_4 \cdots a_2$ is used $n_3=m^3-3$ times

$a_{m-1}a_m \cdots a_{m-2}$ is used $n_{m-1}=m^3-m+1$ times

$a_ma_1 \cdots a_{m-1}$ is used $n_m=0$ times,

with $n_1>n_2>\cdots>n_{m-1}>n_m=0$ and $n_1+n_2+\cdots+n_{m-1}$ the number of orders in the profile. The B_k point total for alternative a_k is

$$
\begin{aligned}
s(a_k, A; (k, k-1, \cdots, 1, 0, \cdots, 0)) &= n_1+2n_2+3n_3+\cdots+kn_k \\
&= (m^3-1)+2(m^3-2)+\cdots+k(m^3-k) \\
&= k(k+1)m^3/2-k(k+1)(2k+1)/6.
\end{aligned}
$$

The B_k point totals for the other alternatives are

$$
\begin{array}{l}
n_2+2n_3+\cdots+kn_{k+1} \\
\vdots \\
n_{m-k+1}+2n_{m-k+2}+\cdots+kn_m \\
\cdots\cdots\cdots\cdots\cdots\cdots \\
n_{m-k+2}+2n_{m-k+3}+\cdots+kn_1 \\
\vdots \\
n_m+2n_1+\cdots+kn_{k-1}.
\end{array}
$$

Since $n_1>n_2>\cdots>n_m=0$, the B_k total for a_k exceeds each sum above the dotted line. Since $n_m=0$ has a positive coefficient for each sum below the dotted line, such a sum cannot exceed $k(k+1)m^3/2-m^3$. Hence the sum for a_k exceeds each of these if $m^3>k(k+1)(2k+1)/6$, which is true; therefore the unique B_k winner is a_k, so that no two B_k have a common winner.

Proof of Theorem 6 for $|A|\geq 5$

With $A=\{w, b, a_1, \cdots, a_m\}$ and $m\geq 3$, we construct a profile with $m(m+1)+1$ orders. The first $m(m+1)$ orders are formed from $m+1$ replications of $C(a_1a_2\cdots a_m)$ as follows. For all m orders in the jth replication of $C(a_1a_2\cdots a_m)$ insert w in the jth position and place b last, except for the jth order in $C(a_1a_2\cdots a_m)$ where bw is inserted immediately before a_1. For example, with $m=4$ and $|A|=6$, we get the following for the 3rd replication of $C(a_1a_2\cdots a_m)$;

$$
\begin{array}{cccccc}
a_1 & a_2 & w & a_3 & a_4 & b \\
a_4 & a_1 & w & a_2 & a_3 & b \\
a_3 & a_4 & b & w & a_1 & a_2 \\
a_2 & a_3 & w & a_4 & a_1 & b.
\end{array}
$$

This process is performed for the first m replications of $C(a_1a_2\cdots a_m)$. In the final replication, bw is placed last in each of the m orders. Finally, the order $wa_1a_2\cdots a_mb$ is adjoined to the other $m(m+1)$ to complete the profile. It is easily checked that, for each a in $\{a_1, \cdots, a_m\}$, $n(w, a)=1+m(m+1)/2$ and $n(a, w)=m(m+1)/2$, so that wPa. And $n(w, b)=(m-1)m+1$ and $n(b, w)=m+m$, so that wPb whenever $m\geq 3$. It also follows from the construction that $a_j(i)=w(i)=mi$ for $j=1, \cdots, m$ and $i=1, \cdots, m$, and that $a_j(m+1)=m(m+1)$ and $w(m+1)=mm+1$. Hence $a_j>_A w$ for each a_j, according to Theorem 5.

Table 4. Per cent Occurrences of Condorcet's Other Paradox[a] Within Profiles with Simple-Majority Winners

		n		
		5	11	21
$\|A\|$	4	2.2	1.3	5.1
	5	1.6	2.3	3.3
	6	2.1	2.4	2.5
	7	1.6	1.4	1.3

[a] Simple-majority winner Borda dominated by another alternative.

Transcending Tragedy: The Idea of Civility

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One of the most serious questions arising from the disappointing and tragic events of this century is how man—not merely man generically, but every individual human being—can participate in history. The view that seems to have been increasingly predominant since the Renaissance is that man participates in history through controlling it, through directing it toward his own ends; each individual can be given a hand in affairs by means of democracy. The very fact that our times have been disappointing and tragic, however, casts doubt on the possibility of controlling history. Some say, of course, that when certain conditions, such as further development of the behavioral sciences or intensification of the contradictions inherent in capitalism, have been realized, then history will fall under human control. It is never explained, however, how man can escape from historical immanence, from the immersion in events by virtue of which unforeseen consequences are attached to his every act. Nor do such assurances of coming mastery remove the suspicion that it is in man's nature to misuse the powers he possesses, so that not only his immanence but his transcendence as well is a source of trouble. Hence one may surmise that recent historical reverses, climaxing the self-confident activism of the eighteenth and nineteenth centuries, are not just accidents of a sort that we can readily enough avoid in the future, but that they derive from perdurable ontological and moral conditions.

In short, the established answer to the question as to how man can participate in history has been undercut. The purpose of this essay is to suggest an alternative answer.[1]

The Communal Impasse

Modern thinkers, and perhaps from time to time even large multitudes, have envisioned man as a bearer of vast historical responsibilities; he must create a new humanity, or he must save the humanity that has developed during the past two or three thousand years. Participating in history thus has meant accepting certain historical goals, these to be achieved by human planning and activity. This statement is plainly applicable to almost all types of radicalism. What has to be noted here is its applicability to conservatism and even, in limits, to liberalism.

Conservatism has not by and large attacked the idea that man can set and deliberately reach historical goals. The basic conservative argument has been that the particular goals held by radicals are wrong and that radical strategy, relying primarily on revolution, promises historical disaster. But conservatives have had their own goals, such as the preservation of the established order, and a strategy of historical control in which they have placed their confidence, that of cautious, piecemeal change. Considering the irresistible and torrential character of historical change during the last two or three centuries, conservatives have hardly been less bold in their designs of historical mastery than have their radical opponents.

Liberalism has perhaps been less consciously historical than radicalism or conservatism have been. The liberal outlook typically reflects a serious concern, however, with man's historical role. This comes about through faith in progress. It may seem that to rely on progress, which is presumably spontaneous, is to assume that deliberate human control is not needed. This is true, and liberals accordingly have not ordinarily called on men collectively to define and work toward historical goals. This sharply differentiates them from typical radicals and conservatives. The difference thus established is not total, however. Liberals have certain historical goals, such as increasing rationality, harmony, and freedom, and, in their view, everyone engaged in vigorous and reasonable individual activity—in business, for example—is contributing to the attainment of those goals. Hence man is not without power over history, nor is he without historical responsibility, but the consequences of his acts are shaped and fitted into a scheme of progress by forces working spontaneously.

On these grounds it can be said that the key idea in modern historical attitudes is that of action. Through action man participates in history. For some, such as Leninists or elitist liberals, only a few can act effectively and thus become historical participants. For others, it is possible for everyone to engage in historical action; many socialists exemplify this attitude.

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[1] While the idea of civility does not fall within the usual boundaries of the philosophy of history, it is closely related to that subject; most philosophers of history deal, at least implicitly, with the issue of man's participation in history. The literature in this area of course is vast. As examples of the kind of works that I have found useful in developing the concept of civility I might cite Rudolf Bultmann, *The Presence of Eternity: History and Eschatology* (New York: Harper & Brothers, 1957), and Karl Jaspers, *The Origin and Goal of History*, trans. Michael Bullock (New Haven: Yale University Press, 1953).

The idea of action typically has two corollaries, although these are less prominent among liberals than among radicals and conservatives. One of these is that if man is to act effectively in history, his way must be illuminated by true ideas; the whole truth concerning man's historical goals and the way to reach them, it is thought, is contained in a particular set of ideas. In brief, the effort to control history gives rise to ideology. In any circumstances human beings are apt to quarrel about the truth. But when man sees himself as the lord of history he is apt to be particularly anxious to identify and spread the ideas that will rightly guide historical action. Thus it is not altogether coincidental that our time is, in its preoccupations, both uniquely activist and uniquely ideological.

The second corollary of the idea of historical action is that to act effectively one must commit himself, not only to a certain ideology but also, to an appropriate group. One cannot enter into history alone, it is assumed, and just as there is one true ideology so there is one group, or one set of groups, that represents historical destiny. This might be a conspiratorial party, a set of traditional leaders, or a broad political movement.

These remarks I believe accurately describe a certain dominant mood, or tendency. I readily admit that the streams of thought always contain many small and subtle currents and that what I have said constitutes at best only a very general characterization. Nevertheless, the character and fate of a dominant mood can be very important to us, and perhaps for this reason these broad strokes are justified. This brief apologia may appropriately be applied also to what immediately follows.

The problem with which this essay is concerned arises from the apparent fact that the modern mode of participating in history has failed. It has proved impossible to set, and deliberately to reach, specific historical goals. This is not to say that modern man has failed in every way, or that contemporary life is nothing but misery and disorder; it is simply to say that practically nothing has turned out as planned. This is true of revolutions, such as those in France and Russia, of reform movements such as European socialism and the New Deal, of efforts to secure world order and peace, and of almost every large-scale historical undertaking. It is even true of conservatism, which, as seems apparent in the precipitate changes engulfing almost all societies, has failed as definitely as has radicalism. Man has proved incapable of directing the course of history.[2]

This seems to be the case regardless of the goals that have been set. As a matter of fact, however, one very general goal has been predominant, and that makes it possible to characterize our situation somewhat more precisely. That goal is community.

It cannot be claimed that community has meant the same thing to all parties, or even that all parties, without exception, have been motivated by the ideal of community. Increasingly during the last two hundred years, however, political thinking and action have been concerned with bringing persons together in their essential being. Like the commitment to historical action, the concern for community is something that conservatism typically shares with radicalism. Of course a certain kind of American conservatism has been individualistic, but that so-called conservatism is really a type of liberalism; the value that classical European conservatism has set over against the community of the future delineated by radicalism is not the individual but the community of the present and the past. Burke was no more individualistic than Rousseau. Liberalism has been another matter; its central idea is perhaps simply that of guarding the individual. But liberalism has fallen under the sway of the communal ideal. The immensely influential figure of Thomas Hill Green, whose philosophy was at once one of liberty and of community, may be taken as a sign of this fact.

Community is a tragic ideal, however, for human beings are not separated by inconsiderable and temporary conditions: they are separated by space, time, death, and their persistent selfishness and pride. These are not obstacles of a kind that can be overcome by political action. Arrangements favoring communication and other forms of relatedness can be made, and these can be important. But we do not thus reach the fullness of community; rather we experience, in some form, the tragic imperfection of every earthly association. There is an impassable chasm between political action and community. When all favorable arrangements have been made, political man finds himself standing before the abyss of death and freedom. Death renders the separations owing to space and time forever irreparable. Freedom is the condition both of iniquity and of community, and a politics that did away with iniquity (if such were devised by behavioral science) would at the same time remove the possibility of community.

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[2] Several twentieth century thinkers have developed able critiques of modern man's self-confident activism. Examples are Reinhold Niebuhr, especially in *The Irony of American History* (New York: Charles Scribner's Sons, 1952); Karl Popper, in *The Poverty of Historicism* (Boston: Beacon Press, 1957), as well as in *The Open Society and Its Enemies* (London: Routledge & Kegan Paul Ltd., 1945), 2 vols.; and Jacques Ellul, in *The Political Illusion*, trans. Konrad Kellen (New York: Alfred A. Knopf, 1967), and *Autopsy of Revolution*, trans. Patricia Wolf (New York: Alfred A. Knopf, 1971). Such writers have contributed much to developing the grounds of the idea of civility; but none of them have gone far toward developing the concepts of personal autonomy and universal communality which, as I shall try to show in the following discussion, are essential elements in civility.

Hence the miscarriage of man's aspiration to historical mastery is owing not only to the conditions mentioned earlier—the historical immanence that renders perfect foresight in principle impossible, and the persistent human tendency to misuse all power. It is owing also to the very nature of the predominant goal. Community cannot reasonably be an immediate object of action. Only arrangements favoring community, but assuring nothing, can be such an object. Our present historical situation may, accordingly, be termed a "communal impasse."

What makes this situation particularly serious is not only that the obstacles we have encountered are insuperable but that they lie on a road that man is bound to travel. Community is not merely one among a number of desires, and consequently a goal that can be given up if it proves unattainable. The extent to which thought and action during the past two centuries have been engrossed with the ideal of community, and the fact that this ideal was a secularized version of an ancient goal, indicates that the project of uniting all human beings, and even all creation, is compelling and inextinguishable. If it is, then giving up all hope of community must mean a fall into despair.[3]

This is why the problem of man's relationship with history cannot be resolved merely by being more sensible and more moderate. The communal impasse requires fundamental thought. What is necessary first of all is to define the concept of participation in history. It will not be surprising if it looks to the reader like a grandiose and empty generality. What does it mean?

Participation in History

As a first step toward answering this question, it may be said that participating in history means realizing one's historicity. It may help, as the discussion proceeds, to keep in mind the concept of historicity, despite the unusual and even bizarre appearance of the term.[4] Historicity is historical actuality and is a quality of anything that has existence in history. Thus all human beings are characterized by historicity, and a number of nonhuman realities are as well; both Alexander the Great and Bucephalus possessed historicity. Saying this, however, brings an important distinction before us. Bucephalus *had a part* in history, but we cannot say of the horse, as we can of Alexander, that it *participated* in history. To participate is to have a part consciously and deliberately—to *take* part. It may be said, then, that participation in history is conscious and deliberate historicity.

This suggests that participating in history means living through one's destiny, not as a horse might but, as only a human being can; it means, moreover, living through a destiny that is uniquely human, in that only man has a history. Why should one seek to do this? The answer, I suggest, lies in the same thirst for unity that gives urgency to the ideal of community: one wishes to link himself, so far as possible in view of the dispersion of mankind through space and time, with all human beings.

Today a great deal is said about the importance of "knowing who you are," with the corollary that only when this has been accomplished can one enter into relations with others. Personal identity thus is seen as prior to community.[5] Although people holding this idea often seem to think of themselves as progressive in their thinking, and independent of the past, in truth their views represent merely a new version of the old individualistic assumption that individuals are complete apart from society; satisfaction, as well as various more limited utilitarian benefits, may be gained from society, but not personal identity itself.

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[3] The most powerful commentary on the dilemma in which man is placed by his unquenchable will to community and the insuperable limits on his powers of action is to be found, perhaps, in the novels of Dostoevsky. It is often assumed that while Dostoevsky was profound in his psychology he was mad in his politics; supposedly he was a nationalistic and theocratic reactionary. This assessment is quite unbalanced, however. As is shown in his major political testament, "The Legend of the Grand Inquisitor," the key to his political thought is the idea of freedom; and while he did not develop a positive concept of civility, he was an exceedingly powerful critic of incivility.

[4] Jaspers has devoted considerable attention to something he calls "historicity." See his *Philosophy,* trans. E. B. Ashton (Chicago: University of Chicago Press, 1970), II, 104–129. Jaspers uses the term very broadly, so that it usually seems to denote man's embeddedness not merely in historical events, properly speaking, but in any particular and unrepeatable set of circumstances.

This individualistic viewpoint is dubious, however. Doubt is cast on it by some of the greatest names, and most powerful movements, in modern social and political thought—by Hegel and idealism, by Marx and various types of radicalism, by Burke and the European style of conservatism associated with his name, and finally by Martin Buber and the kind of existentialism that argues the possibility of "intersubjectivity." Doubt is cast on the notion that discovering who you are is a prerequisite for satisfactory relationships also by a common personal experience: that you discover who you are only *by means of* entering into relationships. And the verdict of personal experience can be verified by a rational test, namely, that it is impossible to describe a particular human being except in terms of relationships—occupation, social status, interests, and so forth. To be, it seems, is to be related, and to possess a certain identity is to be situated in a certain way within a network of relationships.

Regarded from this standpoint, participation in history may be seen as the realization of universal relatedness, as a conscious and deliberate relationship with the whole of humanity ("the whole of humanity" here ought to be understood not as referring to a particular objective entity but rather as symbolizing the term of a relationship which is not in any way exclusive). So far as "knowing who you are" is important, the value of such participation would lie in the discovery of an identity in no way truncated or prematurely defined.

But how is such relatedness possible? This is the main question to be dealt with in the following pages. But we cannot even approach it without trying to clear away an obstacle which recent thought puts in the way. This is the notion that what one needs from others, more than all else except for bare physical security, is to be "recognized." It is often said that only through being recognized can one lay sure hold on his own identity, and the supposed necessity of receiving recognition is made into the fundamental imperative underlying human relations. This notion leads logically, however, to the idea that only face-to-face associations are of ultimate importance, for in most circumstances only intimacy permits one to be sure that he is recognized. Correspondingly, it becomes difficult to see how historicity, which presumably consists in a relatedness of some other kind than that inherent in being recognized, can have much value.

Is it true that being recognized is in some way indispensable to one's humanity? It is doubtful that it is. Perhaps the two most authoritative archetypes of human excellence in Western culture are Socrates and Jesus. Neither were recognized, except by a very few. What distinguished them, rather, was their recognition of others—on the part of Socrates, through unreserved availability for discourse, on the part of Jesus, through universal love. No doubt it is burdensome and discouraging to be unrecognized; to hold that it is inevitably destructive of personal identity, however, seems to invert the wisdom symbolized by Socrates and Jesus. The possibility that this wisdom is unreal cannot of course be eliminated here. It can be pointed out, nevertheless, that even common sense seems to endorse the conception that man is independent of recognition. Is it not commonly held that to pursue creative work, particularly of an artistic or literary nature, one must be able to endure obscurity and that recognition is apt to come late? And can one deny that there is substance in this idea, however trite and overdrawn the pictures of the unknown, struggling genius often are?

It is noteworthy that even Hegel, whose famous "lordship and bondage" section in the *Phenomenology of Mind* is perhaps the major historical source of the doctrine that man needs to be recognized, seems finally to adopt the contrary view.[6] The struggle which leads to the differentiation of lord and bondsman is a struggle for recognition; thus it seems to be suggested that the central issue of social life is that of gaining not wealth or power but recognition. Both sides fail, the lord because in enslaving his opponent he has deprived him of his capacity for according valid recognition, the bondsman because he has lost the struggle. The bondsman, however, who is immediately and manifestly denied recognition, is the one who represents the future of humanity; and this is not because, through some dialectical reversal, recognition finally is gained, but rather, Hegel seems to say, through the sufferings that are inherent in his bondage. Thus in some sense, humanity arises from obscurity—a point explosively restated by Karl Marx.

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[5] For an intelligent expression of this view see R. D. Laing, *The Divided Self: An Existential Study in Sanity and Madness* (Baltimore, Maryland: Penguin Books, 1965).

[6] See G. W. F. Hegel, *The Phenomenology of Mind*, trans. J. B. Baillie, second edition (New York: Macmillan, 1931), pp. 228–240.

It may be permissible, then, to proceed on the assumption that what is morally decisive is, rather than being recognized, the act of granting recognition. If we do proceed on this assumption, then the idea of participating in history comes more clearly into view. It can be construed as comprehensive recognition. One realizes his historical actuality in recognizing the historical actuality of others—their freedom, their finitude, and the conditions and events which have formed their lives.

The reason why man must try to do this also becomes more apparent when recognizing others, rather than being recognized, is stressed. It may be granted that, owing to its all-inclusiveness, participating in history is necessarily a highly imperfect way of according recognition; it would seem that another is recognized fully only in personal love. It may be granted also that as between the universal and the immediately personal relationship, the former is more dispensable. All of us have known people, holding egregious political opinions, who have nevertheless been warm and unselfish in their personal relations, and probably all of us have sensed that the political opinions were somewhat redeemed by the personal relations whereas, conversely, humane political opinions are apt to seem meaningless or hypocritical if accompanied by personal coldness or selfishness. We may excuse someone for hating a nation or a race, but only when we feel that the hatred is owing to ignorance and is purely abstract, without consequences for personal relations.

What this suggests is that recognition granted to mankind at large is a kind of completion of personal love. What is crucial, to be sure, is recognizing those encountered face-to-face. But those few, after all, are only the ones we have accidentally encountered out of the billions who have lived within history; and they themselves are historical beings, possessed of the freedom that is the source of history and shaped by the traditions, and burdened by the conditions, that history creates. Hence there is a perilous incongruity in the lives of those who are historically irresponsible although personally humane. It is not quite enough to say that having become linked with man through the personal love felt for a few, one is logically linked with all human beings. The few who are loved are historical beings and thus only through the realization of historicity are even those few fully recognized.

The reader may feel that it is still quite unclear as to just what recognition means or how historicity can be realized. When these matters are clarified, however, the problem with which we began will have been resolved.

The Nature of Civility

The problem before us, it may be well to recall, is that modern man has conceived of historical participation in terms of action and control, and that our failures along these lines make it incumbent on us to ask anew how participation in history is possible. This essay has the aim of formulating an hypothesis that will help in doing this. I shall try to follow a clue to be found in *The Republic* at a point where Plato, in a mood of historical discouragement, observed that if the ideal city cannot become an actual historical city, it might at least be built here and there within single human beings. From the apparent impossibility of a perfect historical polity Plato thus appealed to the possibility of a perfect inner polity. What I wish to propose, in pursuance of this hint, is a stance that is personal, in being a possible object of personal choice, regardless of the attitudes of others, and that is simultaneously political. This stance may be termed "civility."[7]

Before trying to define civility systematically, it may be useful to sketch the idea in a preliminary fashion. This can be done by noting the position into which an individual is seemingly forced by the communal impasse.

The first aspect of this position that comes to attention is that of standing off from history. One is compelled to acknowledge the limits and the unreliability of action and thus to adopt, in some form and degree, a posture of inaction. This, as we shall see, does not mean total withdrawal; programs may still be formulated, and courses of action still pursued, but one is bound to regard them skeptically and to take care not to ground his own personal being in their success. Moreover, since the corollaries of historical activism have been ideological and group commitment, one has to stand off from history in other ways as well.

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[7] The main philosophical roots of the idea of civility, as I understand it, are to be found in the writings of certain philosophers who have maintained the ontological primacy of persons. In my own case Nicolas Berdyaev and Karl Jaspers have proved particularly illuminating. I would direct the reader's attention especially to Berdyaev's *Slavery and Freedom*, trans. by R. M. French (New York: Charles Scribner's Sons, 1944), and to Jaspers's *Philosophy*, 3 vols. It is to be noted, however, that neither writer applied his "personalism," as it might be called, very clearly in the realm of politics. A writer such as Jean-Paul Sartre also, of course, is a "personalist," and in addition is far more politically conscious than were either Berdyaev or Jaspers. But I am doubtful—and here we touch on an issue too large to be explored in this paper —that Sartre's atheism provides grounds for the personal autonomy on which civility depends; in other words, I suspect that there are metaphysical grounds for the fierce, lifelong activism that has made Sartre politically fascinating but often not very civil.

Since community is not an historical possibility, and cannot reasonably be an immediate object of action, no idea or system of ideas can either show forth the nature of earthly community or prescribe the ways of attaining it. This means that most, if not all, ideologies are in some measure false. One may continue to prefer one ideology to another, but, once the communal impasse has been faced, he can only do so realizing that at best the preferred ideology is a symbol of intuitions and hopes that cannot be fully translated into either institutional or conceptual form. As for groups, a simple truth can be derived from our situation: that no group is a community and no group can create a community. Some groups are no doubt historically useful (and others harmful), but none deserve to be regarded in the way the working class and the Communist Party have been regarded by many people since the time of Marx; none show how man's communal destiny, that is, his inevitable orientation toward community, can be fulfilled. The same sort of thing can be said about leaders. One could hardly take part in political life without adhering at least tentatively to certain leaders; but to think that any leader can create a community is to make a god out of a man.

Thus the foundering of our historical efforts forces us to place a certain distance—constituted of doubt and of limits on allegiance—between ourselves and historical phenomena.

Distance is not total separation, however. Indeed, in physical matters the word "distance" designates a relationship; and so it is here. In the first place, the immanence which bars man from controlling history as he desires prevents him also from withdrawing from it. Unavoidably each one is a part of history, with not only the circumstances of his life but his very self being historically formed. Thus to respond to historical crisis, as the Epicureans did to the downfall of the city-state in the fourth century B.C., by prescribing total withdrawal, is delusory. If man could stand outside of history, then he could stand above it and control it.

In the second place, and perhaps more important for the present argument, total withdrawal is barred by man's communal nature. Since community is not one desire among others, it cannot simply be set aside. We are bound to enter into relationships wherever we can, although these can never constitute anything with the durability and fullness connoted by the word "community." This is why we must be concerned with realizing our historicity, even though we find history to be tragic.

The relationships into which we must enter, moreover, cannot be altogether those of pure interpersonal communion. We must descend into action. Even though community cannot be created through action, the absence of community necessitates action by giving rise to disorder. In a perfect community, all would come about through dialogue; where community is lacking, order must be politically contrived. And for another reason as well, complete inaction is barred: a person must act in order fully to be. As long as one merely thinks and speaks, he is not fully real, for he only entertains possibilities and has no public reality. To be sure, once he has committed himself to a course of action, then through hesitations and regrets he transcends his commitment and becomes something more than his public self. But where there is no responsibility for a public position, discourse and thought become ways of indulging in the luxury of nonexistence.

If one cannot completely give up action, neither can he completely give up ideologies and groups. Here also "distance" becomes a term describing a relationship. It appears, then, that the communal impasse gives rise to a self-contradictory position, that of a withdrawal one is bound to compromise. Such contradictions are not unheard of in the moral and religious traditions of the past. Paul urges that "they that have wives be as though they had none; and they that weep, as though they wept not; and they that rejoice, as though they rejoiced not; and they that buy, as though they possessed not; and they that use this world, as not abusing it."[8] If such paradoxical possibilities can be granted, then we may say that the communal impasse seemingly requires that one act in a spirit of inaction—an idea making up one of the main themes of the *Bhagavad-Gita;* that one accept an ideology while at the same time doubting it—something called for by so sane a thinker as John Locke;[9] that one adhere to groups as though he were alone; and that he plan for the historical future as a way of living in the present.

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[8] I Corinthians 7:29–31.

[9] See John Locke, *A Letter Concerning Toleration*, in *John Locke on Education and Politics*, ed. Howard R.

Civility is the stance in which one lives and sustains these tensions. This at any rate may serve as a preliminary definition. So stated it shows the logic of civility (along with its paradoxes), that it is not merely a personal reaction but that it derives logically from man's nature and present situation.

Let us proceed to a more systematic definition. In doing so, however, one important proviso must be stated; that is, that the definition must remain very general. Readers may ask exactly what one must do, along the lines of voting, holding office, and so forth, in order to realize civility; they may wonder at times whether it comes down to anything more than regularly reading a good newspaper. Such questions are quite reasonable, but they cannot be answered in an essay as short as this one and cannot, in any final fashion and through stating general principles, be answered at all. Civility is necessitated partly by the impossibility of subsuming man and history under rules. Consequently, it must be conceived of as creative. Only a civil person can fully define civility, and his definition can only be one that is incarnate in his own life and is irreducible to detailed principles claiming universal validity.

One thing can be definitely said, and that is that the standard of civility is not primarily one of power, for a single individual in average cases can achieve only a negligible degree of power. What then is the standard? This brings us to the problem of defining civility systematically.

It will be safer, I believe, rather than trying to reduce the idea to a single proposition, to define it in terms of its different constituent elements. These elements, three in number, emerged above in considering the position into which we are forced by the communal impasse. What follows, therefore, is a more exact and systematic delineation of that position than seemed appropriate in first taking note of it. I shall discuss the three elements separately and in turn.

(1) *Historical autonomy.* One cultivates civility when the primary question in every historical situation is not, "How can such and such an historical goal be reached?" but rather, "How should I bear myself?" With this query one recognizes what is within the scope of human power and of his own power in particular. At the same time he detaches himself from all absolute commitments and establishes the possibility that he will refuse a proposed action or break with a certain ideology or group. This is not necessarily to become indifferent to historical results or to refuse historical responsibility. On the contrary, autonomy may enhance one's consciousness of his responsibilities, for these can no longer be fulfilled through adhering thoughtlessly to an ideology or accepting automatically the demands of certain groups and leaders. Historical autonomy was apparently what Plato was thinking of after recognizing that the ideal city probably never would become a real city; although the world makes creative statesmanship almost impossible, there remain possibilities of inner statesmanship.[10]

This attitude no doubt risks a kind of selfishness, that of caring for nothing but one's own rectitude. It does, however, meet two moral demands arising from the communal impasse. One is that of acknowledging that political action is inescapably objectification; to act on others is to view them from the outside and, even if the end be some conception of their own welfare, to reduce them to means. This is the reason for the chasm between political action and community. As already pointed out, civility does not mean wholly abstaining from action; that would turn it into anarchism. It does, however, mean acting with an awareness that action is inherently depersonalizing and thus provides no direct access to the kind of unity man seeks.

The second moral demand met by historical autonomy is that of giving up the pride inherent in thinking man can control history. One grants the uncertainty of the future and concentrates, in pursuance of Plato's suggestion, on the governance of his own soul. It would be another kind of pride to assume that while man cannot dominate history, each one can readily govern himself. But it belongs to man to undertake the latter, whereas seeking to dominate history expresses cosmic presumption.

The refusal of pride may be seen, alternatively, as a refusal of idolatry. At least in most cases this is valid. There is perhaps a demonic pride which scorns allegiance to anything what-

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Penniman (New York: D. Van Nostrand, 1947), pp. 17–68. The idea that one should simultaneously believe and doubt is implicit in Locke's injunction to realize that you may be wrong.

[10] Of course, the pre-eminent philosopher of autonomy is Kant. Kant's whole dualistic outlook, structured by the dichotomy between nature and freedom, is perhaps the firmest philosophical ground for the idea of civility. It is noteworthy that both Berdyaev and Jaspers were self-professed Kantians.

ever beyond the self; the terrible figure of Stavrogin, in Dostoevsky's *The Possessed,* is a picture of this kind of pride. Few people, if any, however, have Stavrogin's strength, and thus pride rarely if ever takes the form of absolute self-assertion. It usually means reliance on something that man has made, that is, idolatry. Thus the pride of thinking that man could dominate history has been equivalent to the idolatrous exaltation of parties and ideologies. In this light, civility becomes a rejection of finite gods, an insistence on leaving oneself free for the totality of the human.

Historical autonomy clearly must entail a readiness to disobey and resist. Some of the clearest examples of civility may be found among individuals such as Thoreau, who are independent and courageous enough occasionally to rebel, rather than among the immense law-abiding majority. Nevertheless, civility should not be identified with habitual resistance. There could be no order, and hence no world in which civility is possible, unless everyone normally obeyed.

Let us turn to the second element in civility.

(2) *Comprehensive communality.* This is perhaps a barbarous phrase, but it accurately denotes a certain way of relating oneself to mankind. To be comprehensively communal is, in the first place, to be unreservedly attentive, not only to be attuned to man's voice wherever it can be heard, but to search out unvoiced experiences. Thus we can see civility in Dr. Gachet's support for Cezanne and Van Gogh when they were unrecognized and maligned, in Dostoevsky's ability to enter into the minds of criminals, in R. D. Laing's sympathetic sense of what it means to be schizophrenic, and in the careful and penetrating reports of the Poor Law Commissioners in nineteenth century England. We can see civility in Abraham Lincoln's awareness that his enemies in the South were human beings like those in the North, fallible, but in most cases doing what they thought was right. Such attentiveness expresses a concern for all human experience, an unwillingness that anything should remain unheard and unknown.[11]

It must be carefully noted, however, that being attentive is very different from merely being observant, and that attentiveness looks for something more than explanation. Attentiveness is intersubjective, whereas observation and explanation, like action, objectify. Perhaps the point I would like to make can be elucidated through considering the example of Marxist thought. There is little doubt that Marxist concepts help to explain many aspects of social and political behavior, and it may well be that no other theory has been so successful in this respect. But the very successes of Marxism have helped to produce what is surely its over-all failure, that it has not, as Marx intended, guided mankind into community. It is not just that the successes of Marxism have encouraged its followers to scorn all other theories, although that has happened and is part of the problem. These successes have encouraged Marxists to reject all serious, inquiring discourse. Marx's followers have felt themselves able totally to explain why many people disagree with them and thus have felt no obligation to put themselves in the place of those people or seriously to consider the possibility that they have spoken with truth. In this way Marxism has been destructive of communication and thus of community. This has happened, I believe, because it has encouraged a purely observant and explanatory relationship among human beings. It has undermined attentiveness.[12]

At this point the concept of recognition, introduced earlier in this essay, where it was suggested that participation in history is based on universal recognition, may provide some help. At best, of course, to recognize someone is to perceive and respect his particular, independently-centered being and consciousness; this requires close personal acquaintance. There is recognition of a more abstract kind, however, and that lies in facing others with consciousness that they are particular, independently centered beings, with claims to respect, although they are not individually known. This kind of recognition can be universal. Marxism undermines it, and one might say that it does so by forbidding one ever to put himself in the place of the other; rather he explains him. On the other hand, such recognition seems to underlie the unreserved attentiveness we are discussing here; attentiveness expresses recognition.

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[11] Some of the best discussions of communality in recent literature, or indeed, so far as I am aware, in the literature of all times, are found in the works of Gabriel Marcel and Martin Buber. Neither writer, however, formulated the concept of civility—Marcel lacking sufficient political concern, and Buber being too optimistic to envision the necessity of so alienated a form of political responsibility.

[12] The observant and explanatory relationship that Marxism encourages, of course, is closely connected with the Marxist emphasis on historical action. The point is not to interpret the world, as Marx observed in the "Theses on Feuerbach," but to change it. Since it was Lenin who brought out the activist component in Marxism, it may be said that Marxism-Leninism constitutes a model of a kind of thought opposed to civility. There are very different kinds of thought, however, that also are opposed to civility. An outstanding example from our own day, the product of a thinker and writer of great power, is the work of Karl Barth, especially the multi-volume *Church Dogmatics.* It is not religious faith as such that determines Barth's opposition to civility but rather his conviction that the truth can be embodied in a dogma that is absolutely authoritative and beyond discussion and that a Christian's sphere of life is the Church, not human society as a whole.

Beyond attentiveness, however, comprehensive communality requires something else equally difficult, and that is availability, or a readiness to respond. I speak of availability, of a state of readiness, rather than of responding itself, because opportunities for responding are only occasional and are never, for most people, nearly adequate to the universal scope of one's concern. Only a few of the most prominent political and cultural leaders can speak to all mankind; many people may feel that they have no opportunities to speak seriously at all—not even to those to whom they are closest. Hence all that can be asked of most of us is that we be prepared to respond to our "neighbor"—to the one whom we happen to encounter in a situation with communal possibilities—and in responding to speak of the concerns of mankind.

There is no outward mark of this universal attentiveness and availability. These qualities constitute an inner disposition and may not be visible in anything that is done. Indeed, in the final analysis they are not even rationally comprehensible. Only causal relations can be rationally understood, which is to say that only relations among objects can be rationally understood. Intersubjective relations can be discussed and illuminated. But the significance of words like "attentiveness" and "recognition" is evocative, rather than objective, and in using them we are not approaching a definitive explanation but are exploring a mystery.

At first it may seem that communality is the opposite of autonomy, and that the two cannot be combined. Reflection will show, however, that autonomy is possible only by means of a standard which can be applied to historical demands, and that communality can serve as that standard; conversely, communality must entail autonomy. Thus comprehensive attentiveness and availability engage one in a universal relationship which precludes total absorption in any particular relationship; in this way they dissolve ideologies and group demands.

Therefore, it is not surprising that the ideal of an autonomy which is at the same time perfect communality is ancient and enduring. Examples are not only Plato's "Inner City," but also the Stoic "Cosmopolis," Augustine's "City of God," and Kant's "Kingdom of Ends." All are versions of the notion that to be perfectly at one with others is to be wholly independent, and that even in solitude a person may live as a citizen of an ideal commonwealth.

At least one actual life evinces this solitary communality, that of Socrates. Although warned that the conversations in which he exposed the ignorance of his fellow-citizens were likely to bring him into collision with the laws of Athens, Socrates pursued his inquiries until they eventuated in his condemnation and execution. Thus not even the threat of death led him to compromise his autonomy; yet that autonomy was nothing but a defiant communality.

The third element in civility is at once in conflict with, and an inescapable manifestation of, communality.

(3) *Exemplary action.* Although civility expresses a dislike of action, it is not simply a form of inaction. As pointed out, responsibility in the absence of community entails action. The problem is to find a formula for action that is congruent with autonomy. How can one engage in action without relying either on the future or on the attitudes of others?

Drawing on the tradition of moral absolutism, one might reply: by acting according to an unconditional moral imperative. I am doubtful, however, that there are such imperatives (even "Thou shalt not kill," for example, is far from unconditional), and I would therefore like to suggest an alternative answer. That is, that one should carry out those acts which he believes would be historically beneficial if all, or at least all in some particular class to which he belongs, were to carry them out. Adhering to such a standard expresses a responsibility toward all human beings. At the same time it permits autonomy. One is freed from calculations of historical results since he does not ask what *will* happen but only what *would* happen if his act became an example which were generally followed. Nor does one await the decisions of others, or merely obey commands.

The idea of exemplary action, of course, is no more than a variation on the classical conception of how to act independently of consequences and of other persons, that embodied in Kant's categorical imperative. I believe, however, that it is a significant variation. It asks that one think in terms of hypothetical historical consequences, not in terms of the bare logical possibility of willing that the maxim of an act be a universal law. This in turn places the exemplary act on a different basis. The categorical imperative is a command of reason; an exemplary act is derived from the sense of ultimate glory we often have in mind in speaking of "the dignity of the individual."

In summary, then, civility is recognition of all humanity through historical autonomy, all-inclusive communality, and exemplary action. There is an old, endlessly-discussed concept which, surprisingly, connotes a great deal of what we are speaking of. That is the concept of tolerance. To be tolerant is to be in some measure communal (I assume that pure indifference does not deserve the name of "tolerance"), for the other is granted freedom to be himself and to be heard. It is also, however, to be autonomous, for granting freedom to opposed ideologies and groups implies a loosening of one's own ideological and group commitments.

The religious dogmatism and uniformity that prevailed in the Middle Ages, and was for a time intensified through the Reformation, made civility impossible. It was not religious faith as such that did this but the identification of faith with a single dogma and a single set of institutions.[13] The development of tolerance in the seventeenth and eighteenth centuries was a great historical achievement not only because it ended an era of slaughter but because it opened up the possibility of civil life. This is not to say that the possibility was exploited. After the beginning of the industrial revolution, the bourgeoisie proved to be highly uncivil; and the revolt against bourgeois incivility, led by Marx, gave rise to new dogmas. Also, no one has considered what the waning of religious faith, leading into the spiritual vacuum of the twentieth century, may have done to block realization of the spiritual potentialities inherent in tolerance. In any case, the establishment of tolerance was more than an enhancement of the basic decencies of life. It opened up modes of human relatedness, and thus of being human, that had been largely closed off since the end of antiquity.

An aspect of civility that deserves particular comment is the self-transcendence it involves. As already noted, civility is a compromised, rather than total, detachment; it does not mean refusing positions, but rather adopting them in a critical and tentative manner. The result is a paradoxical kind of personal identity. Both Hegel and Sartre have powerfully argued that a person establishes his identity through his actions, and that sentiments unexpressed in action, and in the public identity which ensues from action, far from being his "real self," are actually of no significance.[14] This argument is valuable as a counterweight to the easy and sentimental idea that I cannot really be known through the many unworthy acts I have committed but only by becoming acquainted with all of the beautiful feelings which I have never embodied in my observable life. It seems to me, nevertheless, that it is as arbitrary wholly to identify a person with his acts as it is wholly to identify him with the inner regrets and hopes in which those acts are transcended.

This is an issue in which not only civility, but many of what may seem the ordinary decencies of life, are at stake. If one were identical with his actions, for example, neither humility nor repentence would have any meaning. One would always act, so to speak, with his eyes closed, for having once become committed, there would be no possibility of doubt or reconsideration. Genuine communication would be impossible, for that always requires transcending the established self. Tolerance, which, as I suggested, is a possible summary term for civility, would be ruled out since it depends on a realization that one's acts and beliefs may be legitimately called into question.

The philosophical basis of this paradoxical concept of personal identity is the philosophical basis of civility itself, the twofold nature of man: he is both the product of history and the source of history; in relation to others he is a means (this is inevitably true in political calculations) and is also an "end-in-himself." Hence to live, as a human being and with human beings, is continually to be entangled in contradictions. Civility is an effort not to escape from the contradictions but to accept them and to turn them into sources of vital tension.

Civility is in many ways a departure from contemporary attitudes and ideas. For example, it means eschewing "engagement," or at least the kind of total and final commitments which have often been glorified in recent years.

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[13] See my comments on Barth in the preceding footnote. It is to be noted that Berdyaev, Jaspers, Marcel, and Buber all, in different ways, were religious. The core of Dostoevsky's political outlook was not theocracy but—a very different matter—the notion that atheism undermines respect for freedom, and thus civility. That idea deserves serious consideration—something it has not as yet received, owing perhaps to its being confused with the concept of theocracy.

[14] Such views are expressed in Hegel's *Philosophy of Right* and in Sartre's play *No Exit*.

If every commitment is to some finite and imperfect historical entity, then it is surely appropriate that political choices be tentative and regretful. Obviously, the kind of choice one makes will depend partly on circumstances; in some cases an irreversible and unreserved commitment may be inescapable. Have we not tended, however, to make this kind of commitment the norm and thus dangerously to sentimentalize the conditions of political life? The idea of cutting free from doubt and of being wholly engaged in some cause has a powerful appeal. But is this appeal not owing to our craving for a status beyond the imperfections around us and the perfectly appropriate hesitations within us?

Civility entails an ideological detachment which intellectuals are apt to disapprove of and even to find incomprehensible. Thus, to be civil is to be respectful of tradition, and to be inclined toward acquiescing in the demands of those in authority, (since there can be no civilization without tradition and authority) and yet not to be a conservative. To be civil is to realize that it is in the nature of society to dehumanize its members (both because a person in himself is not equivalent to the self defined by his social functions and because of corruption among rulers), yet not to be a radical. To be civil is highly to value liberty, especially in view of the dependence on liberty of civility, and yet, assuming that liberalism is another ideology alongside conservatism and radicalism, not to be a liberal.

The idea of civility also breaks with contemporary thought with respect to personal happiness. It is often held—even by thinkers of the stature of Hannah Arendt[15]—that meeting public responsibilities normally brings personal fulfillment and that conscientious citizenship is a way to happiness. The standard of civility, however, prescribes a taxing and uncomfortable posture, a posture befitting man but not one that can be expected to lead to anything so relatively simple and superficial as happiness. This is true in a number of ways. To be civil is to take on responsibility of a kind that cannot be laid to rest through a few sweeping decisions; with all of its internal conflicts, civility necessarily involves frequent periods of reconsideration and recurrent choice. Clearly it spells alienation; this is implicit in the refusal of identification with any group or program of action. To be civil is to be insecure since it is to be without either theoretical assurance or unequivocal social identity. Civility must carry with it a sense of powerlessness, because the average individual can have only a negligible influence on the course of history, and to feel powerless is to feel superfluous, not just in general but precisely in one's civility; it would not matter to the world if one ceased being civil.

This is a long way from the "public happiness" which Hannah Arendt thinks can be found in the public realm. I do not mean to say that there is no happiness at all in civility, much less that there is no satisfaction of any kind. However, in the age of Hitler and Stalin, of the Hiroshima bombing and the war in Vietnam, to delineate prospects of public happiness through political action is a species of utopianism.

Socrates was mentioned, earlier in this essay, as an example of civility. Let me conclude this long definition by suggesting an example from our own time, this a man whose life clearly exhibits the suffering inherent in civility: Alexander Solzhenitsyn. "Mankind's sole salvation," Solzhenitsyn has said, "lies in everyone making everything his business."[16] This statement, like much that we know of Solzhenitsyn, expresses one aspect of civility, that of universal relatedness. Solzhenitsyn's existence also is characterized by the extreme solitude which may well accompany universal relatedness. So far as we can tell, Solzhenitsyn is a man whose life receives its whole form from communality, from the effort to understand and to communicate, but who is also indefeasibly autonomous; he is both a Russian patriot and an alien within Russia itself; in his face we can see both a record of suffering and a dignity it is impossible to feel that he should have sacrificed for happiness.

Civility and Hope

In concluding this essay it seems appropriate to note, and briefly to reply to, a few of the principal objections which the idea of civility is apt to call forth.

To begin with, it may be said that to accept the idea of civility is to acquiesce in historical despair. It can be argued, either on humanistic or on religious grounds, that despair is always wrong, even where it is not demonstrably in error. Today, however, *historical* despair is particularly unattractive. In an age in which the "death of God" and the collapse of Heaven have been so frequently and confidently proclaimed, it is not easy to give up earthly hope.

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[15] See, for example, Hannah Arendt, *On Revolution* (New York: The Viking Press, 1963), where the concept of "public happiness" is discussed.

[16] Quoted by Anthony Lewis, *The New York Times,* Jan. 6, 1973.

And it is true that the idea of civility calls on us, within limits, to do this; it tells us that history is not to be relied upon.

Hope, however, is not condemned. We are forced by the catastrophes and disappointments of the twentieth century to grant that history is not within the scope of our knowledge or command. But we are not forced to become doctrinaire pessimists; that would be to change the content of our error without giving it up. To hold that history *must* be disastrous, and that general, long-range progress *cannot* occur, expresses pride—not the exuberant pride often associated with the doctrine of progress but the despairing pride of using cynicism as a shield against disappointment. Civility requires historical openness, and that is something different from relying on history to be either benign or malignant.

This is to speak of theoretical presuppositions. As for actual results, civility might paradoxically turn out to be more beneficial than revolutionary determination. Radicals are morally outraged by anyone who is not resolved to uproot all of the evil in human society. They should be reminded, however, that those men in our times who have dedicated themselves to the total eradication of evil have not merely failed; they have themselves perpetrated great evils. Revolutionary determination has not worked out well. But what reason is there for expecting anything better from civility? In responding to this question I think one should not be embarrassed to appeal to the ancient sense that there is a Logos, or meaning, deeply embedded in reality, and that it is accessible only to those who are receptive rather than willful. I assume that Dostoevsky had a thought of this sort in mind when he wrote that "humility is a very great force."[17]

There is a reason more likely to appeal to those who are realistic or skeptical, however, for thinking that widespread civility would be historically advantageous. Civility is an ideal of historical responsibility without historical illusions. It is possible to argue, of course, that illusions are indispensable, that steady and nonfanatical political responsibility cannot be sustained, for example, apart from the illusion of progress. But such a view would be extremely cynical and ought not to be entertained without compelling reasons for doing so. No such reasons are apparent. On the contrary, to live with illusions is to live blindly, without regard for that which, being real, will assert itself regardless of whether we acknowledge it or approve of it. Historical misfortunes can often be traced back to defects of human understanding. Can they ever be traced back to wisdom and to knowledge?

Civility may, in its detachment and inner dividedness, seem contrary to the decisiveness which is often indispensable in affairs of state. It appears, for example, that long periods of indecision and half-way measures were contributory to the tragic American involvement in Vietnam.[18] One may ask whether such are not the kind of consequences which might in many circumstances follow from civility. What civility calls for, however, is not drift but self-transcendence. That these are not the same, and do not necessarily go together, is shown by the example of Abraham Lincoln. Clearly, Lincoln had the capacity both for making hard decisions and for looking at himself from a distance and realizing that not only his opponents, but he too, was under the judgment of God.

Perhaps the most disturbing characteristic of civility, however, is that it implies a certain acceptance of injustice—not merely for oneself, which might seem tolerable, but for others. Thus a comfortably situated professor, practicing civility, may come to terms with the prospect that most of mankind, for generations, or even always, will live in physical squalor. In view of such a case it may seem that "civility" is little more than a euphemism for complacency.

A suspicion of this sort cannot be dismissed. Surely the truth we here touch upon, however, does not concern civility alone but every ostensible moral posture. Morality of any kind lends itself to hypocrisy. No doubt civility may be used as a mask for complacency, perhaps deceiving even the one using it; but the determination to uproot all injustice may be a mask of self-righteousness and resentment. As Reinhold Niebuhr often pointed out, radicals almost always seem naively unaware of the moral ambiguities of their own behavior even though, with their Marxist backgrounds, they are often highly sensitive to such ambiguities in the behavior of others. This naive self-assurance has of course not been a mere peccadillo. It has given rise to despotism and bloodshed, and it has done this to such a degree that it may be reasonably argued that radicals have done less to diminish the injustice in the world than have those whose opposition to injustice has been moderated by a certain resignation to its continuance.

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[17] Fyodor Dostoevsky, *The Notebooks for The Idiot*, ed. Edward Wasiolek, trans. Katharine Strelsky (Chicago: University of Chicago Press, 1967), p. 172.

[18] For evidence of this see David Halberstam, *The Best and the Brightest* (New York: Random House, 1972).

Still, leaving hidden motives out of account, is it not right to oppose injustice? It cannot be denied that it is and that radicalism thus realizes a formal rectitude. The authority of this rectitude is indicated in the fact that it is not more powerfully or emphatically expressed by Marx or any other modern radical writer than it is by the prophets of the Old Testament. They too were outraged by those who acquiesced in injustice. How, then, can the resignation to injustice, which is seemingly inherent in civility, be defended?

As moral will, it cannot. As an assessment of historical prospects, however, it is a different matter. One may be deeply incensed by prevailing injustices; it is simply false, nevertheless, to foresee their imminent end. We have no grounds for doing so, and for anyone to urge that we ignore the lack of grounds and, to give ourselves courage, cultivate the faith that what *should* come to pass also certainly *will* come to pass would be to offer dubious advice. The bloody consequences of the Russian and Chinese Revolutions, as well as the general air of disappointment reigning in the West, indicates that confidence in the future is not always benign.

The conclusion seems unavoidable that what we are morally bound to will and what we can reasonably expect are not always the same. Radicals and conservatives typically join in finding this contradiction insupportable, with the former adjusting expections to will and the latter will to expectations. If the idea of civility is valid, however, then the contradiction between morality and history is one of the tensions inherent in realizing one's historicity. Clearly it is not easy to will one thing and to anticipate something else. Kant's moral writings, however, have familiarized us with such a position and seem to show that it is not psychologically impossible. At bottom, to say that will and expectation must be incongruous seems to be nothing other than a demand for the self-transcendence that has already been discussed.

Further, in this connection it seems in order again to say that civility is not dogmatic pessimism. It is not a dark assurance that injustice is abiding, but only a wariness of the bright and dangerous assurance that it is not.

One way in which Americans might reflect on the historical implicatons of activism and self-confidence, in comparison with civility, is by considering what has happened to them in Vietnam. That experience may plausibly be interpreted as a tragedy of incivility. If America's involvement in Vietnam had a primary spiritual source, this seems to have been the assurance that history was under American control. This assurance seems to have been associated with a multifaceted pride: nationalistic pride, in which it was taken for granted that America, with its restless genius and its unparalleled power, could not possibly be thwarted by a tiny peasant people in Asia; intellectual pride, evident in certain presidential advisers, which gave rise to the confidence that, as one observer remarks, "sheer intelligence and rationality could answer and solve anything;"[19] the pride of youth, prevailing in the Kennedy Administration, and the pride of the Establishment, serene in the assurance that it could wisely direct America's course in world affairs. It is noteworthy, too, that this spirit of historical dominion had its ideological forms, above all anti-Communism and the doctrine of containment.

It goes without saying that historical events are complex and that the mood I am characterizing was not the exclusive cause of the War. The attitude of President Kennedy and his advisers, for example, was self-consciously pragmatic and nonideological, and our early commitments to Vietnam were products of a disposition to avoid bold and masterful decisions by resorting to temporary measures. These, however, were not the major currents in the stream of events. It is unlikely that we would ever have become so deeply involved in a conflict bearing so slightly on national interests had it not been for an extraordinary confidence in our powers of action (David Halberstam on the Kennedy years: "A remarkable hubris permeated this entire time."[20]) and for an anti-Communist ethos that had a far firmer hold on everyone than the Kennedys, in their self-assured pragmatism, realized.

From this perspective it is apparent why civility is not a form of historical despair. It is where the assurance of historical command is at its fullest that there is most to fear. In the diffidence of civility there is room for restrained, but well-grounded, hope.

Regardless of its implications for history, however, one may feel that civility is too difficult and grim for the individual. Today, it seems to be taken for granted almost everywhere that each one should live in a way that is immediately pleasing to himself and that if someone is not happy he must be either mismanaging his life or else a victim of unique misfortune. As I have pointed out, the standard of civility does not wholly accord with this outlook. It imposes trying responsibilities and leads to alienation. One cannot resolve to be civil and still hold himself free to live as he likes; nor can one expect civility to bear fruit in the uncomplicated and satisfied state of mind that is usually called "happiness."

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[19] Halberstam, p. 44.

[20] *Ibid.*, p. 123.

Nevertheless, civility is perhaps not so difficult, or at least not so exceptional, as it sounds. Do not many ordinary people look skeptically on ideologies and on the promises of leaders, view the future with misgivings, and still try to keep in touch with affairs, to consider sympathetically the problems and demands of other groups and peoples, and to meet their political responsibilties? So far as they do, they achieve something like civility. It is rather intellectuals who tend to involve themselves in more dramatic commitments, or at least to feel that they should. They are perhaps more troubled than others by the intellectually humbling notion that no existing, or even possible, ideologies are altogether true; since intellectuals are engaged in the enterprise of noetic mastery, they are perhaps more inclined than others to assume the possibility of practical mastery; and once they lose confidence in the future they may be more likely than those who are less thoughtful to draw the seemingly logical, although erroneous, conclusion that one may as well abandon all political responsibility. This should not be taken to mean that civility is no more than common sense but rather that its paradoxes, although standing out more sharply to those habituated in thinking, do not seem to place intolerable demands on human balance.[21]

Moreover, what is difficult and grim in civility is not gratuitous. It is the burden of sharing existence with billions of beings who are infinitely mysterious, who are sometimes kindly and sometimes cruel, and each one of whom has a claim to be treated as an end and not merely as a means. There is no way in which that situation can be made easy and pleasant for someone who is morally serious. Civility was defined above in terms of tolerance, and it is noteworthy that the word "tolerance" derives from Latin words meaning to lift up, or to bear. This suggests that civility is the stance through which one consciously and responsibly bears the existence of unimaginable multitudes of unpredictable and troublesome fellow humans.

In pursuance of this line of thought, it may be suggested that the modern emphasis on historical results is an effort to escape from beneath this burden of plurality. A great deal has been done to meet the threats of nature through intelligent mastery. It is not surprising that men should seek to meet in the same way the threats arising from one another. But as soon as we see the matter in this way, do we not glimpse also the mortal error that is inherent in this effort?

It should be said also, however, that civility is not all grimness, and that the proposition that civility cannot be expected to produce happiness does not imply that it must result simply in unhappiness. Civility is a position beyond the tragedy of the will to community. It is an effort to take part in history while avoiding political idolatry, to achieve a universal relatedness uncorrupted by fanaticism in action or by absolutism in beliefs. It means trying to participate in the affairs of the whole human race without either killing in order to redeem the earth from all evil or pretending that established society, with all of its subtle and habitual, as well as glaring, injustices is a community. Is it not possible that in thus acknowledging at once the calamitous imperfection of everything historical and the immeasurable dignity of the human—in taking a position so befitting both the historical and the moral dimensions of our existence—something may be found that is better than happiness?

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[21] There are of course many intellectuals who have achieved this balance. The tradition of what might be called "inconclusive rationality" goes back to Socrates. Perhaps its greatest representative in our time is Jaspers. Marcel exemplifies the same tradition by presenting in his written works not a finished philosophy but a process of philosophical reflection; he explicitly designated his thought as "neo-Socratic." Karl Popper expresses an inconclusive rationality that is set on different philosophical foundations, as does Max Weber, at least in his theory of value. Karl Mannheim's *Ideology and Utopia* is in large part expressive of such a viewpoint, but it can be argued that it all is cast overboard when he develops his concept of the sociology of knowledge.

The Social Origins of Liberal Democracy: The Swedish Case*

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A number of prominent economic historians have stressed the benefits of "late" or "delayed" development among the European countries. Veblen first called scholars' attention to the "advantages of backwardness," noting the ease with which Imperial Germany borrowed technology that the English had developed only with great difficulty. More recently, Alexander Gerschenkron has reported on the mechanisms which permitted "primary accumulation" to proceed with less distress and greater speed in Germany and in Russia than in England. Political sociologists, on the other hand, preoccupied with the pathological politics of Nazi Germany and Stalinist Russia, have cited the "disadvantages of backwardness," the errant history of those European nations that undertook the transition to industrialism relatively late. On this point the liberal German sociologist, Ralf Dahrendorf, and the radical American social historian, Barrington Moore, Jr., find themselves in agreement.

Dahrendorf does not present a general theory of development, but it is possible to infer one from his discussion of *Society and Democracy in Germany*.[1] Dahrendorf is concerned to explain why industrialization did not produce in Germany the consequences that it produced elsewhere, and above all, why German industrialization did not generate a modern liberal democracy on the English model. Dahrendorf wants to demonstrate why Imperial Germany became an industrial state, but not a liberal capitalist society; his problem is to ascertain the origins of an "industrial feudal society" and "an authoritarian welfare state." The basic thrust of his argument is that in Germany,

> economic development stuck onto an existing social structure so that, while the economic superstructure (as one might say in an ironic reversal of Marx's terms) is assimilated, the social and cultural context remains unchanged.[2]

The question then becomes why the social and cultural context remained unchanged, how the old ruling elements managed to preserve their position, and Dahrendorf's response is that *"Industrialization in Germany failed to produce a self-confident bourgeoisie with its own political aspirations"*[3] [Italics mine], that the German bourgeoisie sought to enter the prevailing structure of power and prestige as individuals rather than try to alter it as a class, and that hence "German society . . . remained quasifeudal."[4]

Dahrendorf traces the weakness of the German entrepreneurial stratum to five distinctive features of Germany's "late," "speedy," and "thorough" industrialization:

(1) "Large economic units and a powerful but small group of industrial leaders emerged so early in German industrialization as to leave no place for *the traditional liberal infrastructure of medium-size enterprises and bourgeois entrepreneurs*"[5] [Italics mine]. Dahrendorf's assertion that there was no place for the traditional liberal entrepreneur in Germany is too extreme, but certainly the class of small industrialists never achieved the importance in the German economy that it did in England.
(2) The state fostered German industrialization and entrepreneurs accepted its protection and patronage; for Dahrendorf, as for Marx and Engels, the German bourgeoisie ceded the right to rule for the right to make money.
(3) Public ownership of the means of industrial production was extensive; the public sector included railroads, canals, coal mines, blast furnaces, municipal utilities, banks, and part interests in private concerns.
(4) The state provided a variety of welfare benefits; sickness, accident, and old age insurance were established in the early stages of industrialization and later these programs were expanded and new benefits added.
(5) In Dahrendorf's catchy phrase, industrialization was carried through for "national," not for "rational" ends, or more precisely, Geman governments shaped industrialization to their purposes rather than leaving the direction and control of the German economy to private entrepreneurs and to market mechanisms.[6]

From this account one can extract a theory

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[1] Ralf Dahrendorf, *Society and Democracy in Germany* (Garden City, N.Y.: Doubleday, 1969), esp. chapters 3 and 4.

[2] *Ibid.*, p. 44.

[3] *Ibid.*, p. 52.

[4] *Ibid.*

[5] *Ibid.*, p. 38.

[6] *Ibid.*, p. 42.

of democratic development, though not a particularly subtle or complete one. Democratic development, at least for cases analogous to Germany, requires that economic development produce a broad, independent, assertive class of entrepreneurs capable of formulating a liberal program and of imposing it upon the preindustrial ruling classes. Just how this class will exert its political will, just which tools this group will employ to force the old elites to yield, Dahrendorf leaves unspecified. He specifies a set of *conditions* for liberal democratic development, but does not indicate what political *tactics* or choices might introduce the new regime.

Moore works out his theory of development in considerably greater detail.[7] Not only does he detail the conditions for the emergence of liberal democracy, he also strongly hints at the justifiability, even the necessity, of the tactics of violent revolution. He delineates three "routes" or "paths" to "the modern world": bourgeois revolution generating Western democracy, conservative revolution from above culminating in fascism, and peasant revolution leading to communism. These three types are presented less as alternative possibilities and choices than as depictions of the course of development in successive historical periods. Each of these "routes" entails immense suffering for the populations involved, but of the three, the path of bourgeois revolution is perhaps least unsatisfactory, and hence the conditions that favor it acquire a peculiar significance. Moore speculates that democratic development benefited from distinctive features of Western feudalism, most notably the institution of vassalage with its attendant conception of a private realm immune from the ruler's intervention and the idea of contract as a mutual engagement freely undertaken by free persons, but he concentrates on five historical preconditions for democratic development:[8]

(1) "the development of a balance to avoid too strong a crown or too independent a landed aristocracy";
(2) "a turn toward an appropriate form of commercial agriculture either on the part of the landed aristocracy or the peasantry";
(3) "the weakening of the landed aristocracy";
(4) "the prevention of an aristocratic-bourgeois coalition against the peasants and workers"; and
(5) "a revolutionary break with the past."

Although Moore's five conditions appear with significant variations in different historical circumstances, the logic of his position is relatively clear. For democracy to triumph, the monopoly of power by a small clique of arbitrary rulers must be broken. In the early stages of development the aristocracy and the bourgeoisie must ally to check royal absolutism, but the bonds between these two classes cannot be so cohesive that they endure and permit the formation of a common front against the urban and rural working classes; for in the later stages of development the bourgeoisie must press its grievances against the landed aristocracy and must be able to recruit allies from other classes if it is to demand and to institute democratic forms of rule. At some point in the process a violent revolution or civil war occurs which checks either royal absolutism or the landed elite.[9]

In this paper I want to test these two theories of modernization against a remarkably successful case of "late" modernization, the case of Sweden. Both industrialization and democracy came late to Sweden, at least by the standards of the European democracies; as Michael Roberts has written, "by Western standards the whole history of Sweden seems to be retarded."[10] The first significant impulse toward industrialization came in the 1850s with the growth of foreign demand for Swedish timber; a second wave of development, particularly in the iron industry, occurred in the 1870s; but the great forward surge of Swedish industrialization dates from the late 1880s. Democracy, too, arrived very late in Sweden. The reform act of 1866 abolished the ancient representative system of four estates and established a bicameral legislature, but rigid property qualifications still restricted the electorate for the Second Chamber to about 20 per cent of the adult male population.[11] The First Chamber was blatantly plutocratic in character; only 6100 Swedes were even eligible for membership in it.[12] A second reform act in 1907–1909 extended the suffrage for the Second Chamber to all tax-paying adult males, but the Swedish Conservatives managed to retain a privileged position in the First Chamber. In voting for the provincial assemblies which elected delegates to the First Chamber, a wealthy man might still cast forty votes. (Previously he had been able to cast as many as 5,000 in rural districts.[13]) Thus, it is premature to date Swedish democracy from 1907, as Dankwart Rustow does;[14] the definitive break-through came in 1918–1921 with the elimination of plural voting, the extension of the franchise to women, and the firm establishment of parliamentary government. Neither Dahrendorf's nor Moore's theory provides a completely satisfactory explanation of the "conditions" necessary for this transformation; neither adequately accounts for the tactics employed to effect it; and consequently it is necessary to give closer consideration to the course of Swedish development, its variance from the two theories, and possible alterations in these theories to account for this "anomalous" case.

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[7] Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1966), esp. chapter VII.

[8] *Ibid.*, pp. 430–431.

[9] *Ibid.*, p. 426.

[10] Michael Roberts, *Essays in Swedish History* (London: Weidenfeld & Nicolson, 1967), p. 2.

[11] Douglas Verney, *Parliamentary Reform in Sweden, 1866–1921* (Oxford: Clarendon Press, 1957), p. 91.

[12] *Ibid.*, p. 89.

Sweden's Industrialization: Some Distinctive Features

In 1870 about 3 million Swedes, or 72 per cent of the entire population, were dependent upon agriculture and related occupations for their livelihood. Shortly after the turn of the century this proportion fell below 50 per cent.[15] Over roughly the same period the industrial labor force expanded from 82,000 workers to 350,000, or more than quadrupled.[16] The volume of exports measured in fixed prices tripled, and most striking of all, the value of manufactured goods increased twentyfold between 1870 and 1914.[17] Industrial workers enjoyed an increase in real incomes of about 250 per cent, and farm laborers of almost 200 per cent, even though their respective shares of the national income were falling.[18] These figures strikingly illustrate that Sweden's industrialization, like Germany's, merits the description "late, speedy, and thorough," for not only did the Swedish economy undergo a rapid overall expansion, but the basic structural pattern of the economy—its concentration on lumber and forestry products, mining and metal-working, electrical engineering, and modern consumer industries—took place in the years 1870–1914.

By the last third of the nineteenth century the preconditions for rapid industrialization had been established. Canals and railways had effected a revolution in transportation. The remarkably early establishment of a national elementary education system (in 1842) helped create a skilled labor force. The basis for a modern banking and credit system had emerged. Ancient guild restrictions and mercantile regulations had gradually been struck down and freedom of trade and of occupation established. The foundations of the timber, mining, and metallurgical industries were in place, set for the expansionary boom.

When this expansion came, it did not produce vast urban industrial centers; Sweden has nothing to match Germany's *Ruhrgebiet* or the industrial districts of northern England. Instead, the location of industry assumed a special pattern in Sweden with plants dispersed in many small communities rather than concentrated in a few large industrial towns. Thus Fritiof Ander calls Sweden's industrialization "essentially rural rather than urban,"[19] and a 1913 government study could report that nearly one out of every two industrial workers was employed in the country.[20] Many of these scattered industrial enterprises were and are small. In 1913 only 19.1 per cent of the industrial labor force worked in units employing more than 500 men, a proportion well below the figures for the United States and Great Britain.[21] "Sweden is still predominantly a nation of small business," argues a current Swedish commentator, who notes that only 250 firms presently have more than 500 workers on their payrolls and that these larger firms employ only about a third of the industrial labor force.[22]

It would be a mistake, however, to leap to the conclusion that this profusion of small, scattered enterprises produced "the liberal infrastructure of . . . bourgeois entrepreneurs" that Dahrendorf considers crucial for democratic development. In the early stages of industrialization in a small group of entrepreneurs, far fewer in number than the newly-developing enterprises, organized and developed the industrial sector. As one Swedish economic historian puts it, "The same twenty or so names keep cropping up in at least a hundred of the firms that sprang up during the initial decades of Swedish industrialism."[23] Nor did the great number of enterprises engender a competitive free market society in the early phases of industrialization; for a long time the dispersal of Swedish industry guaranteed local monopolies and virtual freedom from competition, as the Swedish economic historian, Kurt Samuelsson, points out:

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[13] Verney, pp. 248–249.

[14] Dankwart Rustow, "Transitions to Democracy: Toward a Dynamic Theory," *Comparative Politics,* 2 (April, 1970), 337–63, at p. 356.

[15] Kurt Samuelsson, *From Great Power to Welfare State* (London: Allen & Unwin, 1968), pp. 146, 186.

[16] O. Fritiof Ander, *The Building of Modern Sweden* (Rock Island, Ill.: Augustana Library Publications, 1958), p. 51.

[17] Samuelsson, *From Great Power to Welfare State,* p. 189; Ander, p. 51.

[18] Samuelsson, p. 199.

[19] Ander, p. 51.

[20] Margaret Cole and Charles Smith, eds., *Democratic Sweden* (London: Routledge, 1938), p. 128.

[21] *Ibid.,* p. 129.

[22] Bengt Ryden, "The Swedish Economy," in *Sweden in the Sixties,* ed. Ingemarr Wizelius (Stockholm: Almqvist & Wiksell, 1967), pp. 117–118.

[23] Erik Dahmen, cited in Samuelsson, *From Great Power to Welfare State,* p. 200.

> Whatever aspect this nascent industrial enterprise is viewed from, it was as remote from the notion of free, open competition as anything that could be imagined. No entrepreneur ever surrendered to any free play of supply and demand, which was the cherished dream of Adam Smith. Instead, each entrepreneur exercised a relatively complete control over his market.[24]

The evidence indicates, then, that the original picture of a broad stratum of small entrepreneurs on the English model has to be qualified by an awareness of a considerable degree of local monopoly and of concentration and interlocking control.

From the early stages of industrialization the government encouraged this concentration of industry and the formation of cartels. With a substantial volume of loans and subsidies, and through direct legislation, the state enticed businesses into producers' organizations. These organizations promoted more efficient operation and more competitive export production. To supplement these measures the government adopted a protective tariff in 1888. This considerable state aid to business, in many respects comparable to the German pattern, may be traced in part to a community of interest between entrepreneurs and public officials; high-ranking public officials invested heavily in Swedish enterprise.[25]

Actual public ownership was limited. The state undertook the construction of the main railway lines, starting in the 1850s, but private branch lines flourished until the great nationalization of railways during the 1930s. The insufficiency of private capital prompted the state to invest in railroad construction, but it was excessive private investment in and exploitation of timber acreage that led to the expansion of public landholdings. To secure its control over forest resources the state began in the early 1900s to extend its ownership to about one quarter of all forestry land. But even when public ownership of electric power plants, iron mines, the post office, telephone and telegraph services, and the state's alcohol and tobacco monopolies are included, public ownership currently comprises only about 6 per cent of all productive capital.[26]

The Swedish welfare state relies not on wholesale socialization, but on governmental regulation and the provision of public services. Certain of these features of the welfare state had emerged before World War I; restrictions upon hours of work and provision for inspection of factories were in force, but these achievements resulted as much from collective bargaining as from legislative enactment. From 1901 the state granted subsidies to private voluntary insurance organizations and promulgated a scheme for insurance against accidents in factories, but in general social legislation lagged far behind the German model that inspired Swedish policy makers.

If the Swedish economy did not evolve in the direction of laissez-faire, neither did it assume the statist features of German development. In this connection one special feature of Swedish industrialization is crucial: Swedish expansion was not propelled by military outlays. A half-century of peace and a relatively secure international position had muted the motive force of the idea of national power for Swedish politicians. In 1864 King Charles XV in a fit of foolish bravado tried to assist the Danes against the Prussians, but his ministers restrained him from undertaking this risky policy. In the years immediately before World War I the Liberals and Social Democrats formed a powerful anti-militarist block; when King Gustav V responded to the Left's attack with an unauthorized speech in support of defense expenditures, he precipitated a severe political crisis. Sweden's reliance upon free foreign trade rather than upon colonies may similarly have helped stimulate its pacific policy; in any case its foreign policy during its industrialization shows the sharpest contrast with Germany's, and the development of its economy differs correspondingly. The Swedish bourgeoisie did not depend upon the state for its markets, but had an independent economic base.

The developing Swedish economy, then, does not correspond closely to the liberal laissez-faire model that Dahrendorf celebrates. The limited development of an urban bourgeoisie, the formation of cartels, the weakness of competitive market structures, and the generous provision of government aid, place Swedish industrialization much nearer the German than the English model; however, the modest degree of public ownership, the limited welfare benefits, and above all the restricted scope of the military component mark Swedish development as distinct from the German pattern—but not sufficiently distinctive to validate Dahrendorf's model. The liberal model thus offers important clues to the origins of Swed-

[24] Dahmen, p. 164.

[25] *Ibid.*

[26] Gunnar Adler-Karlsson, *Functional Socialism* (Stockholm: Prisma, 1969), p. 18.

ish democracy, but hardly a thorough and satisfactory explanation. Hence, it becomes necessary to consider the radical model proposed by Barrington Moore, Jr., and in order to do so, one must turn the clock further back.

Sweden's Political Development: Some Distinctive Features

The starting point of Swedish political development differs from those of the European countries Moore considers in that Sweden "never knew a fully-fledged feudal society such as developed in Germany, France, or Norman England."[27] Nonetheless Swedish society at the close of the Middle Ages displayed several features conducive to democratic development. Though far from complete, a considerable degree of national unity had been obtained. Within the nation's present boundaries the population was remarkably homogeneous; no sizable ethnic, religious, or cultural minority threatened the political system with irreconcilable antagonisms.[28] The nobles, or *frälse*, had carved out a sphere of immunity and corporate privilege, a status institutionally recognized in their dominant position in the King's Council and later in the Estates. Despite their superior social status, however, the nobility did not hold a commanding position in the economy; they owned roughly only a tenth of the land.[29] Free-holding peasants probably owned about half of the land in the fifteenth century; they held sufficient power to tip the balance in the swaying constitutional struggle between monarchy and nobility. The peasants' admission to the Riksdag as a separate Estate constituted recognition of their military and economic importance. The size and strength of the independent Swedish peasantry can hardly be overemphasized, for in Sweden the peasants often played the role that the bourgeoisie played elsewhere as an agency for preserving the balance between the monarchy and the nobility.

This balance between crown and aristocracy, which Moore deems essential for democratic development, was generally maintained in Sweden, with exceptions to be noted. The eminent British historian of Swedish society, Michael Roberts, characterizes much of Sweden's modern history as a balanced struggle between aristocracy and monarchy. "From the beginning of the fifteenth to the end of the eighteenth century," he writes,

> Swedish history derives its inner dynamic from the tension resulting from the pull of two conflicting constitutional forces: on the one hand an aristocracy which combined the assertion of its interests with the defense of constitutional liberties; on the other a monarchy which could count on popular support by seeming to protect the little man against exploitation.[30]

The strength of each tradition—the aristocratic constitutional and the hereditary monarchical—mutually checked any enduring claims to absolute rule. In the early sixteenth century Gustav Vasa, exploiting his seizure of church lands and the recent decimation of noble ranks, powerfully asserted the claims of the monarchy, but in succeeding decades the nobles used their position in the Riksdag to recapture their social and political standing. During the Thirty Years War these leading noble families greatly enhanced their position; they more than doubled their landholdings and, impressed with German agricultural practices, they threatened to reduce the independent peasants to serfdom. The peasantry, however, found allies in the monarchy and among the lower nobility (both frustrated by the pretensions of the high nobility), and using its own position in the Estates, succeeded in staving off the danger of aristocratic absolutism. This defeat of the high nobility is one of the decisive events of Swedish history; it took the form of a massive reversion of land from the nobility to the crown and the peasantry, known as the *reduktion*. From 1680 Charles XI, in exchange for recognition of his absolute power, reduced the nobles' landholdings from about 72 per cent of the kingdom to about 33 per cent.[31] His policy of reduction consolidated the position of the petty bureaucratic nobility and especially of the monarchy itself; he and his successor, Charles XII, now moved to create an absolute monarchy.

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[27] Stewart Oakley, *The Story of Sweden* (London: Faber, 1966), p. 39; cf. Ingvar Anderson, *A History of Sweden* (New York: Praeger, 1956), p. 43.

[28] Robert Dahl, *Polyarchy* (New Haven: Yale University Press, 1971), pp. 105 ff., stresses the significance of this point for "polyarchical" development. This important book by a prominent "liberal" would qualify as a leading representative of the liberal school of development theory, but it does not concentrate upon "late" modernization, nor does Dahl ever come fully to grips with Moore's emphasis upon the therapeutic role of violence in democratic development. Dahl's list of "conditions favoring polyarchy" is so extensive and so heterogeneous that doing his theory justice would require a separate article. Furthermore, Dahl's seems to be a theory of a different type from Dahrendorf's and Moore's, one concerned not only with the genesis of democracy, but also with its functional requisites.

[29] Andrew Stomberg, *A History of Sweden* (New York: Macmillan, 1931), p. 206.

[30] Roberts, *Essays in Swedish History*, p. 119.

[31] Andersson, *A History of Sweden*, p. 216.

But still there could be no durable absolutism. Charles XII's defeat in the Great Northern War and his death in 1718 released the currents of aristocratic constitutionalism once again and ushered in the "age of liberty." The Age of Liberty (1718–1772) was characterized by the rule of the bureaucratic nobility in conjunction with the quasi-sovereign Estates. Despite the diminution of their fortunes as a result of the *reduktion* and the Great Northern War, the aristocrats had come to power, but only in concert with the other estates and with notable fissures in their own ranks. The aristocracy now practiced the forms of parliamentary rule and spoke the language of freedom, but it rigidly clung to its privileges and extended them whenever possible. As Roberts observes, "Its concept of liberty had become narrow and sterile: political liberty for the upper classes, and a Whiggish distrust of the monarchy. Social liberty it strenuously resisted; and equality and fraternity were not among its watchwords."[32] The nobility's obstinate defense of privilege alienated the small but increasingly important class of wealthy merchants, shippers, manufacturers, and ironmasters, and literally brought about a reaction. In 1772 Gustav III capitalized on the nation's disgust with parliamentary incapacity and executed a monarchical coup. Although Gustav's sympathies lay with the aristocrats, he could not offer them rewards without antagonizing the commoners to a dangerous degree. Unable to obtain noble backing, he was forced to rely on popular support, and in 1789 he carried through a further sharp reduction of the nobles' privileges. Without revolutionary violence the Swedish nobility had been deprived of its privileged status. Economic power had already been shifting toward the "Skeppsbro-nobility" of Stockholm and Göteborg (so named because these merchants strung their houses out along the waterfront) and the "mill nobility" of the ironmaking districts. Now the old nobility grudgingly had to concede a greater share of social and political power to these entrepreneurs and to the newly-emerging professional men and civil servants. This new balance of social forces was enshrined in the constitution of 1809.

In this rapid survey of Swedish history one can discover three of Moore's conditions for democratic development: the maintenance of a balance between crown and landed aristocracy; the weakening of the landed aristocracy and its subsequent openness to bourgeois attitudes; and the prevention of an aristocratic-bourgeois coalition against the workers and peasants. In establishing each of these conditions the power of the peasantry was crucial. But there is no evidence of Moore's other two conditions, the turn toward an appropriate form of commercial agriculture by the landed nobility or a revolutionary break with the past. These two factors require closer attention, as do the immediate circumstances under which Swedish democracy was established.

The Swedish nobles did not instigate a transition to commercial agriculture as the English nobility did; many of them got out of farming altogether and hired themselves out as army officers and civil servants, much as impoverished Junkers did in Prussia. Nonetheless, as Moore himself points out, the nobles' failure to develop a prosperous commercial agriculture did not have anti-democratic consequences. Moore attributes this result to the fact that the *peasants* turned to specialized commercial farming.[33] It is true that from the 1890s Swedish agriculture displays a marked trend toward dairy farming, but this trend lagged well behind the Danish switch away from grain-growing. Indeed, one writer maintains that "certain natural forces had checked a change in Swedish agriculture and preserved Sweden as chiefly a grain-producing country instead of a dairy country."[34] Moore's explanation is apparently incomplete.

At this point one has to ask *why* the transition to commercial agriculture is necessary. The answer is that it produces a sound economic base for those peasants who remain on the farm and provides them with enough financial independence to render them impervious to Fascist and Communist appeals. In Sweden the enclosure movement destroyed the peasant village, replacing the old communal way of life with compact, efficient individual family units, but this transformation aggravated class differences in rural areas and coincided with a notable expansion of the rural proletariat, the crofters, cotters, paupers, and itinerant laborers.[35] Some Swedish peasants thus secured viable economic holdings, but many later simply left farming, either to take a job in industry or to emigrate to America. Indeed it seems plausible to argue that in Sweden *emigration* largely eliminated or at least greatly eased the problem of transforming the peasantry into some other kind of social formation. Without this massive depopulation Sweden would have shared the plight of many contemporary developing societies—profound social crisis stemming from rural poverty and underemployment. Between 1860 and 1910 there occurred a massive "flight from the farms;" in this period more than 750,000 Swedes, or about one out of five, emigrated to the United States, thus greatly alleviating the problem of rural poverty and removing the basis for a peasant revolution.[36]

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[32] Roberts, p. 25.

[33] Moore, *Social Origins of Dictatorship and Democracy*, p. 422.

[34] Ander, *The Building of Modern Sweden*, p. 46.

[35] Samuelsson, *From Great Power to Welfare State*, pp. 148–149.

A revolutionary break with the past might have sprung from an urban base, from the bourgeoisie or the industrial workers or both, but there is no event in Swedish history that can reasonably be construed as such a revolution. Moore's fifth and strongly emphasized condition simply does not appear. Swedish democracy does not owe its origins to a revolution, but to a series of reform acts in 1866, 1909, and 1918 extending the franchise in a way reminiscent of the English Reform Acts. What circumstances permitted this gradual evolution toward democracy to take place? To pose this question is to ask what conditions favor non-revolutionary democratic modernization and thus to open the way for amendments to Dahrendorf's and Moore's positions.

The Transition to Democracy and the Establishment of the Welfare State

The standard treatment of the Swedish Reform Acts is Verney's *Parliamentary Reform in Sweden.* From his account one can derive the following conclusions:

(1) The 1866 reform recognized the growth of a commercial and industrial middle class, but farmers formed the largest bloc in the new parliament. The new legislation did not represent a liberal triumph, for at first the composition of the Riksdag changed very little and social reform ground to a halt. Rather the extension of the franchise is best viewed as a shrewd conservative accommodation to altered circumstances.

(2) The 1909 reform resulted from the efforts of a conservative government; it, too, allowed the Conservatives to maintain their position in the First Chamber intact. By skillful maneuvering the conservative leader Lindman defanged proposals for thorough democratization and parliamentary government and substituted measures which enfranchised the *småfolk,* the middle class and numerous workers, but retained power in safe conservative hands.

The limits of this paper prohibit a careful examination of these first two reform bills, but two points must be noted. First, neither passed without strong popular pressure; in 1866 crowds thronged around the chamber while the final vote was taken, while the 1909 reform was stimulated by a broad suffrage movement, a demonstration strike, and the union crisis with Norway. Second, the property qualifications embodied in each act lost their force with the growth of per capita income; each act thus prepared the way for a further extension of the suffrage.

The events of 1917–1918 and the third reform act, which effected the transition to democracy and parliamentary government and as a result drastically shifted the social balance of power, deserve closer scrutiny. The Liberals and Social Democrats had long been demanding complete democratization of the franchise and parliamentary supremacy, but had lacked a sufficient majority to carry out their program. Now, however, the economic crisis stemming from the wartime blockade caused general disenchantment with the conservative government. By 1917 the per capita consumption of grain had decreased by more than 50 per cent.[37] In April and May of 1917 food riots broke out in the larger cities, an ominous portent to conservatives, who knew that just such riots had triggered the February revolution in Russia. In the general elections of 1917 the parties of the Left gained a victory which increased their majority in the Second Chamber. Still, the conservative government might have salvaged its position but for the Luxburg affair, an international scandal which involved the German's use of the Swedish diplomatic cable. This breach of Swedish neutrality, not satisfactorily denied by the government, thoroughly discredited the conservative regime.

A Liberal–Social Democratic government took office, but the Conservatives continued to use their dominant position in the First Chamber to block suffrage reforms and the eight-hour day. At this point left-wing Social Democrats, later to form the Swedish Communist party, threatened a general strike. Workers in the streets demanded democracy and a republic. Prospects of suppressing this agitation were minimal: The Stockholm chief of police submitted a highly pessimistic report about the reliability of his forces; the Minister of War and the Minister of the Marine described sentiments in the army and navy as "very revolutionary" and announced special measures to deny the armed forces access to their weapons.[38] Under this extraordinary strain right-wing unity began to crumble. The king, shaken by reports of the revolutions in Russia and in Germany, had already opened negotiations with the Conservatives for a constitutional settlement. Within the Conservative party prominent spokesmen urged a conciliatory course; an important association of right-wing editors supported their policy.[39] Influential bankers, merchants, and industrialists in Stockholm and in Gothenburg energetically advised the Conservative leaders to renounce their opposition to a democratic regime. Even the Lutheran archbishop recommended that the Conservatives yield.[40] At the decisive moment, the frightened Conservatives, fearful that Sweden might follow the example of Russia, Germany, and Austria-Hungary, finally capitulated. Swedish democracy had triumphed without a revolution—but not without the *threat* of revolution.

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[36] Wilhelm Tham, "Industrialization of Sweden's Rural Areas," *Scandinavia Past and Present* (Odense: Arnkrone, 1959), p. 800.

[37] Cited in Steven Koblik, "Wartime Diplomacy and the Democratization of Sweden in September-October 1917," *Journal of Modern History,* 41 (March, 1969), 29. The non-partisan, but highly conservative Hammerskjold cabinet earned the nickname "Hungerskjold" cabinet because of its failures in supplying food to working-class Swedes.

Four conditions were necessary to produce among Conservatives the necessary capacity and willingness to make concessions and to allow a non-revolutionary transition to democracy:

(1) the availability of parliamentary institutions in which concessions could be extracted;
(2) the absence of a professional standing army available for repressive purposes;
(3) the presence of massive unrest and some minor outbreaks of violence; and
(4) the conviction among the elite of the possibility of revolution and also of the possibility of a peaceful resolution of grievances. (The ruling elements had to have both the *incentive* and the *possibility* to settle affairs short of revolution; they would not grant radical reforms willingly, but only as the choice of lesser evils.)

A legal constitutional order infused with liberal principles facilitated the operation of these conditions.[41]

To test the adequacy of this analysis of the conditions for democratization without revolution, a survey of the breakthrough to social democracy in 1932 suggests itself. The depression provided the impetus to the final triumph of the Social Democratic party, but whereas in Italy, Germany, and France, depression had produced strong Fascist movements, especially in rural areas, in Sweden the Fascists never mustered a threat. In Sweden the Social Democrats managed to enlist the support of the Farmers' party for social reforms. They extended agricultural protection and subsidies to the farmers in exchange for support for their own program. Such a coalition was virtually unthinkable in Germany, but the Swedish Social Democrats had cast aside revolutionary rhetoric and slogans calling for the socialization of land.[42] But above all what made this reformist coalition possible was the fact that the Swedish farmer could sympathize with urban workers' demands. He himself was often as much of a worker as a farmer, taking winter employment as a miner or lumberjack; this experience, combined with his unpleasant dealings with banks, barred a firm community of interest with urban capitalists. His participation in farm cooperatives accustomed him to the collective pursuit of social goals. Hence, he consented to his party's participation in the construction of the welfare state.[43]

The depression weakened the resistance of Swedish capitalists still further psychologically and economically; it destroyed their confidence in their ability to manage the economy and their failure was costing them money. Meanwhile, a series of harsh labor conflicts culminating in the shooting of a crowd of demonstrators in 1931 recalled to their minds the possibility of a revolutionary upheaval. Far better, they reasoned, to let the moderate Social Democrats rule, particularly when they could rely on the Farmers' party to veto truly radical reforms.

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[38] From the diary of Social Democratic Cabinet Minister Värner Rydén. Gunnar Gerdner "Ministären Edén och författningsrevisionen" in *Kring demokratins genombrott i Sverige,* ed. Stig Hadenius (Stockholm: Wahlström & Widstrand, 1966), pp. 105 ff.

[39] Alex Brusewitz, *Kungamakt, Herremakt, Folkmakt* (Stockholm: Tidens förlag, 1951), pp. 109–112.

[40] Sven Anders Söderpalm, *Storföretagarna och det demokratiska genombrottet* (Lund: Gleerup, 1969), pp. 189, 208, 216; Gunnar Gerdner, "Ministären Edén och författningsrevisionen," pp. 110, 113.

[41] "National unity, legality, liberalism, and complex constitutional government thus facilitated Sweden's political transformation. They are not, however, enough to explain it—precisely because they were traditions that had been in effect long before the transition to democracy began. They are best put in the category of background conditions that came into play once the process of change was set off by other stimuli." Dankwart Rustow, "Sweden's Transition to Democracy: Some Notes toward a Genetic Theory," *Scandinavian Political Studies* VI (1971), 15.

[42] The reformist orientation of the Social Democratic left was clearly a further condition of liberal democratic development in Sweden. This important matter of Social Democratic attitudes is taken largely as a "given" here, partly because analyzing the origin of this "moderation" would go well beyond the limits of this paper, and partly because Herbert Tingsten has thoroughly documented the evolution of the Social Democratic platform in his detailed two-volume study, *Den svenska socialdemokratiens idéutveckling* (Stockholm: Aldus/Bonnier, 1967). A more recent and somewhat revisionist account is Leif Lewin, *Planhushållnings debatten* (Uppsala: Almqvist & Wiksell, 1967).

[43] For a similar phenomenon in Norway see Sten Nilson, "Wahlsoziologische Probleme des Nationalsozialismus," *Zeitschrift für die gesamte Staatswissenschaft,* 110 (1954), 279–311.

The events of 1932 largely confirm the four conditions for radical reform without revolution. Parliament served as a forum for the bargain between Farmers and Social Democrats. Armed conservative resistance was out of the question because the armed forces had been substantially reduced following the war. The depression had shattered the prestige of the industrialists, but in most cases left their fortunes only moderately impaired. Their own incapacity to cope with the economic crisis and the threat of radical working-class action reconciled them to the prospect of a Social Democratic regime tempered by a more conservative rural element.

The four conditions that I have outlined, combined with those features of the Dahrendorf and Moore analyses that have not been rejected, comprise what I shall call a *radical liberal model of democratic development.* This model postulates, and Swedish experience confirms, the existence of a distinctive set of conditions and tactics for democratic development—the availability of another "path to the modern world." It incorporates Dahrendorf's emphasis upon the avoidance of an industrialization fueled by vast military outlays and promoted by a soldier-aristocracy, but it rejects the notion that a liberal laissez-faire society constitutes an essential prerequisite for democratic development. The model also employs three of Moore's preconditions: the preservation of a balance between monarchy and aristocracy in the early stages of development; the subsequent weakening of the landed aristocracy and the permeation of its thinking by bourgeois values; and the prevention of a reactionary alliance of the aristocracy and bourgeoisie against the peasants and workers. Sweden's development demonstrates, however, that massive emigration may substitute (in large measure, if not wholly) for the growth of commercial agriculture as a means of eliminating a financially unstable and politically dangerous peasantry. The Swedish case further demonstrates that, given the four conditions discussed above (p. 568), a radical reform can substitute for revolutionary violence.[44]

The radical liberal model thus specifies *tactics* as well as *conditions* for relatively nonviolent democratic modernization. Unlike currently prevailing treatments of political development, the radical liberal model does not imply that political modernization—in America as elsewhere—must proceed either by the practice of violent revolution or by "institution-building" and purely parliamentary tactics ("working through the system"). The Swedish case points to a path between timid liberalism and revolutionary violence. It focuses attention not on an endless list of "socio-economic variables" but on the relationship between the power of the state and the power of insurgent popular movements. The actual balance of forces may determine the outcome less than does the *perceived* balance of forces; the perceptions of the parties in the conflict may be shaped by recent events (cf the effect of the Russian and German revolutions on Swedish conservatives), by effective propaganda, or by misleading reports about the loyalty of the army. When the popular movement is able to persuade its governors that they face a choice between radical reform and revolution, when the insurgents are able (as the Social Democratic leader Branting was in 1918) to keep their revolutionary wing relatively small, and when the diehard conservatives perceive the futility of repression, the radical liberal path to democratic development opens.[45] In short, Swedish experience

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[44] Alexander Gerschenkron has pointed to a similar kind of substitution in his writings on economic backwardness. *Economic Backwardness in Historical Perspective* (Cambridge, Mass.: Belknap Press, 1966), esp. ch. 1. All industrializing societies must acquire a surplus sufficient for investment in industrial capital, but the generation of this surplus can assume quite different forms. The development of a banking system like the German one may substitute new processes for the cruder forms of "primitive accumulation"; similarly foreign investment or forced appropriation by the state may "substitute" as methods of acquiring investment capital. In political development, too, "substitutes" may be found for "preconditions," or to employ more accurate language, there are certain obstacles that must be overcome if democratic and liberal tendencies are to emerge, but these obstacles may be overcome in a variety of ways. Unfortunately the language of necessary "preconditions" and "prerequisites" has tended to obscure the empirical importance and the logical validity of "substitution."

[45] This analysis obviously owes debts to and complements the work of Robert Dahl, Albert Hirschman, and Joseph Hamburger. Dahl's "Axiom 5" (*Polyarchy,* p. 49) might be rephrased for present purposes as "The likelihood that a government will tolerate radical reform increases with a reduction in the capacity of the government to use violence or socioeconomic sanctions to suppress an opposition." Dahl quite properly concentrates on the relationship between government and opposition rather than the absolute size and strength of the government.

My colleague David Collier called my attention to Hirschman's precise logical analysis of "reformmongconfirms two attractive propositions: one, that under favorable conditions a powerful popular movement can extract important liberal and democratic concessions without resorting to extensive violence, and two, that "late" development need not preclude a successful and largely non-violent transition to liberal democracy.

Swedish Development and American Politics

The radical liberal model may also have significance for American politics, not only as a description of previous reformist successes (such as the unionization of heavy industry and the passage of civil rights legislation), but also as a prescription for future efforts at reform. It is now a fairly well-recognized fact that discussions of political development abound with normative implications; explicitly or implicitly they propound particular visions of what political institutions are desirable and of what political tactics are necessary or legitimate. Dahrendorf, for example, candidly admits that his study of German development "is, among other things, a plea for the principle of liberal democracy."[46] Moore argues that the absence of a revolutionary break with the past helps account for the problems of parliamentary democracy in India;[47] while this empirical assertion falls well short of an espousal of revolutionary violence, it does impute value to revolutionary activity. A host of other studies might be cited to demonstrate the degree to which analyses of development imply models of a "good society" and of desirable developmental paths. Seldom, however, do the implicit or explicit proposals of these studies approximate the radical liberal model of political development, and seldom do they suggest that the United States is in need of some sort of "modernization."

This tendency is unfortunate, for the development of a modern social welfare policy in the United States, if it occurs, might well assume forms comparable to the Swedish transition to democracy. The achievement of the radical reforms essential for such modernization (a national health program, rational planning for urban communities, greater public control over investment decisions, a more equitable distribution of property and incomes, the dismantling of the garrison state, and a full employment economy) will probably require conditions similar to those created by the Swedish left in 1918. The parliamentary institutions through which such reforms might be obtained are available, even if Congress (as of this writing) has shown remarkable reluctance to assert its constitutional authority. To propel strong measures through these institutions will require massive popular pressure under the best of circumstances; regrettably the prospect of violent upheaval within and outside the urban ghettoes may also be a necessary goad to action. If this turbulence is to generate a willingness among the elite to grant concessions, larger sections of the elite will have to endorse the reform program; a unified elite bent on resistance could frustrate the possibility of a relatively non-violent transition to a more decent society. The relevant elites must be persuaded that armed repression is no longer feasible public policy; only a declining sense of their own legitimacy and substantial disaffection in the armed forces are likely to produce such a conversion.

Some of these conditions for radical reform seem to be emerging. There are indications that the volunteer army with its high percentages of black officers and enlisted men may be politically less reliable than its predecessor; the best evidence on this point is the Pentagon's sudden concern with the racial balance of the armed forces. Furthermore, substantial business and industrial interests that would profit from a diversion of resources from military projects to civil purposes are lending their support to initiatives for the expansion of expenditures on education, health, housing, and other social services. The most essential condition for radical reforms, however—the existence of a powerful popular movement—

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ering" (*Journeys Toward Progress*, Garden City, 1965, esp. pp. 360 ff.) after my analysis of Swedish development was well-advanced. Hirschman's schematic analysis of "engineering reform with the help of the perspective of revolution" parallels remarkably the Swedish situation in the closing years of World War I.

Comparisons between the English Reform Acts and Sweden's evolutionary transition to democracy are natural and instructive, particularly so when one consults Hamburger's *James Mill and the Art of Revolution* (New Haven: Yale University Press, 1963). Hamburger goes well beyond a recital of Mill's activity to a careful statement of the conditions and tactics for the achievement of fundamental reforms without violence. Hamburger stresses the importance of the army's strength and reliability, the government's self-confidence or "nerve," and the reformers' organization and leadership.

All of these works, but particularly the latter two, illustrate the often-neglected point that liberal democratic development entails policy *choices;* it does not follow automatically upon the fulfillment of certain "socio-economic preconditions."

[46] Dahrendorf, *Society and Democracy in Germany*, p. 15.

[47] Moore, *Social Origins of Dictatorship and Democracy*, p. 431.

is nowhere in sight, nor does it appear likely to develop. Without this essential prerequisite the United States will not modernize in the direction of a more decent society; it will not duplicate Sweden's successful experience on the radical liberal path of political development.

The Simple Act of Voting*

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Modern prodigies of election forecasting tend to obscure an important fact: Our ability to predict how voters will vote is far more solidly based than our ability to explain why they vote as they do. To acknowledge this fact, as any analyst of voting must, is to admit that some of the most interesting questions about voting and elections cannot at present be given satisfactory answers. What particular attitudes actually bear on voting, the character and quality of such attitudes and perceptions, the impact of campaigns, the extent to which elections serve to increase the responsiveness of officials on the issues that voters see as at stake in elections—secure knowledge about these matters must wait upon a securely established theory of voting. At present, there is no such theory.

The main purpose of the research to be reported here can be put quite simply: It was to improve upon current explanations of voters' choices and so to increase the relevance of studies of voting to politically significant questions. Obviously, such a goal requires one to adopt some criterion or criteria for assessing the degree to which it has been achieved. In our work we have chosen to regard one set of facts as a better explanation of how people vote than another, if it (a) shows a stronger, nonspurious statistical association with voters' choices, (b) involves a more believable (and nontrivial) account of the way voters arrive at their decisions, and (c) permits one to predict voters' choices more accurately.[1]

In the published literature the explanation of voting that best meets this test is that offered by Angus Campbell and his associates. The authors of *The American Voter,* in that book and elsewhere, have shown that one can discriminate with great accuracy between Democratic and Republican voters in presidential elections on the basis of voters' attitudes, variously weighted, toward a limited set of political objects: the personal attributes of the candidates, the relations of interest groups to the major parties, issues of domestic policy, issues of foreign policy, and the records of the major parties in managing the affairs of government.[2] Some 85 per cent of all voters

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* This article is a somewhat revised version of a paper presented at the 1972 Annual Meeting of the American Political Science Association. In the course of the work it reports, we received help from a great many people. Bruce Henderson, assisted by François Amar, did much of our programming; some of it was also done by Michael Stoto. Edward R. Tufte and Glenn Shafer of Princeton University and Joseph Verbalis of the University of Pittsburgh Medical School suggested some of the analyses we undertook and made other helpful comments. Other quite detailed criticisms came to us from Walter Dean Burnham of the Massachusetts Institute of Technology, James F. Miller of the Stanford Law School, Norman Nie of the University of Chicago, Edward V. Schneier of the City University of New York, and Orley Ashenfelter, Henry Bienen, Harry H. Eckstein, Michael Kagay, Charles A. Miller, Russell Nieli, Richard Quandt, Ronald Rogowski, Dennis Thompson, and Nicholas Wahl, all of Princeton University. Still others to whom we have accumulated debts are Harold Feivesen and Michael Reed of Princeton University, Peter Fishburn and Duncan Luce of the Institute for Advanced Studies, and Warren Miller of the University of Michigan. The Woodrow Wilson School of Public and International Affairs, Princeton University, provided financial support for our work, and in it we made use of computer facilities supported in part by National Science Foundation Grants NSF-GJ-34 and NSF-GU-3157. The data analyzed were made available by the Inter-University Consortium for Political Research and were originally collected by the Political Behavior Program of the Survey Research Center. Neither the original collectors of the data, nor the Consortium, nor the other institutions and persons we have mentioned, bear any responsibility for the analyses or interpretation presented here.

[1] In using this criterion of explanatory value, we are following C. G. Hempel who observes: ". . . it may be said that an explanation . . . is not complete unless it might as well have functioned as a prediction: If the final event can be derived from the initial conditions and universal hypotheses stated in the explanation, then it might as well have been predicted, before it actually happened, on the basis of a knowledge of the initial conditions and the general laws." (Carl G. Hempel, *Aspects of Scientific Explanation* [The Free Press: New York, 1965] p. 234.)

[2] See Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (John Wiley and Sons, Inc.: New York, 1960), pp. 66–68. See also Angus Campbell and Donald E. Stokes, "Partisan Attitudes and the Presidential Vote," pp. 353–371, in *American Voting Behavior,* Eugene Burdick and Arthur J. Brodbeck, eds. (The Free Press: Glencoe, 1959); Donald E. Stokes, Angus Campbell, and Warren E. Miller, "Components of Electoral Decision,"

(as represented in sample surveys) vote consistently with their attitudes on these matters. That is a higher proportion than vote consistently with their partisan identifications, their social status and position, or even their stated intentions.

Nonetheless, by the test just proposed, this explanation of voting has three important shortcomings. The 15 per cent of voters that one cannot account for in terms of it are a sizable proportion of the electorate; it provides no believable account of the way voters make up their minds; and it gives no satisfactory basis for *predicting* how voters will vote. The method used in *The American Voter* to account for votes is only (and was intended to be only) an accounting after the fact. This is so because the accuracy of the method is dependent on the weights assigned to each of the "components of electoral decision," and the multiple regression techniques used to assign these weights require that one know before beginning analysis how voters have voted. Of course, if the values of the assigned weights remained constant from election to election, the regression equation that accounted well for the results of one election would predict votes well in later ones. But this is not so; as calculated by Campbell and his associates, the values of the weights change.[3]

These shortcomings indicate that something is amiss in this explanation of voting, but what? A quite simple conception of decision making suggests a possible answer. Any decision may be thought of as involving both a set of considerations (conscious or unconscious) and a rule (conscious or unconscious) in accordance with which these considerations are weighed. Now, the attitudes in terms of which Campbell and his associates explain voters' choices are quite credible as *considerations* that enter into such choices; perhaps what is wrong is that the rule or rules that voters follow in translating these attitudes into votes have not been taken into account. To put it another way, perhaps the authors of *The American Voter* have identified the ingredients that go into voting decisions but not the recipe for mixing the ingredients.

A guess that this might be true was our point of departure from previous research on voting. We assumed tentatively that attitudes toward candidates and parties (and issues of policy, as these may be implicated in such attitudes) are the principal considerations that figure in voting decisions. We looked for the rule or rules voters apply to these considerations in deciding how to vote—hoping, but not assuming, that voters' decision rules would not turn out to be idiosyncratic. If we could identify such a rule or rules, we might have the better explanation of voting that we sought: Knowledge of the considerations that a person is taking into account *and* of the rule he is applying are a sufficient basis both for predicting what he will decide and for explaining his decision. Such knowledge is also a necessary, though not sufficient, basis for the obvious next step in the elucidation of a decision: explaining why particular considerations were taken into account and why a particular decision rule was used.

Research Procedure

Our basic data came from the surveys conducted in connection with each of five presidential elections (1952, 1956, 1960, 1964, 1968) by the Survey Research Center of the University of Michigan. Respondents' answers to a set of questions about the major parties and candidates gave us (as they did Campbell and his associates) our information about voters' attitudes. These questions, put to voters in pre-election interviews, were open-ended and identical in form for each of the elections.[4] Interviewers invited respondents to state what they liked and disliked about each of the candidates and each of the major parties in turn. Respondents replied with statements as various as "he [Stevenson] is divorced," "it's sort of begun to tighten up since the Republicans got in"; and, "he's [Eisenhower has] pulled us out of war."[5] Interviewers recorded up to five dif-

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The American Political Science Review 52 (June, 1958), 367–387; and Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *The American Political Science Review* 60 (March, 1966) 19–28.

[3] See Stokes, Campbell, and Miller, "Components of Electoral Decision," pp. 380–385, and Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," p. 20.

[4] The questions about parties were worded as follows: "I'd like to ask you what you think are the good and bad points about the two parties. Is there anything in particular that you (like, don't like) about the (Democratic, Republican) Party? What is that?" The questions about candidates were, "Now I'd like to ask you about the good and bad points of the (two, three) candidates for President. Is there anything in particular about (name of candidate) that might make you want to vote (for him, against him)? What is that?" The variable numbers for these questions in the code books of the Inter-University Consortium for Political Research are as follows: 1952, 0018–0021; 1956, 0015–0022; 1960, 0020–0027; 1964, 0021–0028; 1968, 0028–0037.

[5] For a larger sampling of responses to these questions, see Angus Campbell *et al.*, *The American Voter*, pp. 224–229.

ferent responses to each question for each respondent.

Having taken such responses to be the stuff of which voters fashion choices among candidates, we adopted a straightforward procedure in our search for the voter's decision rule. We formulated several rules—some simple, some complex—that voters might apply in proceeding from a set of attitudes to a decision to vote for a particular candidate. Each rule was then applied to the likes and dislikes that respondents had expressed about candidates and parties, yielding a set of predicted decisions that we could check against the votes reported by respondents in postelection interviews.

The reader should note that the predictions to be checked concerned only *how* voters would vote, not *whether* they would vote. Given that our primary interest was in predictions of the former sort, each decision rule was properly examined by applying it only to the attitudes of those survey respondents who reported having voted. Such rules could of course also be applied to the attitudes of nonvoters and we did apply them in that way, but this application was, for our purposes, of secondary interest. Even if our goals had been different, however, the distinction between the decision to vote and the decision to vote in a particular way is important to preserve. A comparison of candidates resulting in a choice among them should be one consideration—but not the only one—in the decision about whether to vote. Considerations about the efficacy, convenience, and cost of voting should also enter.[6]

We will report our results for one decision rule only, which, for the sake of convenience, we will call the Voter's Decision Rule, or simply, "the Rule." It is as follows:

> The voter canvasses his likes and dislikes of the leading candidates and major parties involved in an election. Weighing each like and dislike equally, he votes for the candidate toward whom he has the greatest net number of favorable attitudes, if there is such a candidate. If no candidate has such an advantage, the voter votes consistently with his party affiliation, if he has one. If his attitudes do not incline him toward one candidate more than toward another, and if he does not identify with one of the major parties, the voter reaches a null decision.

When applied to the data, this rule consistently (and unexpectedly) produced a more accurate set of predictions than any we could obtain with alternative decision rules.[7]

It will be noted that the Rule produces two types of predictions: (1) predictions from attitudes—in those cases in which the Rule yields an unequal net number of attitudes favorable to the candidates; (2) predictions from "residual partisanship"—in those cases in which the Rule indicates that the vote is to be in accordance with the voter's party affiliation. Null decisions are, for us, unpredictable; that is, they give us no basis for saying how the voter will vote. While they might be interpreted either as decisions to abstain or to make a random choice among candidates, we shall count as errors cases in which the Rule yields a null decision, just as we do cases of incorrect predictions from attitudes and of incorrect predictions from residual partisanship. A large part of our analysis of the Rule will be an examination of these three types of errors, especially of the first.

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[6] *Cf.* Anthony Downs, *An Economic Theory of Democracy* (Harper and Brothers: New York, 1957), pp. 260–274.

[7] For a precise description of the procedure employed in predicting how voters would vote on the basis of the Voter's Decision Rule, see Appendix.

It may be useful here to note some of the other decision rules we formulated and tested against data for the 1964 election. There were twenty-five such rules applied to data of the kind to which we applied the Rule, but they may be thought of as members of two families. The first of these comprised rules that differed from each other in regard to the responses to which they were applied but were alike in giving equal weight to all responses treated as considerations. Thus, in the case of each rule, voters were predicted to vote for the candidate toward whom they had the greatest net number of favorable attitudes. Some of the different sets of responses treated as considerations were those to questions about candidates, in which one or both were associated with favorable or unfavorable developments in some named area of public policy; about parties, in which one or both were associated with favorable or unfavorable developments in the general course of public policy, etc.

The thirteen rules of the second family were more complex. Their application involved four distinct steps: (a) Responses to the questions described in *f.n.* 4 were grouped into mutually exclusive subsets (as few as two, as many as nine). (b) These subsets of responses were assigned an order of importance (*e.g.*, responses involving specific issues of policy might be treated as of greatest importance to the voter, those concerned with the general course of public policy as of next most importance, etc.). (c) The responses (considerations) in each *subset* were summed, each response weighing equally. Then, (d) in the case of each rule, voters were predicted to vote for the candidate for whom they had the greatest net number of favorable attitudes in the subset of attitudes designated as most important, if there was such; if not, for the candidate for whom they had the greatest net number of favorable attitudes in the subset of attitudes designated as next most important, if there was such, etc. The rules of this general sort differed from each other in the order of importance they assigned to different subsets of considerations, and they were applied to subsets of responses that were defined in several different ways.

At a very early point in our research we formulated still other rules, and these were applied to responses other than those to which we applied the Rule and the rules just described.

Is the Rule a believable decision rule for voters? Its simplicity should count for it in this regard. One would expect the voter's decision rule to be a simple one, since the act of voting is not one in which most people invest much time or thought. Application of the Rule as stated amounts to saying, "There is more to be said for voting for this candidate than for his opponent (opponents). Therefore, I will vote for him," or "There is nothing more to be said for voting for this candidate than for his opponent (opponents). Therefore, I will stick with my party." This procedure is akin to what one does when one lists pros and cons on a sheet of paper in trying to decide whether to take a new job—akin, except that a voter in applying the Rule would not try to differentiate the weights of the various considerations that enter into his decision. If voters apply the Rule, undoubtedly most do so unconsciously; otherwise we should have been aware of their use of it long ago. The observation is not a strong point against the Rule's credibility, however; we are not conscious of the way we arrive at many kinds of decisions. We will return to this issue of the Rule's credibility after assessing its value in predicting how voters will vote.[8]

Findings: The Presidential Elections of 1952, 1956, 1960, and 1964

Principal Findings. The results in predicting how respondents voted in the two-candidate presidential elections of 1952, 1956, 1960, and 1964 were impressive. Our data enabled us to make a prediction based on the Rule for more than 98 per cent of those respondents who reported how they had voted in those elections. (If the Rule yielded a null decision, we made no prediction.) Most of these predictions were based on respondents' attitudes toward candidates and parties, only a few on the partisan identifications of respondents. We predicted *correctly*, on the average, the votes of 88 per cent of those who had reported how they had voted. Table 1 summarizes these results.

Party canvassers and political analysts alike often predict the votes of individual voters either from the voter's party affiliations or from

Table 1. Summary of Results in Predicting the Votes of Individual Voters in Accordance with the Voter's Decision Rule

Election	Sample Size[a]	% of Cases in Which Data Permit a Prediction from			% of Cases in Which Votes Were Correctly Predicted from		
		Attitudes Alone	Residual Partisanship	The Rule	Attitudes Alone	Residual Partisanship	The Rule
1952	1184	94.2	4.3	98.5	84.3	2.8	87.1
1956	1270	93.2	5.3	98.5	81.9	3.3	85.2
1960	1413	95.5	3.4	98.9	85.7	2.5	88.2
1964	1113	95.6	4.0	99.6	87.3	3.0	90.3

[a] Samples are samples of persons who voted, excluding those who refused to say how they had voted, or who said they did not remember how they had voted.

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[8] It is appropriate to acknowledge here some lines of thought akin to those that we brought to the present study. Anthony Downs in *An Economic Theory of Democracy* has made the notion of a decision rule for voters a central element in his theory of voting, as do a number of other writers, including Otto A. Davis, Melvin J. Hinich, and Peter Ordeshook in "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review* 64 (June, 1970), 426–448. Donald E. Stokes, Angus Campbell, and Warren E. Miller (in "Components of Electoral Decision") have used a summated scale, not interpreted as a decision rule, to indicate the preferences of voters with regard to each of the "components of the electoral decision" that they identify, and Walter F. Murphy and Joseph Tannenhaus (again, among others) have used such a scale to measure specific support for the Supreme Court. (See their "Public Opinion and the United States Supreme Court," *Law and Society Review* 2 [May, 1968], pp. 357–384.) Very similar to our own conception of voting is that of Richard Brody and Benjamin I. Page in "Indifference, Alienation, and Rational Decisions: The Effects of Candidate Evaluations on Turnout and the Vote" (unpublished ms.). The same may be said about the conception of voting underlying one of the models tested by William R. Shaffer in *Computer Simulations of Voting Behavior* (Oxford University Press: New York, 1972), pp. 65–68 and that developed by David M. Kovenock, Philip L. Beardsley, and James W. Prothro in "Status, Party, Ideology, Issues and Candidate Choice: A Preliminary Theory-Relevant Analysis of the 1968 American Presidential Election" (a working paper prepared for Specialist Meeting B:XI, Eighth World Congress of the International Political Science Association, August 31–September 5, 1970).

Table 2. A Comparison of Results in Predicting the Votes of Individual Voters in Accordance with the Voter's Decision Rule, from Partisan Identifications, and from Stated Intentions

Election	Sample[b] Size	% of Cases in Which Data Permit a Prediction from			% of Cases in Which Votes Were Correctly Predicted from			Accuracy[a]		
		The Rule	Party[c] Indent.	Intended Vote	The Rule	Party[c] Ident.	Intended Vote	The Rule	Party[c] Ident.	Intended Vote
1952	1184	98.5	94.3	87.9	87.1	76.1	82.8	88.4	80.9	94.1
1956	1270	98.5	90.5	89.2	85.2	75.3	83.1	86.5	83.2	93.1
1960	1413	98.9	91.3	87.0	88.2	78.5	81.9	89.1	86.0	94.1
1964	1113	99.6	94.1	88.7	90.3	78.4	84.1	90.7	83.4	94.8

[a] Correct predictions as a percentage of all predictions.

[b] Samples are samples of persons who voted, excluding those who refused to say how they voted, or who said they did not remember how they had voted.

[c] Here, and throughout, respondents who described themselves as "strong" Democrats, "not so strong" Democrats, or as Independents leaning toward the Democrats are treated as identifying with the Democratic Party; and respondents who described themselves as "strong" Republicans, "not so strong" Republicans, or as Independents leaning toward the Republicans are treated as identifying with the Republican Party.

his statement that he intends to vote for a particular candidate. Table 2 permits one to compare the scope and accuracy of predictions arrived at in both these ways with the scope and accuracy of predictions derived from the Voter's Decision Rule. Clearly, the Rule is a better basis for predicting votes than are voters' partisan identifications: It permits one to make predictions in a larger percentage of the cases, to predict a larger percentage of all votes correctly, and to make more accurate predictions. While predictions based on the Rule are not so likely to be correct as those based on the voter's statement of how he intends to vote, the Rule permits one to predict a larger proportion of all votes correctly. Its superiority in this regard comes from the greater number of predictions one can make; About ten per cent of each of the samples that figure in Table 2 were comprised of voters who had not decided, or would not say, how they intended to vote, but whose votes could nonetheless be predicted by applying the Voter's Decision Rule to their stated likes and dislikes of candidates and parties.

Secondary Findings. How well can one predict the division of the major party vote by applying the Rule? Table 3 shows the answer to be: Better than one can predict it from voters' partisan identifications, and at least as well as one can predict it from voters' statements of how they intend to vote. None of the three procedures for prediction is superior to the others in every case. In estimating the division of the vote in the sample, however, we find that estimates based on the Rule are on the average much more nearly correct than those based on partisan identifications and somewhat more accurate than estimates based on voters' stated intentions.[9]

As we have noted above, the Voter's Decision Rule is a rule for translating attitudes toward candidates and parties into a decision about how to vote, not whether to vote. If,

Table 3. Results in Predicting the Division of the Major Party Vote in Accordance with the Voter's Decision Rule, from Partisan Identifications, and from Stated Intentions

Election	% of Actual Vote for Major Parties That Was Democratic		% of Major Party Vote Predicted To Be Democratic		
	Population	Sample	The Rule	Intended Vote	Party Indent.
1952	44.6	41.7	45.0	42.7	59.2
1956	42.2	40.2	44.8	42.6	58.0
1960	50.1	49.0	49.4	45.1	58.5
1964	61.3	67.4	69.5	72.3	65.5

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[9] There are errors involved in all the estimates presented in Table 3 (particularly large ones in 1964). Since the three procedures for estimating the division of the vote have been applied to the same data for any given election, however, these sources of error have been (for our purposes) held constant.

however, the outcome of the former decision does figure in the latter, it is reasonable to suppose that respondents for whom the Rule yields either a null decision or a narrow advantage for one candidate will be less likely to vote than those for whom the Rule yields a strong advantage for one of the candidates. What happens if we apply the Voter's Decision Rule to all respondents, nonvoters as well as voters?

Figure 1 shows the relationship between nonvoting and respondents' attitudes toward candidates and parties, aggregated in accordance with the Rule. In that figure, data from the surveys of the presidential elections of 1952, 1956, 1960, and 1964 have been averaged. Along the horizontal axis are the net scores that the Voter's Decision Rule yields: Net scores favorable to Democratic candidates[10] are positive, and net scores favorable to Republican candidates are negative. In the center of the scale are the scores given those respondents whose attitudes gave no advantage to any candidate. Scores of +0 were given such respondents when they identified with the Democratic Party; scores of −0 when they identified with the Republican Party; and scores of 0 when they identified with neither party. The figure's essential message is obvious: Rates of nonvoting among respondents were strongly related to the scores that the Rule yielded for them. On the average, almost 70 per cent of those with net scores of zero (i.e., respondents reaching a null decision) failed to vote, while quite low rates of nonvoting were associated with high (absolute) net scores, particularly among respondents who preferred Republican candidates.

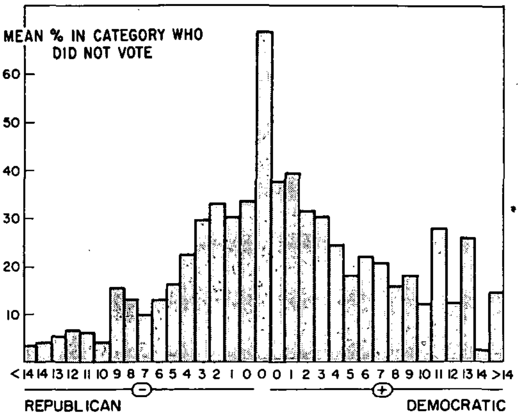

Figure 1. Mean rates of nonvoting related to net scores derived from the application of the voter's decision rule, for the elections of 1952, 1956, 1960, and 1964.[a]

[a] Samples for the four elections are samples of all respondents who said they had voted (including those who would not say how they had voted or who said they did not remember how they had voted) and all who said they had not voted.

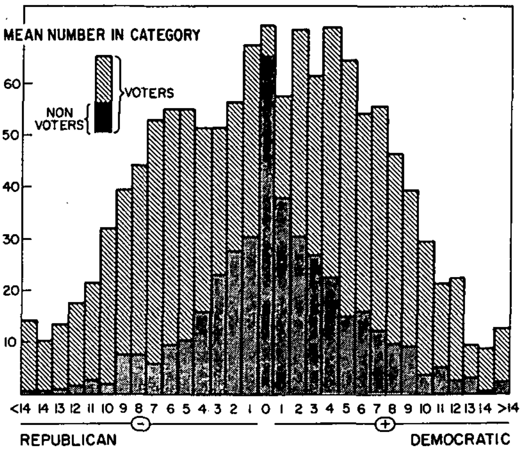

Figure 2. Mean numbers of voters and nonvoters related to net scores derived from the voter's decision rule, for the elections of 1952, 1956, 1960, and 1964.[a]

[a] Samples for the four elections include all respondents who said they had voted (including those who would not say how they had voted or who said they did not remember how they had voted) and all who said they had not voted.

Now, the high rates of nonvoting associated with net scores of low absolute value would be of little significance if almost all voters had high net scores. That, however, is not the case. Figure 2 shows the numbers of respondents in the subsamples of voters and nonvoters, classified by net scores from the Rule. These are the raw data underlying Figure 1, except that we have collapsed the three zero categories into one.[11] From the figure, two facts are immediately apparent: In the four elections a large proportion of the respondents were not deeply committed to the candidate they preferred; and, on the average, almost 70 per cent of the respondents who did not vote had scores ranging from +4 to −4 inclusive. There is thus little room for doubt that the *rates* of nonvoting associated with differing net scores have a strong impact on the actual *amounts* of nonvoting.[12]

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[10] We are using the term, "net score," here and elsewhere to refer to the quantity yielded by subtracting the net number of favorable attitudes that a voter has toward the Republican candidate from his net number of favorable attitudes toward the Democratc candidate. See Appendix.

[11] Note that Figure 2 is really two figures: The average number of voters with each net score is given by the full height of the bars, while the average number of nonvoters is given by the lower portions of the bars.

Errors in Principal Findings. In the four two-candidate presidential elections from which our data thus far have been taken, respondents whose votes were not predicted correctly ranged from 14.8 per cent of all the respondents who voted in 1956 to 9.7 per cent of all such respondents in 1964. We will discuss the shortcomings of the Voter's Decision Rule as a basis for predicting votes by examining separately and in reverse order, errors attributable to incorrect predictions from attitudes, to incorrect predictions from residual partisanship, and to null decisions.

In 1952, 1956, and 1960, the votes of those voters for whom the Rule yielded a null decision made up about 10 per cent of all votes not predicted correctly; in 1964, about 5 per cent. When our results for all four elections are considered together, there is, or seems to be, a pattern in the behavior of these respondents. In all four elections (and in 1968 as well), they cast their votes preponderantly for the candidate who actually won the election; this was true even in 1960 when the sample as a whole gave a tiny margin of victory to Richard Nixon.

Respondents whose attitudes gave no advantage to a candidate and whom we wrongly expected to cast votes consistent with their partisan identifications ranged from 8 per cent (1960) to 17 per cent (1956) of all respondents whose votes were not predicted correctly. The behavior of residual partisans generally was akin to that of the voters for whom the Rule yielded null decisions: In the elections of 1952, 1956, and 1964, residual partisans of the winning candidates were more likely to cast votes consistent with their partisanship than were residual partisans of the loser. In 1952 this tendency was pronounced: 80 per cent of the Republicans who voted as residual partisans voted for Eisenhower, while only 58 per cent of their Democratic counterparts voted for Stevenson.

Most of our errors—never less than three-fourths of them—were incorrect predictions from attitudes. These were cases in which the voter's attitudes gave an advantage to one candidate while his vote went to the other. Errors of this sort were so large a part of the total number, not because attitudes were a poor basis for prediction, but because predictions of this sort were so large a part of all predictions. Our predictions from attitudes were more accurate in every election than our predictions from residual partisanship.

Perhaps the most striking fact about the errors made in predictions from attitudes is the extent to which they were associated with the net scores that the Voter's Decision Rule yielded for respondents. Figure 3 gives the rates of error, averaged over the four elections, that were associated with predicting the votes of respondents with different net scores. Predictions for respondents with high (absolute) net scores were very accurate, those for respondents with low (absolute) net scores much less so. Figure 4 shows the mean proportion of the errors in predictions from attitudes that were associated with each net score. On the average, 84 per cent of all errors made in predictions from attitudes, and 57 per cent of all

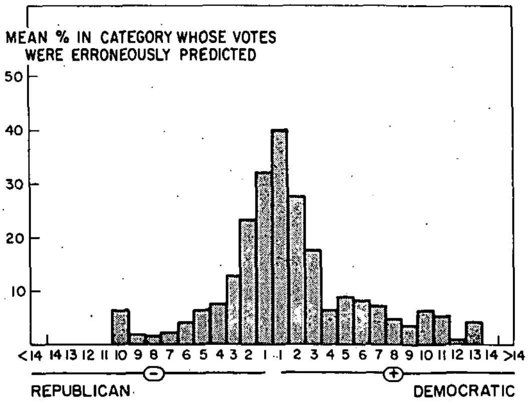

Figure 3. Mean rates of error in predictions from attitudes, related to net scores derived from the voter's decision rule, for the elections of 1952, 1956, 1960, and 1964.

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[12] William R. Shaffer tried to simulate decisions of voters to vote and voters' choices among candidates in accordance with rules inspired by the analysis of the "components of electoral decision" by Campbell, Converse, Stokes, and Miller. His decision rule for choice among candidates was nearly the same as ours, and he tested the accuracy of his simulation against data from the SRC survey of the 1964 presidential election that was only slightly different from that with which we worked in analyzing that election. His rule for decisions about whether to vote did not work well, and he concludes that "abstention does not appear to be rooted in the same preferential decision-making process" (Shaffer, *Computer Simulations of Voting Behavior*, p. 133). We think there is good reason to believe that some of the same considerations enter both the decision to vote and the decision about how to vote, but there is equally good reason to think that each is determined by a different *set* of considerations. For instance, once in the polling booth, considerations of convenience will play no part in a choice among candidates, since it is equally convenient to vote for any of them, but convenience does have an important bearing on the decision about whether to vote.

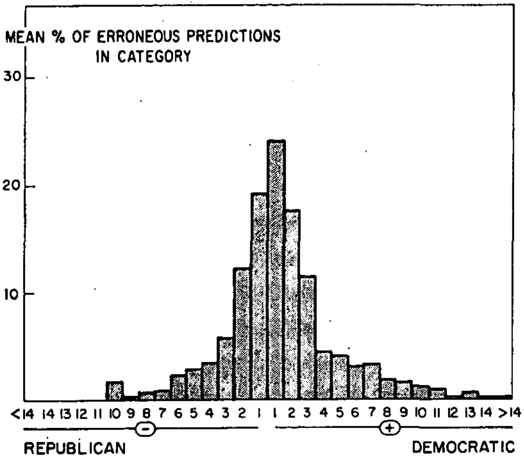

Figure 4. Mean percentages of all errors in predictions from attitudes that were associated with each of the net scores derived from the voter's decision rule, for the elections of 1952, 1956, 1960, and 1964.

errors, were made in predicting the votes of respondents with net scores ranging between +3 and −3, inclusive.

When the net number of attitudes favoring one candidate or the other is held constant, another fact about the errors in predictions from attitudes emerges: The greater the total number of attitudes expressed by a respondent, the greater was the likelihood that the Rule would predict his vote incorrectly. Table 4, in which the data from the four elections are pooled, demonstrates this relationship. From the table, one can see that our error rate went down as the net number of attitudes favoring a candidate rose, but that it rose fairly sharply with increases in the total number of attitudes from which the net scores were computed. The effect of this latter phenomenon on our ability to make correct predictions was not trivial. About one-third of the cases that figure in Table 4 were cases of respondents who had expressed from 9 to 20 attitudes toward the parties and candidates, while 46 per cent of the errors tallied in the table occurred in predicting the votes of such respondents.

Table 4. Error Rates in Predictions from Attitudes Related to the Total Number of Attitudes Expressed (Data from Surveys of the Elections of 1952, 1956, 1960, and 1964)

Net Score	Number of Attitudes		
	1–4	5–8	9–20
+1, −1	26.9%[a]	34.5%	42.9%
+2, −2	16.7	24.2	36.6
+3, −3	5.9	14.1	24.5

[a] Percentages are errors as a proportion of all predictions in each cell.

Table 5. The Relationship of Party Identifications to Rates of Error in Predicting Votes from Attitudes

Election of	Vote Predicted Was		
	Consistent with Party Ident.	Inconsistent with Party Ident.	Other (Independents)
1952	5.7[a]	24.0	15.9
1956	7.1	36.3	20.6
1960	6.4	37.7	9.3
1964	5.8	27.7	4.9
Mean	6.3	31.4	12.7

[a] Figures are incorrect predictions as a percentage of all votes predicted in the indicated categories.

The errors made in predictions from attitudes were also systematically related to the partisan identifications of respondents. We did well in predicting votes of respondents when our predictions were consistent with their partisan identifications, not nearly so well when our predictions were inconsistent (see Table 5). Some 40 to 50 per cent of the votes erroneously predicted from attitudes, and 30 to 43 per cent of all votes not predicted correctly, were those of respondents who we wrongly predicted would cross party lines.

Errors in predictions from attitudes were related to partisanship in another way also. In the landslide elections of 1952, 1956, and 1964, we were more likely to be wrong in predicting votes for the losing candidate than in predicting votes for the winner. As one can see in Table 6, this effect was relatively small in 1964 but substantial in 1952 and 1956. The data presented in Table 6, together with those

Table 6. Comparison of Rates of Error in Predicting Votes from Attitudes for Democratic Candidates vs. Republican Candidates

Election of	Vote Was Predicted To Be	
	Democratic	Republican
1952	14.1[a]	7.7
1956	17.4	8.2
1960	10.9	9.7
1964	7.8	10.7

[a] Figures are incorrect predictions as a percentage of all predictions in the indicated categories.

already presented regarding the votes of residual partisans and of the respondents who reached null decisions, suggest that the short-term forces affecting elections (in the sense Campbell and his associates use that phrase) were not adequately taken into account in our predictions.

Sources of Error in Principal Findings. The construction that one puts on this and on the other facts that we have so far set forth will differ, depending on what one takes to be the main source of our errors. If it is right to think of decisions as being determined by a set of considerations and a rule for weighing such considerations, then, logically, our predictions could be in error because

(1) subconscious considerations were a significant factor in the decisions of some respondents;

(2) the decisions of some respondents were determined wholly or in part by considerations of which they were aware at the time they were interviewed but which were not among those on which our predictions were based;

(3) some respondents' decisions were determined wholly or in part by considerations which arose in their minds after they were interviewed and before they voted;

(4) some respondents did not give equal weight to the considerations that entered into their decisions—that is, the Rule was not the decision rule that they used;

(5) there were errors, systematic or random or both, in the data to which the Rule was applied;

(6) some respondents, or we ourselves, made mistakes in applying the Rule;

(7) some respondents made mistakes in voting (as, for instance, by pulling the wrong lever) and reported these mistaken votes as their votes.

We have not tried to find out the extent to which our errors may be explicable in terms of the first or last of these propositions. It is not clear how we could do so, given the data. Proposition (2) does not seem a likely explanation of most of our errors since it was the respondents themselves, in answers to open-ended questions and subjected to repeated probes from interviewers, who volunteered the attitudes we have treated as considerations. Moreover, among the respondents whose votes we predicted from attitudes and who expressed a preference for a candidate, our predictions were consistent with their preferences about 96 times out of a hundred. In the absence of any specific hypotheses about what considerations may have been left out of account, we made no further effort to test the proposition. We have considered at length whether, and to what extent, our errors might be explained in terms of propositions (3), (4), and (5). Proposition (6), if true, would lead to behavior (or an attribution of behavior) equivalent to that described in Proposition (4). Except for checks on our own work, we made no effort to test it separately.

Proposition (4)—that some voters do not give equal weight to the considerations bearing on their decisions—has considerable common sense appeal as an explanation of our erroneous predictions. One might suppose, for example, that in the election of 1960 the so-called religious issue weighed more heavily than other issues with Catholic voters. If that were so, one would expect predictions derived from the Rule to have been in error more frequently for Catholics predicted to vote Republican than for other voters so predicted. This expectation is borne out: Our rate of error for those Catholics predicted to vote Republican on the basis of their attitudes was about 25 per cent, while that for all those respondents predicted to vote Republican on the basis of their attitudes was 9.7 per cent. Moreover, our rate of error in predicting that Catholics would vote Democratic was unusually low—about seven percentage points below that of all those respondents predicted to vote Democratic on the basis of their attitudes. In the case of these respondents, it would seem that something—and Kennedy's Catholicism is the obvious guess about what that something might be—made the sets of considerations that inclined them toward Kennedy of more than average weight.

The argument just made becomes less convincing, however, when our data are examined further. The above- and below-normal rates of error in predicting the votes of Catholics for Nixon and Kennedy, respectively, do not compel the conclusion that those Catholics into whose decisions the religious issue entered, gave it unusual weight. Counting against that conclusion is the fact that our predictions of the votes of Catholics accorded with their own pre-election expression of preferences for a candidate just about 98 times out of a hundred;[13] this means that *at the time of their pre-election interviews* almost all of these voters saw their attitudes as adding up to a preference for the candidate that they would have selected, had they been applying the Voter's Decision Rule.[14] In any event, heavy weighting of the religious issue by Catholics cannot account for any sizable proportion of the errors in our predictions for the 1960 elections. A "normal" rate of error in predicting that Catholics would vote Republican would have reduced our over-all error rate for that election by a trivial amount; a "normal" rate of error in predicting that Catholics would vote Democratic would have *raised* our overall rate of error by about two percentage points.[15]

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[13] This figure holds, of course, not for all Catholic voters, but for those Catholic voters (a) whose votes were predicted on the basis of their attitudes, and (b) who expressed a preference for a candidate in their pre-election interview.

Proposition (3)—that some respondents changed their decisions in response to considerations arising in their minds after their pre-election interview—could have been ruled out as an explanation of our erroneous predictions, if such predictions were not associated with changes of mind or inclination on the part of respondents. We attempted to test for such an association in two different ways. The first test was an obvious one. Most respondents voted as they had intended to vote at the time of their pre-election interview; others did not vote as they intended; and still others, when first interviewed, did not know, or would not say, how they intended to vote. If changed minds or changed inclinations were the source of many of our errors, the rates of error in predictions for the last two groups of respondents should be higher than those for the first group. Furthermore, a sizable proportion of the respondents whose votes we predicted incorrectly should have been respondents from the last two groups.

The data support these hypotheses. The rates of error in predictions for respondents in the three groups differed dramatically, as Table 7 demonstrates. Moreover, on the average, 73 per cent of our errors in predictions from attitudes occurred in predicting the votes of respondents who did not vote as they had intended to vote or who were undecided at the time of the pre-election interview.[16] In most of the cases in which we made erroneous predictions for respondents who voted as they had intended (93 per cent, on the average), respondents had net scores that fell between ±3; for these respondents, the net scores derived from the Rule appear to have given us an accurate measure of *firmness* of intent.

A second test for an association between erroneous predictions and opinion changes was only a little less obvious. Let us assume, as seems amply warranted from panel studies of voting, that during a campaign a voter is more likely to stick to his decision (or inclination) about how he will vote than to change it. Let us assume also (1) that, during any given unit of time during a campaign, a voter is less likely to change his mind twice than once, three times than twice, and so on; and (2) that these probabilities that a voter will change his mind increase with increases in the number of units of time elapsed after he has decided how he

Table 7. Rates of Error in Predicting the Votes of Respondents of Various Intentions (Predictions Based on Attitudes of Respondents)

Election of	Respondents Who Voted as Intended (%)	Did Not Vote as Intended (%)	Were Undecided (%)
1952	3.4	84.5	33.0
1956	4.9	83.3	36.0
1960	3.5	62.3	35.8
1964	2.0	84.1	35.6
Mean	3.5	78.6	35.1

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[14] We thought that differentiated weighting of considerations by respondents in two other groups—Southerners in 1964 and blacks in 1968—might be reflected in the error rates of our predictions for them. The error rate in predictions for Southern respondents voting in the 1964 election turned out to be virtually identical to that for all respondents—9.1 per cent as against 9.3 per cent. The case of black respondents in 1968 is astounding, but it makes no better evidence for unequal weighting of considerations than the case of Catholic voters in 1960. Our predictions for black respondents voting in the election of 1968 were correct in 97 per cent of the cases, and we made no errors at all in predicting that blacks would vote Democratic. This phenomenal accuracy might seem to be evidence that black respondents were tied to the Democrats by a consideration or considerations of more than average weight. That could be true, but two facts count against this interpretation of the data: (1) In fewer than 20 per cent of the cases did the Rule yield a net score of +3 or less for those black respondents predicted to vote for Humphrey; (2) among those black respondents whose votes were predicted from attitudes and who expressed a preference for a candidate in their pre-election interviews, our predictions squared with the preference in 98 per cent of the cases.

[15] Probably the best way to find out the extent to which errors in predictions based on the Rule derive from the unequal weighting of considerations by respondents would be to modify the set of questions described in *f.n.* 4 so that respondents could say whether any particular like or dislike was of unusual or decisive importance. This kind of data, which we would have liked very much to have had, was unavailable to us.

[16] The figures from which this average was computed were, for 1952, 73.5 per cent; for 1956, 66 per cent; for 1960, 71.9 per cent; and for 1964, 80.4 per cent.

will vote. Given these conditions, the likelihood that any particular respondent's vote was wrongly predicted should vary with the length of time before election day that he was interviewed, if our prediction squared with his decision at the time he was interviewed.

To test for this hypothetical relationship of time to error, we pooled the data from the four elections and examined the accuracy of our predictions only for those respondents for whom the Rule had yielded net scores of ±3, ±2, and ±1. These were the respondents most likely to have changed their minds, and together they comprised just over 28 per cent of all respondents and 58 per cent of those respondents whose votes were not correctly predicted. Then, working back from election day, we bunched respondents into groups of 50, adding respondents to each such group according to the recency of their interviews, until we arrived at that number. This done, we computed our rate of error in predicting the votes of each of these groups of respondents and the mean number of days before election day that respondents in each group were interviewed.[17]

A simple linear regression of the first of these variables on the second shows them to be strongly related. The equation yielded is:

$$\text{Rate of Error} = 17.4 + .23 \text{ (days before election)}$$

In this formula (and those immediately following) the phrase "days before election" stands for the mean days before election day that a group of fifty respondents was interviewed. The formula describes a relationship that in a statistical sense explains some 28 per cent of the variance in the dependent variable; and, since the standard error of the estimated coefficient is .07, the relationship is statistically significant ($t = 3.15$). Most interesting, perhaps, is the implication of the equation's constant term: Had the interviews of these respondents been conducted on election day, the mean rate of error in predicting their votes would have been 17.4 per cent, not 34.1 per cent, as it actually was.

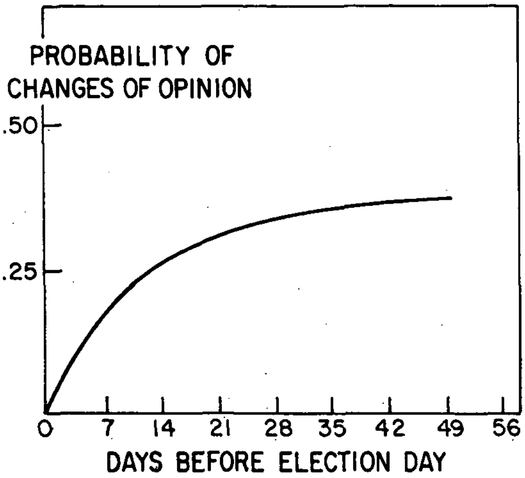

Figure 5. Hypothetical relationship between the likelihood that opinions will change and the time that attitudes toward parties and candidates are expressed.

And it is quite possible that this value for the constant term is too high. The volume of partisan propaganda is normally much heavier in the last two or three weeks of a presidential campaign than it is earlier.[18] We might therefore suppose the relationship between time and changes of opinion to be like that shown in Figure 5, in which the likelihood of such changes (and thus the error rates of our predictions) at first increases rapidly with increases in the number of days between election day and the time the opinions were expressed, then more slowly. By regressing the rates of error in our predictions for groups of respondents on the logarithm to the base 2 of the mean number of days before election day that the respondents in each group were interviewed, one can see if a curve like that shown in Figure 5 fits the data that entered into the first regression. The equation produced by this new regression is:

$$\text{Rate of Error} = 5.3 + 4.03 \text{ } (\log_2 \text{ days before election})$$

This second equation accounts for as much of the variance in the dependent variable as did the first and yields an equally reliable estimate of the regression coefficient ($R^2 =$

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[17] In raw form our data consisted of the number of respondents interviewed on a given date and the number of these whose votes had been predicted incorrectly. If we had treated all respondents interviewed at the same time as the unit of observation, we would have given equal weight to cases involving only one respondent (with a consequent error rate of either 100 per cent or zero per cent) and cases involving many more. It was to avoid this weighting problem that we bunched respondents into equal groups of 50.

[18] Joseph Napolitan, the well-known campaign consultant, observes: "Political time buying on television and radio follows a traditional curve; you usually start about three weeks before the election and build up to a heavy concentration the final week. We've even evolved a reasonable formula: spend one sixth of your money the third week before the election, two sixths the second week, and three sixths the final week" (*The Election Game:* [Doubleday and Co., Inc.: Garden City, N.Y., 1972], pp. 143–144).

.28, $t = 3.14$). The value of the equation's constant term implies that our mean rate of error in predicting the votes of groups of respondents would have been 5.3 per cent (or less than one-sixth of what it was, on the average), if those respondents had been interviewed one day before election day. The equation as a whole implies that, starting from the day before the election, the error rate in predictions derived from the Rule will rise by four percentage points with each doubling of the length of time before election day that respondents are interviewed.

Clearly, we cannot rule out the possibility that many of our erroneous predictions resulted from respondents' changing their opinions in response to considerations not present in their minds at the time of their pre-election interviews. Indeed, given the evidence so far, that is a highly credible explanation of our errors. These may be as strongly related to net scores as they are because respondents whose decisions were close ones were more likely than other respondents to have changed their minds and to have changed them in a direction favorable to the candidate favored in the flow of information in each election (if one candidate had such an advantage). The relationship we found between errors and the total number of attitudes expressed by respondents may reflect a tendency of these respondents to expose themslves to campaign discussion more than other voters did; as one would expect if that were true, our data show a quite strong negative relationship between the number of responses a respondent gave to the questions described in f.n. 4 above and the degree of his political involvement, as measured by the Survey Research Center's involvement scale. The relatively high rates of error associated with predictions of votes inconsistent with partisan identifications may mean that potential apostates were more likely than other voters to have had their initial preferences challenged by their friends and families and, because that was so, were more likely to have changed their minds.

We have already noted that a failure to account adequately for the short-term forces at work in the four elections (that is, issues that were important in particular elections and the appeal of the particular candidates) may be the reason that our predictions of votes for the losing candidates in 1952, 1956, and 1964 were less accurate than those for the winning candidates. In each of these cases we predicted that more respondents would vote for the losing candidate than did (or said they did). Evidence suggests that the net impact of short-term forces was to the benefit of the Republicans in 1952 and 1956, to the benefit of the Democrats in 1964, and nearly neutral in 1960,[19] hence, or so one would argue, the pattern of error in predictions derived from the Voter's Decision Rule.

But might not some of our errors in predicting votes for the losing candidate have come from respondents having reported giving them too few votes? We think that the answer is "yes" and that such an answer is not inconsistent with what we have just said about the influence of short-term forces in the four elections.

After every presidential election since 1944 (except that in 1960), respondents in the samples drawn by the Survey Research Center have reported voting for the winning candidates by a larger margin than the nation's voters in fact gave to them. Aage Clausen has suggested three ways in which a bias of this sort could have arisen:

> (1) Nonvoters who preferred the winning candidates by larger majorities than voters falsely reported that they had voted.
>
> (2) Some respondents who would not otherwise have voted were stimulated to vote by the pre-election interview and in voting gave larger shares of their votes to the winning candidates than other voters did.
>
> (3) Some respondents who voted for the losing candidates falsely reported having voted for the winning candidates.[20]

We may put the first of these hypotheses to one side, since it was refuted—at least for the election of 1964—by Clausen's research. Checking the votes reported in that year against official records, he found that "Wherever we draw the line . . . whether to include just those respondents whose vote report is matched by the official record or to include all respondents whose report is not contradicted by the record, the sample vote division stands at 67–33 in favor of Johnson, a slight fraction less than the over-all sample vote division."[21] Of the two remaining hypotheses, Clausen prefers the second.

It can be stated more fully as follows: Being interviewed in an election survey will interest some respondents in voting who would

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[19] See Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *The American Political Science Review* 60 (March, 1966), pp. 19–28.

[20] Aage R. Clausen, "Response Validity: Vote Report," *Public Opinion Quarterly* 32 (Winter, 1968–1969), 602.

[21] *Ibid.*, p. 603.

Table 8. Some Consequences of Adjusting Samples by Removing Those Who May Have Been Stimulated to Vote by Interviewing

Election of	Vote in Population (% Dem.)	Vote in Sample (% Dem.)		Vote Predicted by the Rule (% Dem.)		Difference in Error Rates, by Party[a]		Total Vote Predicted Correctly (%)	
		Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.
1952	44.6	41.5	41.7	44.3	45.0	8.0	8.0	89.1	87.1
1956	42.2	40.2	40.2	43.9	44.8	10.4	11.1	87.9	85.2
1960	50.1	48.2	49.0	49.2	49.4	2.1	1.4	90.4	88.2
1964	61.3	66.3	67.4	66.3	69.5	2.6	3.7	91.1	90.3

[a] The difference in our rates of error in predicting Democratic and Republican votes by respondents.

not otherwise have been interested, just as canvassing by party workers does. Such respondents will be poorly informed about the election, and any additional information about it that comes to their attention can easily be decisive for their votes. This being so, they will cast their votes disproportionately for the candidate who is favored in the flow of information about the election, if a candidate is so favored. However, because the Democratic party has an initial advantage among those voters who are least interested in politics, a Republican bias in the flow of information, to produce any given advantage in votes, must be greater than a Democratic bias.

If the statements just made are correct and if our earlier statement about the net impact of short-term forces in the four elections is correct also, one would expect:

(1) Nonvoting to be proportionately less among survey respondents than among the population of eligible voters;

(2) the winning candidate's margin of victory in 1952, 1956, and 1964 to be proportionately greater among survey respondents than among the population of eligible voters who voted;

(3) the winning candidate's margin of victory among respondents to be proportionately greater in 1964 than in 1952 and 1956;

(4) errors in predictions derived from the Voter's Decision Rule to be proportionately greater in 1952 and 1956 than in 1964.

All these expectations are borne out in the data.

One nonetheless hesitates to accept the hypothesis; the case in support of it involves too many unproved assumptions. Fortunately, it can be tested further. We know, roughly at least, by how much the turnout of respondents exceeds that of the population of voters—it is by about 10 percentage points. We can also identify in our data the respondents who according to the hypothesis are most likely to have been stimulated to vote by interviewing: They should have been voters for whom the Voter's Decision Rule yielded net scores of zero or of small absolute values and who expressed few opinions about parties and candidates. What happens if the votes of such respondents do not figure in our calculations?

Table 8 shows some of the things that happen. The division of the vote in the sample moves a bit closer to that in the population in one case (1964) and a bit farther away in two cases (1952 and 1960). The division of the vote predicted by the Rule moves closer to the division of the vote both in the sample and the population in three cases, further away from that in the population in one (1960). In two cases (1956 and 1964) our rates of error in predicting Republican votes become more like our rates of error in predicting Democratic votes, and our over-all rate of error is reduced in every case. It does appear that an interview effect on the turnout of respondents may have increased our rates of error in predicting votes in the four elections, although not by a great deal.[22]

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[22] A more precise description of the procedure we followed in adjusting samples is in order here. In each case the number of respondents removed from our samples of voters was equal to 10 per cent of the corresponding population sample. Those respondents removed all had net scores between ±4 and had expressed no more than four likes and dislikes of parties and candidates. Because the number of respondents to be removed was in every case fewer than the total number of respondents with these scores, respondents were removed from the sample in this order: (0), (0); (±0), (0); (±0), (2); (±0), (4); (±1), (1); (±1), (3); (±2), (4); (±3), (3); (±4), (4); where (±0), (0) indicates respondents with net scores of ±0) who expressed no likes or dislikes of parties and candidates, (±0), (2) indicates respondents with net scores of (±0) who expressed two likes or dislikes of parties and candidates, etc. When the total number of respondents removed would have been too large if we

Clausen's third suggestion—that respondents in postelection surveys may tend falsely to report having voted for winning candidates—remains to be considered as a possible explanation of our over-prediction of votes for the losing candidates in 1952, 1956, and 1964. If the suggestion is correct, how might this postelection bandwagon effect reveal itself in our data? Our best efforts to think about this question led only to the following line of thought: Voters may forget how they have voted; having done so, and as a part of that general movement toward consensus that tends to follow presidential elections, they later "remember," if asked, having voted for the winner.[23] If such forgetfulness leads voters to misreport their votes, the amount of such misreporting should increase as the time intervening between the election and the postelection interview increases. Therefore, if one can correctly predict how any given respondent will vote, the chance that the prediction will be "falsified" by a misreported vote will increase directly with the time intervening between the election and the respondent's postelection interview.

Whatever one's attitude toward the notions just advanced, it is a fact that a strong association exists between the dates of the postelection interviews in the surveys of the elections of 1956, 1960, and 1964, and our rates of error in predicting how respondents voted in those elections. In testing for this association, we pooled data for these three elections only (the precise dates of postelection interviews in the survey of 1952 were unavailable), but otherwise we followed a procedure precisely comparable to that we adopted in examining the relationship between the times of pre-election interviews and the rates of error in our predictions. In this instance a regression of error on time yielded this equation:

$$\text{Rate of Error} = 11.3 + 3.4\ (\log_2 \text{ days after election})$$

This relationship easily passes tests of statistical significance ($t = 2.42$) and accounts for some 32 per cent of the variance in the dependent variable. The constant term implies that our rate of error in predicting the votes of the respondents concerned would have been less than half of what it actually was, if postelection interviews had all been conducted one day after election day. (In this test, as in the earlier ones that examined the relationship of times of interviews to error rates, we considered only those respondents whose votes were predicted from attitudes and for whom the application of the Rule produced net scores between ±3.)

Although the evidence bearing on our errors in prediction has necessarily been indirect, it points rather clearly to these conclusions: (1) Unequal weighting of considerations by respondents; subconscious considerations; conscious but unarticulated considerations; and the stimulation of voting by surveys cannot be ruled out as sources of error. As of now, however, the case for regarding any of them as major sources of error is not a strong one. (2) There is a good case for thinking that delay in administering postelection interviews is a significant source of erroneous predictions. (3) Early pre-election interviewing was probably the most important source of errors in predictions derived from the Rule; pre-election interviewing on or near election day should reduce the error rate of such predictions to something like 5 or 6 per cent. One is led to this view by the high correlation of our predictions with voting intentions; our very low rates of error in predicting the votes of respondents who voted as they intended; the strong association of errors in prediction with low net scores, changed intentions, and indecision; the apparently great sensitivity to short-term electoral forces of persons with low net scores; and the strong (and perhaps curvilinear) relationship between the dates of pre-election interviews and erroneous predictions.

Findings: The Presidential Election of 1968

Since the 1968 election presents some problems for analysis that elections involving only two candidates do not, we will discuss our results for that election separately. A few words will suffice to describe the most important of these. The Voter's Decision Rule yielded predictions for more than 99 per cent of those respondents who voted. An unusually large number of these predictions were for respondents whose attitudes gave no advantage to any candidate (7.8 per cent of those respondents who voted) and whose votes were therefore predicted from their partisan identifications. Our predictions were correct for 80.9

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had removed all those in the last set of respondents to figure in our calculations, we kept within bounds by taking a smaller number proportionately from respondents with plus and minus net scores.

[23] Politically sophisticated readers may find it difficult to believe that anyone could forget how he voted for President. If so, they should try to recall for sure how they voted for freeholder or registrar of deeds at the last election; for some voters, a vote for president may carry with it no greater emotional involvement than the sophisticate's vote for one of these lesser officers of government.

Table 9. Results in Predicting the Votes of Individual Voters in Accordance with the Voter's Decision Rule, from Partisan Identifications, and from Stated Intentions (1968 Election)

Basis of Prediction	% of Cases in which Data Permit a Prediction	% of Cases in which Votes were Predicted Correctly	Accuracy of Predictions[a]
The Voter's Decision Rule	99.1	80.9	81.6
Stated Intentions	86.6	77.0	89.0
Party Identification	91.6	68.9	75.2

[a] Correct predictions as a percentage of all predictions.

per cent of the respondents who voted. This figure is about four percentage points below the comparable one for 1956, the two-candidate election for which the rate of error in our predictions derived from the Rule was highest.

Even so, relative to the alternatives we have been discussing, the Rule was a good basis for predicting votes in 1968. How predictions for individual voters derived from the Rule compare with those based on voters' declared intentions and their partisan identifications is shown in Table 9.

Table 10 gives our results in predicting the division of the major party vote in 1968.

Neither set of results indicates any need for revising our earlier assessment of the relative merits of the three bases for prediction.

One reason for the relatively high rate of error in our predictions for 1968 is plain: An unusually large number of respondents changed their minds about how they would vote in the course of the campaign. Almost 9 per cent did, more than twice the number who had changed their minds in 1964.

A second reason for a relatively large number of errors was the candidacy of George Wallace. As one might expect, given his candidacy, our rate of error in predicting the votes of residual partisans was high—almost 44 per cent; this figure is more than double the average figure for the four preceding elections. In predictions from attitudes, Wallace's candidacy made us prone to error both coming and going: Some 44 per cent of the respondents predicted to vote for him did not, and some 49 per cent of his vote came from respondents predicted to vote against him.

It seemed possible that a part of the difficulty in predicting votes for Wallace stemmed from the treatment of respondents' comments, pro and con, on the Democratic party. Statements favorable to the Democratic party were counted as favorable to Humphrey but not to Wallace; statements critical of the party were counted as unfavorable to Humphrey but not to Wallace. If many voters saw Wallace as a Democrat, this treatment of comments about the Democratic party was clearly the wrong one. As a test, we computed the error rate of predictions for Southern voters, those voters most likely to have linked Wallace to the Democrats. It was not very different from that for non-Southerners (19.1 as compared to 18.3). The ambiguity of Wallace's partisanship does not appear to have been an important source of trouble.

That voters' expectations regarding the outcome of the election did not figure in predictions derived from the Voter's Decision Rule does seem to have been a major source of error. One would expect neglect of expectations to be a source of error in a three-candidate race. In an election involving two candidates it makes no sense to vote for one's second choice because one expects one's first choice to lose; in a three-candidate election it may. In the event, we were decidedly more likely to be in error in predicting a vote for Wallace if the respondent thought Wallace would lose than if that was not the case. Of those re-

Table 10. Results in Predicting the Division of the Major Party Vote in Accordance with the Voter's Decision Rule, from Partisan Identifications and from Stated Intentions (1968 Election)

	Nixon	Humphrey	Wallace
Vote in Population (%)	43.4	42.7	13.5
Vote in Sample (%)	47.8	40.9	11.1
Vote Predicted From:			
The Voter's Decision Rule (%)	46.2	41.9	11.9
Stated Intentions (%)	48.9	38.6	12.5
Party Identification (%)	40.7	59.3	0.0

spondents predicted to vote for Wallace who thought he would win, about 75 per cent voted for him. Of those respondents predicted for Wallace who thought he would lose, only 47 per cent voted for him. Fear of wasting one's vote thus appears to have been a significant influence on voting in 1968.

Concluding Observations

If one's aim is to make the maximum number of correct predictions of voters' choices, the method we have used gives the best results we know of, with the possible exception of those associated with the very similar "decision rule model" proposed by Brody and Page.[24] Moreover, the results we have obtained by applying the Rule could be improved upon with only a minor adjustment of procedures and at no cost to the integrity of the theory of voting involved. Interviewing respondents at times closer to election day and adjusting samples to remove from consideration those who may have been stimulated to vote by interviewing should increase the accuracy of predictions. So should allowances for the strength and direction of short-term electoral forces, for the higher rates of error associated with predicting votes inconsistent with respondent's partisan identifications, and (in elections involving more than two candidates) for the expectations of respondents regarding the outcome of the election.

Our objective, however, has not been to make accurate predictions for their own sake; we have been interested in accurate prediction because it is one important test of explanatory value. Explanation of voters' choices in terms of the rule meets this test rather well. The respondents of five independent samples of the national electorate voted in the great majority *as if* they were applying the Rule. There were an appreciable number who did not vote that way and thus falsified our predictions, but some part of these errors may be reasonably assigned to causes unconnected to the use or nonuse of any decision rule by voters—specifically, to respondents' misreporting of their votes and to random inaccuracies in the data. More importantly, the greater part of our errors were of the sort one would expect, were voters applying the Rule. They occurred when, from the respondent's point of view, how to vote was a close question. They occurred also when time and circumstance were such that the respondent was likely, before he voted, to have been exposed to considerations he had not taken into account at the time he was interviewed.

How well does the explanation of voters' choices that we have proposed meet the other tests suggested in our introductory remarks? A case can be made that such explanation is spurious, because the attitudes to which we have applied the Rule are mere rationalizations of decisions to vote in a particular way and are thus without causal significance.

In rejoinder to this line of argument Stokes, Campbell and Miller make one point very well: Among the responses of voters on which we

> . . . attitudes toward the candidates and issues have sometimes been treated simply as rationalizations of preferences fixed by long-term party allegiances or by social characteristics such as ethnic or religious memberships, socio-economic status, etc. Both a person's standing party loyalties and his place in the social structure *do* have profound consequences for his voting behavior. . . . But it is quite clear that fixed party loyalties and sociological characteristics cannot account fully for the vote. In particular, neither of these factors, relatively inert through time, can account for the short-term fluctuations in the division of the vote which are of such great significance in a two-party system . . . the Republican proportion of the two-party vote increased between 1948 and 1952 among strong Republicans, weak Republicans, Independents, weak Democrats, and strong Democrats alike. Clearly neither social characteristics nor fixed party loyalties can account for the general movement toward the Republican standard between these two years. But the movement can be explained quite easily if we accord genuine motivational significance to attitudes toward the candidates and issues, and observe that the popular response to the Republican candidate and to the configuration of issues salient in the campaign was far more favorable to the Republicans in 1952 than in 1948.[25]

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[24] Working with data from postelection surveys of the 1968 election, Brody and Page attempted to predict how respondents had voted from their responses to a request to rate each of the presidential candidates on a scale ranging from 0 (very unfavorable) to 100 (very favorable). Predictions were made in terms of a "decision rule model," which postulated that "if a voter favors one candidate even slightly more than others, he will vote for that candidate" and (an amendment) "party identifications break ties." When this rule was applied to respondents' ratings of candidates, respondents' votes were correctly predicted in 90 per cent of the cases. These results are not directly comparable to ours, since our predictions were based on data taken from pre-election surveys. The conception of voting arrived at by Brody and Page, however, is obviously quite similar to our own. They have also suggested that their decision rules might be applied "to candidate evaluations in previous elections—computed from the S.R.C. open-ended items, lacking 'thermometer' ratings. . . ." That, of course, is very nearly what we have done. Brody and Page, "Indifference, Alienation and Rational Decisions."

[25] Donald E. Stokes, Angus Campbell, and Warren E. Miller, "Components of Electoral Decision," pp. 368–369. Emphasis in the original.

based our predictions were quite explicit statements by voters that their party loyalties or economic and other interests inclined them to vote in a particular way; without doing violence to the language, such statements can hardly be called "rationalizations" of "long-term party allegiances" or "social characteristics." It is of course impossible to prove that responses not describable in these terms were not rationalizations of *something,* but it is clear that the "something" is not what one might expect it to be—party loyalty or interests associated with the voter's position in the social and economic order.

Our own research suggests some additional points. First, about two-thirds of the respondents in the surveys with which we worked expressed attitudes that ran *counter* to their voting intentions or their eventual decisions or both. Normally, that is not what people do when giving good "reasons" for a decision made on other grounds or, to make the point in another way, it is not the sort of behavior to which the term, "rationalization," is usually applied.[26] Second, when respondents expressed conflicting attitudes, these were accompanied by the kind of behavior one would expect if such attitudes actually counted in the making of a decision: The more nearly equal were the net number of favorable attitudes toward candidates and parties expressed by a respondent, the greater was the likelihood that he was undecided, or that he cast his vote against the candidate that he had intended to vote for, or that he did not vote at all. If the attitudes expressed were without motivational import, would there be such a nice association as holds in fact between the respondent's report of his behavior and the balance of his comments, pro and con, about candidates and parties? Is it likely that respondents, with no forewarning of the questions they would be asked, could produce a pattern of fabricated "reasons" so consistent in detail with the preferences and behavior they report?

There is another line of argument that might lead one to dismiss explanation of voters' choices in terms of the Rule as spurious. The gist of this argument can be put as follows: The attitudes on which our predictions are based, aggregated for each voter as we have aggregated them, simply *measure* intentions to vote in a particular way. Because so much information is summarized in the measure, it is a good measure, giving a quite accurate indication of how a voter intends to vote. It is as if we dipped a thermometer into a solution several times and averaged the results, as opposed to taking but one reading.

We think that the appeal of this argument derives mainly from a confusing use of words. One can legitimately speak of a response to the question, "How do you intend to vote," as a "measure" of the respondent's true intention. One can also call the net scores derived from the Rule "measures" of the firmness and direction of intent, as we have at an earlier point in this paper. These data, however, are not "measures" in the same sense of the word. In the sense of the word "measure" as it is used in the second sentence of this paragraph, the responses which underlie our predictions measure, not voters' true intentions, but voters' true likes and dislikes of parties and candidates; these responses measure intentions only in the sense that they are highly reliable *indicators* of intentions. Why are they? Why do the answers to one set of questions, aggregated in a particular way, indicate so accurately the answers to a quite different question? Presumably, because a person's recital of his likes and dislikes regarding objects of choice will indicate what he intends to choose, *provided* that his likes and dislikes are actually motivating him, that the procedure he uses to translate his likes and dislikes into choices is known, and that he is not lying. If these three conditions hold, however, explanation of respondents' votes in terms of net scores derived from the Rule is not spurious and thus no substantial point is at issue.

Is the Voter's Decision Rule a believable account of the way voters weigh the considerations that figure in their choices of candidates? To our knowledge, there is no body of theory about voting or the making of decisions which in any strict sense would lead one to expect voters to apply the Rule in deciding how to vote. Still, the notion that they do squares quite well with certain general, and credible, ideas. It is reasonable to think that voting decisions follow the application of some kind o decision rule to a set of considerations and t suppose that most voters, faced with the sam kind of decision, may apply the same rule That, for instance, is essentially what Convers argues when he observes that "gross, simple group-oriented cues" will play a major rol in the voting decisions of the politically un-sophisticated majority of voters.[27] It is an intuitively acceptable notion also that a voter's decision rule should involve a comparison of the utilities to be derived from alternative acts of choice—or, to put it more simply, that it should involve a comparison of the decision-maker's likes and dislikes with regard to objects of choice. Finally, most voters do not appear to be consciously aware of the application of *any* decision rule in their choices of candidates (and in the making of many other sorts of decisions); it is therefore not remarkable that voters, if they apply the Rule, do so unconsciously.

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[26] That term can of course be construed differently. One may speak of a person as "rationalizing" if, when explaining or justifying an action, he cites only such *actual* motives as are socially acceptable—he tells the truth, but not the whole truth. To call the responses at issue here "rationalizations" in this sense, however, is to admit that they are expressions of genuine motivating attitudes.

What makes it most difficult to accept the Voter's Decision Rule as a description of something real is its *crudity,* its assignment of equal weight to all the considerations that may enter into any given voter's choices. This aspect of the Rule seems less unrealistic if we note two points: First, it was the respondents themselves who volunteered the likes and dislikes that we treated as considerations in their voting choices. It may be that they simply did not report likes or dislikes that did not attain to a certain threshold of importance. Second, it may be that the crude formula of the Rule is applied by voters only if and when choices of candidates seem *relatively inconsequential.* By "relatively inconsequential," we do not mean to suggest that voting decisions are not of great importance in the aggregate or that they are utterly trivial to most voters, individually. We do mean to suggest that few, if any, voters would regret nearly so keenly and persistently a choice at the polls that they perceived to have been mistaken as they would a wrong (to them) choice of jobs or colleges or automobiles. Two factors, at least, tend to trivialize voting in the eyes of voters: Whatever the voter does, it will almost certainly make no difference to the outcome of an election in any sizable constituency; and candidates—actually or in the view of the voter—frequently fail to take contrasting stands on issues of importance. Under these circumstances one would expect the voter to use some convenient, simple formula for deciding how to vote, one that would conserve his time and energy. The Rule is one possible formula—or mental "program" —that might serve this purpose.

The cautious reader will not find conclusive the case we have just made for regarding the Rule as the decision rule that most voters actually use. While we do not think it conclusive either, we do think that the case for that conclusion is good enough to warrant discussing some of its implications.

The view of voting suggested by our work has some immediate implications for those who interpret elections. The political historian wants to know what attitudes on the part of voters were decisive in particular elections. If the Rule correctly describes the manner in which most voters decide how to vote, he could in principle obtain this information from surveys taken close to election day. Historians and other analysts alike would benefit greatly from repeated surveys of opinion during the course of a campaign for, with the Rule as a guide to interpreting the data they yield, one should be able to show the impact of campaigning on the attitudes and perceptions of the electorate—and through these on voting—in great detail. Survey data could also give analysts indices that would allow them to differentiate elections in meaningful ways. For example, the median voter's vote will always be the last vote, or the last vote but one, that a winning candidate must necessarily receive. Thus, the net score of the median voter would be one sort of measure of how close a thing an election was in terms of the attitudes of marginal voters. Analysis of this kind could be greatly extended. Measures of the political sophistication of voters with net scores at or near the median should show with precision the roles in elections of the Independent of classical conception and of the ill-informed and indifferent voter. The median score of a candidate's supporters would give an indication of the strength and intensity of his support. And so on.

Whether the Rule is used by most voters has important implications for the practice of politics. It is important to be clear as to what these implications are. The research reported above gives political activists no better method of predicting *whether* voters will vote than they have now, though it perhaps points the way toward one. Neither does that research tell one how a fact or an issue comes to be a consideration in voting, nor how they can be made such. If voters do vote in accordance with the Voter's Decision Rule, however, candidates in principle should be able, with an accuracy heretofore not possible, to identify uncommitted or weakly committed voters and to find out what *specific* influences are bearing on their votes. This information would not surely yield an efficient campaign strategy, but it would be a better basis for developing one than any information candidates can now obtain.

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[27] Philip E. Converse, "The Nature of Belief Systems in Mass Publics," p. 249, in *Ideology and Discontent,* ed. David E. Apter (The Free Press: Glencoe, 1964).

Thoughtful people will not greet the prospect of more efficient campaign strategies without uneasiness. This is an understandable reaction but one that is hard to justify. In theory, if not always in practice, elections are designed to give weight to the opinions of the electorate in the choice of political leaders and in the conduct of governmental affairs. If the account of voting suggested by our research should turn out to be the correct one, the elective official should be able to learn more accurately than he now can, why the electorate prefers him or does not prefer him to a challenger or challengers; he could learn also what voters hope to accomplish by casting their votes for him. Given the goal of elections, if one accepts that goal, it would be against common sense to argue that officials should lack such information, for without it, decoding the "messages" that voters are urged to send must necessarily involve much room for error. The same kind of information, moreover, would be as useful to the statesman as to the politician, because it is necessary for informed assessment of the degree to which elections work as we might want them to work.

The correctness or incorrectness of our account of voting has consequences of importance for one's view of the American electorate. The evidence of a strong relationship between nonvoting and the net scores derived from the Rule supports the view of Schattschneider and others that nonvoting to a considerable extent reflects the nonvoter's indifference to the choices he perceives to be offered him in elections.[28] Our data suggest also that voters cluster in the greatest numbers around a "center" of sorts, as Scammon and Wattenberg would have it, and that these centrist voters have decided our recent presidential elections. It is not necessarily true, however, as Scammon and Wattenberg would have it also, that centrism is to be equated with any particular brand of public policy, now or in the recent past. Nor, perhaps, are the dark portraits of "swing" voters that emerge from some of the best studies of voting quite so true to life as many have thought. Our research has produced some evidence supporting Clausen's view that the interviewing done in connection with election surveys may stimulate some respondents to vote who would not vote otherwise. If interviewing has that result, and if we have identified correctly the voters affected, it follows that analyses of survey data unadjusted to take account of the phenomenon may exaggerate the electoral importance of voters who are ill-informed about public affairs and who pay little attention to them.

Students of politics occasionally despair of the "irrationality" of voters or treat it as a fact that the sophisticate must accept. Our results give no cause for either reaction. On the contrary, most voters appear to be conscious of the considerations they bring to voting, and the substance of such considerations seems often to involve highly appropriate concerns about public policy, the qualifications of candidates, and the records of political parties. It is true that many voters vote in ignorance of important matters, and the decision rule they apply may be a crude one. Neither of these things constitutes "irrationality." Both most probably reflect the meager incentives that our mass democracy offers to induce individual voters to expend time and energy in learning and thinking about the public business. If that is so, both conditions are in principle remediable.

Appendix

Predicting the Votes of Individual Voters in Accordance with the Voter's Decision Rule. Here we will describe in detail the procedure that we used in predicting respondents' votes on the basis of the Voter's Decision Rule. The easiest and clearest way to do so, perhaps, is to review the application of the Rule to the data, step by step.

(1) *The voter canvasses his likes and dislikes of the leading candidates and major parties involved in an election.*

Any respondent who said that he had voted and said how he had voted was counted as a voter. Thus, we treated as voters all respondents who said that they had voted for a major party candidate or for a minor party candidate, who said they had voted for a candidate who was not running (*e.g.*, Lyndon Johnson in 1968), and who said that they had voted for a party but did not name a candidate. We did not treat as voters those respondents who refused to say how they had voted or who said they did not remember how they had voted (except as indicated in Figures 1 and 2). The sample of voters in the survey of the 1960 election was a weighted sample; the others we worked with were not.

We regarded respondents' likes and dislikes of parties and candidates to be given by respondents' responses to the set of questions described in *f.n.* 4, as these responses were coded at the Survey Research Center. As has been noted already, up to five responses to each question were coded for each respondent; and, for any given respondent, only those responses that differed from each other in content were coded.

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[28] Cf Brody and Page.

(2) *Weighing each like and dislike equally, the voter votes for the candidate toward whom he has the greatest net number of favorable attitudes, if there is such a candidate.* (Yields "predictions from attitudes.")

In analyzing the two-candidate elections of 1952, 1956, 1960, and 1964 we determined the net number of favorable attitudes for the Democratic candidate by subtracting from the number of favorable attitudes toward the Democratic party and its candidate (D^+) the number of unfavorable attitudes toward the Democratic party and its candidate (D^-); the net number of favorable attitudes toward the Democratic candidate is ($D^+ - D^-$). Similarly, the net number of favorable attitudes toward the Republican candidate is ($R^+ - R^-$). The voter compares these two net numbers to make his decision. In the two-candidate elections, it is possible to compute a net score $[(D^+ - D^-) - (R^+ - R^-)]$; a positive net score means that the Rule yields a decision to vote for the Democratic candidate, a negative net score yields a decision for the Republican, and a zero score yields no "prediction from attitudes."

Those familiar with the work of Campbell and his associates will note that the method we used to compute net scores in these two-candidate elections is like that used in *The American Voter* to arrive at a respondent's scores on particular "components of electoral decision." In the words of Stokes, Campbell, and Miller, "In scoring a person's attitude toward a given object [of national politics] we have merely subtracted the number of his pro-Republican or anti-Democratic references to the object from the number of his anti-Republican or pro-Democratic references" (p. 370). This procedure—in our terms, $(D^+ + R^-) - (R^+ + D^-)$—is equivalent to $(D^+ - D^-) - (R^+ - R^-)$.

In predicting respondents' votes in the election of 1968, we counted, and then classified, respondents' likes and dislikes of parties and candidates as D^+ (favorable to Hubert Humphrey); D^- (unfavorable to Humphrey); R^+ (favorable to Richard Nixon); R^- (unfavorable to Nixon); W^+ (favorable to George Wallace); and W^- (unfavorable to Wallace). For any given respondent we took the net number of favorable attitudes toward each of the candidates to be given by ($D^+ - D^-$) for Humphrey; ($R^+ - R^-$) for Nixon, and ($W^+ - W^-$) for Wallace. The respondent was predicted to vote for the candidate toward whom he had the greatest net number of favorable attitudes. If there were no such candidate, no prediction from attitudes was made. In the case of elections with more than two candidates, the concept of "net score" is ambiguous and we have not had occasion to use it.

(3) *If no candidate has such an advantage, the voter votes consistently with his party affiliation, if he has such.* (Yields "Predictions from residual partisanship.")

In the surveys with which we worked, we treated all respondents classified as "strong Democats," weak Democrats," or "independent Democrats" as affiliated with the Democratic Party. All respondents classified as "strong Republicans," "weak Republicans," or "independent Republicans" we treated as having an affiliation with the Republican Party.

(4) *If his attitudes do not incline him toward one candidate more than toward another and if he does not identify with one of the major parties, the voter reaches a null decision.* (Yields "null decisions.")

Respondents may have given no advantage to a candidate on the basis of attitudes if there was no candidate with the greatest net number of favorable attitudes or if the respondents refused to cite likes and dislikes of parties and candidates in the survey. Respondents may not have identified with one of the major parties if they were independents leaning toward neither party or if they refused to answer questions about their party identification. In all cases in which predictions could be made neither on the basis of attitudes nor on the basis of residual partisanship, the Rule yields a null decision.

Racial Differences in Protest Participation*

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Introduction

During the years of the past decade an extraordinarily wide variety of Americans have chosen at one time or another to take their politics into the streets. The diversity of those who have employed protest tactics is striking: urban and rural blacks, college and high school students, farmers, veterans, white homeowners, the parents of school children, the peace constituency, so-called hard hats or construction workers, religious groups, ethnic minorities, and conservative organizations have all recently engaged in widely publicized protests of one form or another.[1]

While scholarship on protest is beginning to accumulate, a problem of limited perspective continues to inhibit our understanding of the phenomenon. This perspective, constrained both by lack of data and by inadequate conceptual development, has given rise to the view that protest is essentially extraordinary in character[2] and that those who use it must do so because they lack the resources to employ more conventional means of bringing demands to bear on the political system. The purpose of this article is to challenge this view and to begin to restate some of the generalizations built upon it. In particular, I would contend that if we examine the attitudes toward protest, the social characteristics of protesters, and the uses and organization of protest among blacks and whites separately, we find racial variations important enough to suggest substantial modifications in the standard interpretation of the role and nature of this form of political expression. Protest among blacks was found in the present research to be carried out primarily by members of the adult middle and working classes and to represent an integral and normal feature of the black adaptation to urban politics in particular. Protest among whites, in contrast, is an extraordinary form of participation at all levels of politics for a small, predominantly upper middle-class segment of the white community. One of the chief ironies of this more complex view of the role of protest in domestic politics, as we shall see, is that the routine quality of protest and its institutionalization in the black community—a strength in the short run—are eventually bound to strip the technique of much of its force as a means for blacks of making political demands. In pursuing this analysis, we will find that the data challenge and clarify some assumptions about the relationship of participation in protest to the possession of social resources.

Data and Research Setting

The data on which this analysis is based are drawn from a 1970 survey of Milwaukee residents age 18 and over. The study design was constructed to yield two separate probability samples, one each of the black and white populations as close in size as possible. The final sample numbered 331 whites and 246 blacks.[3] Comparing 1970 Census data from Milwaukee with the survey sample indicates that with a few minor exceptions the fit of the sample with its universe is high. Sample distributions along the sex, occupation, education, and income dimensions closely match the corresponding census distributions, except that low education (less than high school) blacks and whites and high income (over $10,000) whites

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[1] Jerome Skolnick, *The Politics of Protest* (New York: Ballantine, 1969) p. 22.

[2] Lester Milbrath has written, for example, that protest demonstrations are "by definition, extraordinary rather than normal." *Political Participation* (Chicago: Rand McNally, 1965), p. 27. And Michael Lewis argues that one of the common defining threads which characterize various sorts of demonstrative political manifestations is their proclivity to go "beyond the constraints of the institutionalized political process." "The Negro Protest in Urban America (1968)," in *Protest, Reform and Revolt*, ed. Joseph Gusfield, (New York: John Wiley, 1970), p. 151.

[3] The black population of Milwaukee numbered 105,000 in 1970, or 14.7 per cent of the total. This proportion of blacks is not especially small for cities of this size. Milwaukee falls among the second ten largest cities in the country. The average percentage of blacks in these cities is 20.7. The percentages of blacks in Boston (16.3), San Francisco (13.4), and Indianapolis (18.0), all of which cities fall in the second ten largest group, compare most favorably with Milwaukee.

are slightly undersampled. Interviewers were all professionals, and they were matched with their respondents by race.

Since cross-sectional surveys in single cities suffer the obvious disadvantages of limited time and geographical perspectives, it is necessary both to warn the reader of the tentative nature of the findings and to make explicit at least one relevant characteristic of the research setting, i.e., that among cities of its general size Milwaukee has a particularly rich recent history of political protest and violence, especially among blacks. Among the riots in the summer of 1967, for example, the Milwaukee riot was considered by the Kerner Commission to be among the three most severe in the nation.[4] The city appears to occupy a similarly high position in regard to the level of protest activity: in one study of protest in 43 large American cities over a six-month period in 1968, Milwaukee experienced more protest incidents than any other city.[5] Milwaukee's most notable protest campaign occurred in 1967 and involved six months of daily street marches and rallies led by Father James Groppi to secure open housing laws. Although these marches drew predominantly from the black community, a large number of whites took part sporadically. Other major black protests included a school boycott (1965) by an organization called Milwaukee United School Integration Committee (MUSIC) to protest racial imbalance in the inner city schools. Various private institutions have also been targets of prolonged protest: the Eagles Club, a major supermarket chain, and the largest manufacturing employer in the city, Allis-Chalmers, have been picketed and boycotted by black organizations to protest various discriminatory racial practices. Finally, during 1969, the National Welfare Rights Organization became active in Milwaukee and helped to sponsor a number of demonstrations and sit-ins by welfare recipients and their sympathizers.

This high level of political intensity and militance may have increased the extent and diffusion of individual protest participation in both the black and the white communities to such an extent that patterns reported here are atypical of what we might find in other parts of the country. The rare and scattered bits of comparable evidence from other surveys cited in the text, however, generally support the Milwaukee findings. One presumed advantage of the frequency and scope of protest in Milwaukee is that it virtually ensures a significant pool of protest participants for analysis which random sampling techniques can be expected to tap.

Attitudes Toward Protest as a Political Tactic

Protest may be defined as a form of collective expression, disruptive in nature, designed to provide its users both with access to decision makers and with bargaining leverage in negotiations with them.[6] While protest relies for its dynamics on a delicate balance of threat and moral appeal,[7] it falls short of and may be distinguished from outright political violence.[8]

Most surveys which have sought to tap attitudes toward protest have revealed predominantly hostile sentiments, leading to the view that protest is a form of political nonconformity. For example, a national Harris poll conducted in 1965 showed that 68 per cent believed that antiwar picketing and demonstrations were more harmful than helpful; 68 per cent believed that civil rights demonstrations were likewise more harmful than helpful; and 65 per cent felt the same way about student demonstrations.[9]

In 1966 the Harris poll asked whites whether, if they were in the same position as Negroes, they would feel justified "to march and protest in demonstrations." Exactly half the sample said they would not feel justified; 35 per cent said they would.[10] A later Harris poll, taken in 1968, found that a full 82 per cent of their respondents "disapproved" of Negro "demonstration tactics."[11]

In a smaller survey conducted in Ann Arbor, Michigan, Marvin Olsen found that disapproval of protest was not only relatively high but that it focused particularly on mass demonstrations and sit-ins. Sixty per cent of his sample, most of which was above average in education, expressed strong negative opinions regarding such tactics.[12] Angus Campbell, summarizing findings based on the 15 city Michigan Survey Research Center study, writes that:

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[4] *Report of the National Advisory Commission on Civil Disorders* (Washington, D.C.: Government Printing Office, 1968), pp. 158–159.

[5] Peter K. Eisinger, "The Conditions of Protest Behavior in American Cities," *The American Political Science Review*, 67 (March 1973), 11–28.

[6] James Q. Wilson was one of the first to characterize protest as a bargaining process in these terms. "The Strategy of Protest: Problems of Negro Civic Action," *Journal of Conflict Resolution*, 3 (September 1961), 291–303.

[7] See the discussion of this point in Ralph Turner, "The Public Perception of Protest," *American Sociological Review*, 34 (December 1969), 815–831, at p. 820.

[8] Eisinger, pp. 13–14; see also H. L. Nieburg, "The Threat of Violence and Social Change," *American Political Science Review*, 56 (December 1962), 865–873, at p. 872.

[9] These data are cited in Amitai Etzioni, *Demonstration Democracy* (New York: Gordon and Breach, 1970), p. 10

[10] William Brink and Louis Harris, *Black and White: A Study of Racial Attitudes Today* (New York: Simon and Schuster, 1967), p. 222.

[11] Etzioni, p. 10.

> White response to the black protest movement . . . was generally unfavorable, a majority believing it to be pushing too fast and too violently and with hurtful consequences . . . Black evaluation of the black protest was far more positive than that of whites. . . .[13]

As instruments for assessing attitudes toward protest, these surveys are suggestive, but they have two major shortcomings. One is that, with the exception of Campbell, investigators generally have failed to introduce controls for race in their reports or their study design. Either marginal totals are presented for racially undifferentiated samples, or the surveys themselves have been conducted only among whites. A second shortcoming involves the phrasing of questions: Respondents were asked about their feelings toward *antiwar* picketing, *civil rights* demonstrations, *student* protest, and the *black* protest movement. Thus, the resulting negative attitudes may simultaneously reflect hostility toward any or all of the following: toward a particular mode of political expression (protest), toward presumed positions on certain broad substantive issues (civil rights, the war in Asia), and toward certain distinct protesting groups in society (blacks, students).

The advantage of the Milwaukee survey is that it asks both blacks and whites questions about protest itself, without reference to the issues which might give rise to protest or the groups which use it, although, to be sure, we still cannot be absolutely certain that respondents did not supply their own substantive associations with protest in answering the questions. Sharp racial differences emerge in the assessment of protest as a tactic. Favorable attitudes toward protest are widespread in the black community, and these feelings transcend the boundary dividing those who have actually taken part in protest from those who have not.[14] Black protesters come to participate in demonstrative politics in an aura of subcommunity approval and confidence. But while the attitudes of the black subcommunity reinforce, if not stimulate, individual decisions to engage in protest, those of the white community are largely antipathetic to such participation.

Table 1. Explanations of the Reasons for Protest

	Black	White
Instrumental Reasons To gain attention; to win demands	56% (138)	36% (119)
Expressive Reasons Because they are angry; to express outrage	20% (49)	15% (48)
Negative Reasons Imitation; trouble-making	8% (20)	38% (123)
Other	1% (2)	1% (2)
DK	15% (37)	12% (39)
	100% (246)	102% (331)

Differences significant at .001 level (Chi-square =63.503).

A majority of the blacks in the sample are convinced that the motivations which lie behind protest participation are largely instrumental in nature. When respondents were asked in the survey why they supposed that "people protest and demonstrate so much these days," 56 per cent of the blacks explained that protest was essentially a device to gain certain ends. People were seen to protest in order to gain attention or access to decision makers or they protest in order to gain substantive goals. (Table 1). Only eight per cent attributed pro-

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[12] Marvin Olsen, "Perceived Legitimacy of Social Protest Actions," *Social Problems*, 15 (Winter, 1968), p. 299. Other sources of data showing similar attitudes toward protest may be found in Skolnick, p. 22–23; and Hazel Erskine, "The Polls: Demonstrations and Race Riots," *Public Opinion Quarterly*, 31 (Winter 1967–1968), 655–677.

[13] Angus Campbell, *White Attitudes Toward Black People* (Ann Arbor, Michigan: Institute for Social Research, University of Michigan, 1971), p. 139.

[14] This finding seems consistent with the observations concerning widespread black support or at least sympathy for black rioters. Matthew Holden has written in this regard that:

> in addition to people accustomed to violating the law or being in trouble with the police—whose participation in riots could be easily predicted—there was a substantial reservoir of more respectable black people who were at least *sentimentally* friendly to violence. They did not perceive how many black people—hard-working, tax-paying, and responsible—would permit themselves to 'get caught up in the situation,' let alone how many more would say 'I wouldn't do it myself, but I can understand those who do.'

Politics of the Black "Nation" (New York: Chandler, 1973), p. 77. Empirical evidence to this effect surfaced first in the Watts riot studies: David O. Sears and T. M. Tomlinson found in their study of attitudes after that riot that the major opinion cleavage in interpreting the disturbances was between the races, not between black rioters and black nonrioters. "Riot Ideology in Los Angeles: A Study of Negro Attitudes," *Social Science Quarterly*, 49 (December 1968), p. 485–503.

Table 2. Percentage Which Would Like to See More, Fewer, or No Demonstrations, Controlling for Protest Participation

	Black			White		
	Protesters	Nonprotesters	Total	Protesters	Nonprotesters	Total
More	71% (37)	36% (69)	43% (106)	26% (9)	5% (14)	7% (23)
Fewer	13% (7)	20% (38)	18% (45)	40% (14)	53% (157)	52% (171)
None	4% (2)	24% (46)	20% (48)	6% (2)	27% (80)	25% (82)
Doesn't matter	10% (5)	21% (40)	18% (45)	11% (4)	14% (40)	13% (44)
DK, NA	2% (1)	* (1)	1% (2)	17% (6)	2% (5)	3% (11)
	100% (52)	101% (194)	100% (246)	100% (35)	101% (296)	100% (331)

Differences between black protesters and white protesters are significant at the .001 level (Chi-square = 19.044). Differences between black nonprotesters and white nonprotesters are significant at the .001 level (Chi-square = 92.062).

test participation to negative motives such as imitation ("It's the thing to do") or meaningless trouble-making. By contrast, 38 per cent of the white sample interpreted protest negatively as trouble-making or imitation, while 36 per cent saw it as an instrumental tactic. To explain protest participation in terms of instrumental motivations is not necessarily to approve it, of course, but it is to concede that some people find it genuinely useful for serious purposes. Most blacks do not question the integrity of the motives of protest participants, but a substantial number of members of the white community apparently do.

Not only do most blacks believe that protest is a useful political tool, but a significant proportion of them would like to see it used more often. Forty-three per cent of the black sample would like to see more demonstrations rather than fewer or none, compared to only seven per cent of the white respondents. As might be expected, black protesters contribute disproportionately to the black group which would like to see more demonstrations, *but the percentage of black nonprotesters*[15] *who say they would like to see more is larger than that of white protesters who hold the same view* (Table 2).

In short, support for protest is greater in that segment of the black community which has never taken part in protest than it is even among white protesters.

Among protesters themselves, blacks were much more confident than whites about the impact of their particular efforts. The protest participants were asked whether each of the protests in which they had taken part had helped "to get what you wanted." In 53 per cent of the instances of participation, black protesters answered in the affirmative, compared to 38 per cent among whites.

Black protesters receive other supportive cues from the black community, but whites engage in protest in the face of strong community opposition. For example, the figures in Table 3 show that a high number of blacks believe that demonstrating is *better* than voting in Milwaukee as an instrumental device. (Forty-one of the 59 blacks who agree with this proposition are *nonprotesters.*) The overwhelming proportion of the white sample expresses an unambivalently negative opinion concerning the primacy of protest. Even the white protesters themselves disagree that demonstrations are "better" than voting.[16]

Blacks also regard protest in general as an *effective* device to gain the attention of government. Protesters and nonprotesters scarcely differ on this score. More whites, however, doubt the efficacy of protest.

Finally, blacks appear to subscribe to an attitude similar to the notion of citizen or civic duty which scholars have developed in relation to voting. We might call this attitude "protest duty." A sizable majority of whites are either ambivalent about or opposed to the necessity of protest participation, but most blacks believe it is important to take part.

In summary, the attitudes of the black subcommunity, in contradistinction to those of the larger white community, support protest participation and foster expectations of its effec-

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[15] As I explain in greater detail later on, protesters are those who answered affirmatively when asked in the interview whether they had ever taken part in a sit-in, demonstration, mass march, or other type of protest action. The category "protester" does not include those who merely engaged in union picketing on strike.

[16] Twenty-two of the 35 white protesters disagreed with the statement; only three agreed.

Table 3. Attitudes Toward Protest

		Black	White
Demonstrations are better than voting in this city because demonstrations are about the only way to get your point across.	Agree	24% (59)	4% (14)
	Agree & Disagree	22% (54)	11% (38)
	Disagree	47% (115)	82% (270)
	DK	7% (18)	3% (9)
		p<.001 (Chi-square 79.201)	
Demonstrations and mass marches are one good way to get the city government to listen to you.	Agree	69% (169)	23% (75)
	Agree & Disagree	19% (47)	20% (66)
	Disagree	9% (23)	54% (179)
	DK	3% (7)	3% (11)
		p<.001 (Chi-square 157.320)	
It's sometimes important to take part in demonstrations because that's one way to make your voice heard.	Agree	73% (179)	29% (97)
	Agree & Disagree	13% (31)	19% (63)
	Disagree	10% (24)	47% (153)
	DK	5% (12)	5% (16)
		p<.001 (Chi-square 120.381)	

tiveness. Favorable attitudes toward protest are not held by a splinter minority, but seem widespread among blacks, both those who take part in demonstrative politics and those who do not. Previous surveys, then, have given an incomplete impression. While protest may constitute an act of political nonconformity in the society at large, it enjoys major and widespread support as an acceptable device for political self-expression among blacks.

Social Status and Protest Participation

Efforts to generalize about the relationship between social status and protest participation have produced a forest of ambiguities. Part of the problem may be attributed to inadequate data, but the more important source of confusion stems from inconsistent, vague, and occasionally inaccurate conceptualizations of the roots of protest.

Most empirical efforts to explore the social correlates of protest participation have been based on surveys of college students. Samples have invariably been racially homogeneous or so nearly uniform that interracial comparisons cannot be made with confidence. Drawing entirely on the educated young, these selective samples ensure that even the most tentative generalizations must be extremely limited in scope. The difficulties inherent in these studies are compounded by the contradictory conclusions which are beginning to emerge. Early studies of the southern civil rights movement and of campus protest indicated that protest participants among both black and white students were more likely to come from middle- rather than lower-class families, a finding which suggested that protest participation, like voting, is likely to be associated with the individual possession of social resources.[17] One of the most recent studies of campus political activists, however, concludes that protesters are in fact sociologically no different from nonparticipants and conventional activists.[18]

The Milwaukee data have the advantage of being drawn from the entire adult populations of both racial communities, which therefore renders them somewhat more reliable bases than student samples for the construction of general propositions about protest participation.

The major ambiguities in the discussion of the relationship between protest participation and social status, however, derive not from the empirical works cited here but rather are found in the more influential theoretical literature. In speaking of the sources of protest, several writers view the tactic as a technique of the "powerless" or the "relatively powerless."[19]

[17] On the social backgrounds of white student protesters see David Westby and Richard Braungart, "Class and Politics in the Family Backgrounds of Student Political Activists," *American Sociological Review*, 31 (October 1966), 690–692. Seymour Martin Lipset offers a thorough summary of the literature which arrives at these findings in "The Activists: A Profile," in *Confrontation*, ed. Daniel Bell and Irving Kristol (New York: Basic Books, 1969), pp. 45–57. On black student protest participation see Donald R. Matthews and James W. Prothro, *Negroes and the New Southern Politics* (New York: Harcourt, Brace and World, 1966), p. 418; and John M. Orbell, "Protest Participation Among Southern Negro College Students," *American Political Science Review*, 61 (June, 1967) 446–456, at 450.

[18] James W. Clarke and Joseph Egan, "Social and Political Dimensions of Protest Activity," *Journal of Politics*, 34 (May 1972), 500–523.

[19] Wilson uses the term "powerless." Michael Lipsky speaks of the "relatively powerless." Lipsky, "Protest as a Political Resource," *American Political Science Review*, 62 (December 1968), p. 1144.

Powerlessness is conceived in a generally narrow sense as a function of low social status, a condition characterized largely by the absence of middle-class resources. For Wilson, blacks are a prime example of a powerless group because they lack, among other things, the resources which come with class status.[20] Lipsy characterizes "relatively powerless groups as those lacking in conventional political resources, namely those various currencies of political exchange which accompany high position in the social order.[21]

The problem here is that the Milwaukee data show that *individuals* who participate in protest activity in fact possess the social attributes of middle-class status. These findings, apparently at odds with the blanket conceptualization of protest as a tactic primarily of those low in the possession of social status resources,[22] suggests the need for some attempts at reconciliation and reformulation.

It is imperative, first of all, to consider the sort of powerlessness which gives rise to protest behavior on the part of individuals as a multidimensional phenomenon, one that may include other characteristics and conditions besides low social status. Second, it is necessary to make clear that, so long as we do conceive powerlessness in social status terms, among others, such powerlessness may refer to the relative position in society of the *group* or *collectivity* from which the protesters are drawn without necessarily implying that the individuals who actually participate in protest are also low in status.

Let us turn to the data. The extent of protest participation in Milwaukee is just high enough to enable us to discern patterns with some measure of confidence. Among blacks, 21 per cent ($N = 52$) claimed to have taken part in at least one protest, while 11 per cent of the whites ($N = 35$) said they had done so. Protest participation was determined by answers to a series of questions inquiring whether the respondent had ever taken part in a mass march, a demonstration, a sit-in, a protest rally or protest meeting, or any other form of protest or direct action. The variety of terms employed to refer to protest is of little importance for analytical purposes. Rather, it simply reflects the numerous conventional ways of characterizing protest in current usage.

Each respondent was asked how many of each type of protest he had participated in. Then he was asked a variety of questions concerning the details only of the three most recent of each type of incident.

Black respondents reported a total of 226 instances of participation, of which we have some details on 149. Whites reported 153 instances, of which they described 97. (It is important to note that in the later discussion on patterns in the use of protest the unit of analysis is often an instance of participation, not a separate protest incident. Sometimes instances of participation by different individuals occurred in the same protest incident.) While more blacks, proportionally, took part in protest, the rate of participation by individual protesters did not differ much by race: white protesters averaged 4.5 instances of participation, while blacks averaged 4.3.

In order to examine the relationship between protest participation and status, respondents were first divided within each racial sample into groups of those who had participated in protest and those who had not.[23] Protesters ranked

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[20] Wilson, "Strategy of Protest," p. 292.

[21] Lipsky, p. 1144. Lipsky does not enumerate those resources but relies on Robert Dahl's list of resources in "The Analysis of Influence in Local Communities," in *Social Science and Community Action*, ed. Charles R. Adrian (East Lansing, Michigan: Institute for Community Development and Resources, 1960), p. 32.

[22] Wilson has written, for example, that "Protest actions involving such tactics as mass meetings, picketing, boycotts, and strikes rarely find enthusiastic participants among upper-income and higher status individuals." "Strategy of Protest," p. 206. Etzioni has argued that it is the "underclasses," socioeconomically speaking, which "have a special affinity for protest." *Demonstration Democracy*, p. 6. That blacks as a group, regardless of the objective social status of any given black individual, may comprise a racial "underclass" is a possibility I shall consider later.

[23] Protest participation was initially treated both as a dichotomous dummy variable (protest/no protest) and as a continuous variable (frequency of protest participation, ranging from 0 to 10 or more). Fifty-six of the 87 protesters had taken part in more than one protest. The relationships between protest participation, treated as a dummy variable, and socioeconomic and demographic indicators are, for the moment, more important, since the object of the analysis is to distinguish those who take part in such activity from those who never do. The relationships are uniformly slightly stronger than when protest is treated as a continuous variable. Pearson's r coefficients are compared in the table below when the protest variable is treated differently.

Table A. SES and Protest Participation as a Dichotomous and a Continuous Variable

	Black		White	
	Protest/ no protest (dichotomous)	Frequency of protest (continuous)	Protest/ no protest (dichotomous)	Frequency of protest (continuous)
Age	−.27	−.20	−.20	−.12
Income	.06	.08	.10	.01
Education	.37	.25	.43	.35
No. of Organizational memberships	.24	.16	.19	.05

Table 4. Socioeconomic Differences Between Protesters and Nonprotesters by Race*

	Black			White		
	Protesters	Nonprotesters	Entire Black Sample	Protesters	Nonprotesters	Entire White Sample
Mean Income	$6790 (48)	$6300 (172)	$6400 (220)	$8320 (34)	$7310 (270)	$7400 (304)
Mean No. of Group Memberships	1.90 (52)	1.02 (194)	1.21 (246)	2.14 (35)	1.28 (296)	1.37 (331)
Mean Education	11.96 Yrs. (52)	9.89 Yrs. (193)	10.3 Yrs. (245)	14.57 Yrs. (35)	10.96 Yrs. (294)	11.04 Yrs. (329)

* Differences between protester and nonprotester means for both races are significant at .001 level using a two-tailed difference of means test (t) except for difference in black income means (not significant) and white income means ($p<.10$).

higher than nonprotesters in terms of income, education, and group memberships,[24] as we see in the figures in Table 4.

Protesters also fall disproportionately in the higher occupational categories. For both races, protest and high occupation are positively related. The gamma coefficient for blacks is .38, for whites .41.[25] Eighteen per cent of the black protesters are professionals or businessmen compared to seven per cent of the nonprotesters. In the white sample 37 per cent of the protesters are professionals (none fall in the business category) compared to 12 per cent of the nonprotesters who fall in either the professional or business classifications. Among whites, skilled and semi-skilled workers are underrepresented among the protesters, but among blacks they are nearly perfectly represented. Twenty-nine per cent of the black protesters fell in these categories compared to 30 per cent in the entire sample. Only 15 per cent of the white protesters fell here compared to 30 per cent of the entire white sample.

By all standard indicators of socioeconomic status, protesters are better-off than nonprotesters within each racial group. If these data reflect accurately more universal patterns of the correlates of protest behavior, then any implication that protest is primarily a tactic of poor *individuals* is clearly wrong.

While both black and white protesters stand above the average status indicators of their respective racial samples, it would appear that the black protesters *do not differ as much* from the black population as a whole as white protesters differ from the white population. In this sense it seems possible to argue that black protesters occupy a slightly different social location in their racial community than white protesters do in theirs.

White protesters possess all the trappings of the upper middle class. They not only tend predominantly to be professionals (the second largest occupational category for whites is housewives, however), but they are the only group with a mean education which falls in the post high school years. They have nearly four more years of education than white nonprotesters and they earn substantially more money. By these characteristics alone, they stand apart from the bulk of the white population. They are unrepresentative of the mass, a minority by virtue not only of their participation in protest but also of their status.

The black protesters are not strictly representative, socioeconomically, of the black mass population, for they too are better-off, but they seem more than their white counterparts to resemble the norm of the community out of which they come. That is to say, simply, that the gaps in education, income, and group memberships between black protesters and nonprotesters are smaller than those between white protesters and nonprotesters.

One must be cautious in dealing with this difference, for it is to some degree an artifact of statistical constraints. The *range* in the white income and education data is simply greater than it is in the black data.[26] Nevertheless, it would be ill-advised to dismiss the difference altogether. Consider the data on education. White protesters *average* two and a half years of college; white nonprotesters do not quite average completion of high school. Both black protesters and nonprotesters, however, average less than a high school education. The difference between high school and even some college education makes a profound difference in psychology, outlook, earning potential, and opportunity. Simply by entering college, an individual differentiates himself from his fellows in the community who do not go to college in a way far more significant than the year or two which this adds to his educational level would indicate. The simple number of years of education, in other words, does not always represent a perfect additive interval measure of all the things for which education stands, especially when the step upward between high school and college is considered. In short, the black protester with his high school education is more like the average black individual than the white protester with his college years is like the average white individual.

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[24] Robert Alford and Harry Scoble offer a discussion of organizational membership as a political resource in "Sources of Local Political Involvement," *American Political Science Review*, 62 (December 1968), 1192–1206.

[25] Goodman and Kruskal's gamma is a nonparametric measure of association for ordinal grouped data. Occupational categories, based on the U.S. Census classification, are ordinally ranked. For a discussion of gamma, see William L. Hays, *Statistics for Psychologists* (New York: Holt, Rinehart and Winston, 1963), p. 655.

[26] The standard deviation for black income is 87, for white income 166. For black education the standard deviation is 99, for white 183. This simply indi-

Two other minor points here support the argument that black protesters are more typical of their racial community than white protesters are of theirs. One is that the income difference between black protesters and nonprotesters is not statistically significant, as we saw in Table 4, while the difference in white incomes is significant at a moderate level. We may be relatively sure, then, that the white protesters do differ on this dimension from the nonprotesters, but we cannot make the same assertion about blacks with confidence.

Another point involves an examination of the correlation coefficients derived from running the frequency of protest *for protesters only* against the status indicators. (That is, protest is treated as a continuous variable, but since we have eliminated nonprotesters from the calculations, the range of protest frequency now runs from one to 10 or more.) It will be recalled that protest and high status are positively related to some degree for both races, whether protest is treated as a dichotomous or a continuous variable. When nonprotesters are eliminated, however, we discover that for black protesters the frequency with which an individual takes part in protest *decreases* as status indicators increase. But for whites protest frequency and status vary together positively.

Table 5. Correlations (Pearson's *r*) Between Frequency of Protest and Status Indicators for Protesters Only

	Black	White
Income	−.14	.14
Education	−.12	.08
Number of organizational memberships	−.13	.23

These coefficients are, of course, extremely small, but they do at least hint that once an individual makes the decision to protest, his subsequent patterns of protest participation will differ depending upon his race. The more whites protest, the more likely it is that they are socioeconomically differentiated from the norm, i.e., from the mean scores on the various status indicators. Black protest is most frequent, however, among those who stand just above the socioeconomic norm. As blacks advance up the status hierarchy, becoming increasingly differentiated from the black socioeconomic average, they are less likely to protest with great frequency. Thus, if frequency of protest can be thought to indicate the degree of commitment to protest or seriousness of intent, then the most committed and serious black protesters are more likely than occasional black protesters to resemble socioeconomically their fellows in the community who do not protest. To this extent at least, protest attracts a more "normal" constituency among blacks than it does among whites.

Other data suggest that black protesters not only resemble the socioeconomic norm in the black community but that they are also drawn disproportionately from its more integrated, stable elements. Black protesters are more likely than black nonprotesters to own or to be in the process of buying their homes. Among whites, however, the pattern is reversed: protesters are more likely than nonprotesters to be renters (Table 6).

Black protesters and nonprotesters also tend to resemble one another in terms of the average length of time they have lived in the city. The average for the former is 13.6 years, for the latter, 15 years. White protesters have lived in the city an average of 11.4 years compared to the white nonprotester average of 24.2 years.[27] While these figures are to some

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cates that the dispersion around the mean for blacks is lower, signifying a greater probability that black protesters' means will fall closer to the sample mean.

[27] Black protesters have lived in their neighborhoods an average of 5.4 years compared to 4.5 for black non-protesters. For whites the figures are 6.5 and 11.3 respectively.

Table 6. Home Ownership and Protest Participation

	Black			White		
	Protesters	Nonprotesters	Sample	Protesters	Nonprotesters	Sample
Own	12% (6)	8% (16)	9% (22)	20% (7)	32% (96)	31% (103)
Buying	35% (18)	24% (46)	26% (64)	17% (6)	17% (49)	17% (55)
Rent	52% (27)	68% (131)	64% (158)	57% (20)	49% (144)	50% (164)
Other	2% (1)	* (1)	1% (2)	6% (2)	2% (6)	2% (8)
	100% (52)	100% (194)	100% (246)	100% (35)	100% (295)	100% (330)

degree a function of age,[28] they nevertheless add to the evidence that black protesters seem more centrally located in or integrated members of their racial community than white protesters do of theirs.

The discovery that individual protest participation cannot be explained by the absence on the part of the protesters themselves of conventional resources does not necessarily require that we abandon the notion that protest is a political tool of the powerless, *as long as we understand powerlessness as a function of other factors besides low status.*

Powerlessness may, for example, be related not so much to individual poverty as to lack of group legitimacy. To lack legitimacy is to be distrusted, ignored, or dismissed by those in power. Legitimacy implies that decision makers will accede willingly to petitions for a hearing and will take seriously substantive demands. Middle-class people have traditionally had legitimacy in American politics, but status is no guarantee of legitimacy. Race, age, and ideology are other, often more important, factors which enter into a community's judgment about the legitimacy of citizen political groups. When legitimacy is withheld, members of penalized groups are not readily admitted to positions of power in political parties or electoral organizations; they are not appointed to administrative posts; their advice is not actively or routinely sought by decision makers. Since protest is a means of gaining access to councils of power by groups which have been denied legitimacy,[29] protest may be conceived as a strategy employed by those who are relatively powerless in terms of the legitimacy they command.

The resort to protest may also be a signal of impatience regardless of whether or not a group has status *or* legitimacy. For the impatient, protest is a particularly useful interim tool between periods of electoral activity designed to force consideration of certain controversial issues by an often unwieldy political system. But even in polities in which such issues are raised and discussed, action may be deemed slow or inadequate. Groups which do not control the governmental apparatus capable of hastening consideration or fulfilling of demands are likely to feel impatient and powerless. It goes without saying that blacks fall into this category. From this perspective, it is not difficult to understand why blacks have persisted in their use of protest despite the fact that the political system has at times finally been responsive. To be impatient about the *pace* of change and at the same time unable to affect that pace with a high degree of certainty is also to be powerless.

Finally, we may retain the original equation of powerlessness with the absence of conventional resources as a source of protest in one sense: that is that protest may occur *on behalf of those groups which are disadvantaged socioeconomically.* Protest in this particular context, then, is a means by which certain better-off members of the community can *represent* their disadvantaged fellows or their disadvantaged group in the political arena and dramatize their or its concerns.

To summarize, we have seen that both black and white protesters possess more resources than those who do not take part in protest. In this respect the protesters of the two races resemble each other. But we have also seen that black protesters appear much more to resemble the average member of their racial community than white protesters do theirs. This discovery has led to the construction of a tentative argument about the different social location of protesters in their respective racial communities.

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[28] Pearson *r* coefficients for the relationship between length of residence and age are .53 for blacks and .74 for whites.

[29] A study of protest incidents in 43 American cities indicates that protest is a more effective tool for gaining access to public officials than it is for gaining substantive demands. Protesters met with their target to present their demands in 58 per cent of the 120 cases of protest, but they gained concessions in only 15 per cent of the protests. Eisinger, "The Conditions of Protest Behavior in American Cities," p. 17.

The Uses and Organization of Protest Among Blacks and Whites

To this point we have examined data which indicate that in Milwaukee (1) black attitudes are broadly congenial to protest as a tactic while white attitudes are generally hostile; (2) black protesters, while socioeconomically somewhat better-off than the norm in their community, are still more likely to resemble black nonprotesters than white protesters (also better-off) resemble white nonprotesters; and (3) black protesters seem to exhibit more stable attachments to the community relative to nonprotesters than do white protesters. All of these findings suggest that protest activity is a relatively normal form of political participation among blacks and that protest participants are integrated members of the black community. Protest among whites, however, is an act of nonconformity in the broader white community, and white protesters represent a minority not only by virtue of their small numbers[30] but by their marked deviance from the average measures of social status and community attachment. Further data on the use to which protest is put and the means by which it is organized confirm and complement these findings.

First of all, protest in the black community is used almost exclusively in the local arena. While blacks occasionally participate in protests aimed at institutions of the federal government or at nongovernmental targets, the overwhelming focus of their protest in Milwaukee has been on municipal agencies. (Not only is most black protest aimed at city government, but, nationally, most protests against city government are carried out by blacks.)[31]

Whether or not this pattern reflects a greater interest among blacks in local government as opposed to government at other levels is difficult to say, but it does suggest a different level of focus. White protest participation was more evenly distributed among the various targets, as Table 7 indicates. White protesters also appear more physically mobile than black protesters. Forty per cent of the instances of protest participation cited by whites involved leaving the city of Milwaukee; only 11 per cent of those mentioned by blacks did.

In Table 7 instances of protest participation are divided according to the nature of the target and the particular area of concern which prompted the protest. State government is notably absent from the table, a reflection, undoubtedly, of its relatively low salience for the general public.[32]

Those who took part in protests against the

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[30] It is appropriate to recall here that only 11 per cent of the white sample had ever taken part in protest, while 21 per cent of the black sample had done so.

[31] Eisinger, pp. 16–17.

[32] See for example M. Kent Jennings and Harmon Zeigler, "The Salience of American State Politics," *American Political Science Review*, 64 (June, 1970), 523–535.

Table 7. Targets of Protest

	Black		White	
Federal Government		3% (4)		24% (23)
Antiwar protest	(2)		(15)	
Demonstrations to escalate the war in Vietnam	—		(7)	
Presidential candidates	(2)		(1)	
Local Government		68% (101)		20% (19)
Welfare	(9)		(1)	
Schools	(12)		(5)	
Police	(8)		—	
Jobs	(8)		(3)	
Housing	(64)		(10)	
Universities		* (1)		10% (10)
Martin Luther King Memorial Marches		5% (8)		4% (4)
Private Institutions		—		3% (3)
Protest Seeking "Civil Rights" or "Equality" (target unspecified)		17% (25)		32% (31)
Other		7% (10)		7% (7)
		100% (149)		100% (97)

* Less than one per cent.

federal government and its representatives were largely antiwar demonstrators, participants in a movement in which blacks, by all accounts, were not vigorously active.

A breakdown of protest against local government targets shows that most Milwaukee blacks were moved to take part in protest through the campaign led by Father James Groppi for a local open housing ordinance. These marches took place almost daily over a six-month period from the end of August, 1967 to March, 1968. This ready availability of an outlet and focus for protest undoubtedly exaggerated or increased the concern for open housing in relation to other local issues. Nevertheless, even if the Groppi protesters are excluded, most blacks who took part in protest still directed their energies against local governmental institutions and agencies.

After housing, issues having to do with public education stimulated the most protest participation among blacks. This finding accords with data on aggregate patterns of protest incidents in American cities: a survey of protests against local government targets in 43 large cities found that the major target of protesters was the public education system.[33]

The overwhelming focus of black protesters on local government is probably best explained by the greater black dependence on municipal services. For blacks much more than whites, the level and nature of the local public commitment to housing, police, schools, and welfare substantially determine the quality of their daily lives. Hence, the perceived failures, oversights, and transgressions of municipal government are less easy to ignore, for most blacks have no way of substituting private resources for inadequate public ones in order to secure the decent amenities of urban living. Whatever the explanation, the patterns of participation indicate that protest is a tactic of major importance on the local scene for blacks, an integral part of their pursuit of urban politics. For whites it plays a more peripheral role in local politics. Indeed, many whites who take part in protest directed at local targets do so in behalf of primarily black causes, judging from the proportion of instances of white participation in the Groppi marches.

While the races appear to use protest for substantially different purposes, one aspect of their protest experience is very similar. For most people of both races individual protest participation is not a function of organizational membership. Only 27 per cent of the black protesters and 20 per cent of the white said they belonged to an organization which had taken part in some type of protest.

Despite the lack of formal group affiliation among the protesters, however, most protests in which blacks took part have been carried out by stable, ongoing organizations rather than by ad hoc groups created for a single protest campaign. When respondents were asked who organized the protests in which they had taken part, blacks named Father Groppi as the organizer 75 times out of a total of 149 instances of participation. Groppi was the head of the NAACP Youth Council, also known in Milwaukee as the Commandoes. Groppi used the organizational base and resources of this group to launch a number of protest campaigns, including the six-month open housing marches among others. An additional 31 instances of black participation were inspired by civil rights and welfare rights organizations.

In contrast white protesters were mobilized less frequently by ongoing organizations. Out of a total of 97 instances of participation, only 11 were prompted by Groppi, six by civil rights groups, and seven by peace groups. Twenty, however, appeared to be stimulated by ad hoc student groups and 13 by individual clergyman. Another 10 occurred in protests organized by ad hoc neighborhood organizations, formed to press one particular grievance (see Table 8).

These findings are suggestive on several counts. For both races the major portion of the potential protest constituency does not lie within the confines of organizational membership lists. While we can be certain that many protest leaders or organizers know their potential constituency, we can be equally sure that many do not. The task of mobilizing unattached or unaffiliated individuals for protest can now be seen to assume major proportions. Communication with potential participants is made more difficult by the absence of organizational ties and networks linking those indi-

Table 8. A Breakdown of the Type of Group Responsible for Stimulating Instances of Protest

	Black	White
Ongoing stable organization	72% (107)	26% (25)
Ad hoc organization	11% (17)	46% (45)
Undetermined	17% (25)	28% (27)
Totals	100% (149)	100% (97)

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[33] Eisinger, p. 17.

viduals together. Protest constituencies must often be assembled anew each time a protest occurs.

The two races, however, seem to confront this problem differently. While we have no way of knowing at present whether or not this is peculiar to Milwaukee, blacks more than whites seem to have institutionalized the organization of protest to a greater degree. Hence, they are potentially more efficient in performing the mobilization task.

Institutionalization occurs because most protests are organized by ongoing groups. These groups supply a ready leadership cadre, as well as auxiliary workers. They have other resources at hand as well, such as office space and mimeograph machines. In addition, they have institutional experience and memories, both of which may be drawn upon over and over to provide guidelines and lessons for dealing with new situations. In effect the black organizations which mobilize protesters on a semiregular basis as one of their several functions are analogous in certain respects to local political party organizations and may in some sense be surrogates for them. Party organizations are after all essentially collections of leaders and workers, one of whose tasks is to seek out, identify, and mobilize potential voters among the populace.

The protest into which whites are drawn is less institutionalized in the sense that its organization is carried out most often by ad hoc groups created for the immediate purpose at hand. The task of mobilization is made all the more difficult by the fact that white groups must organize anew, establish leaders, create communications networks, seek workers and resources, and set out without prior institutional experience to identify likely partisans of a particular cause.

The patterns of the uses of protest and the ways in which it is organized substantiate and enlarge upon the earlier findings based on protester characteristics. All of the evidence presented indicates that protest is a tactic which blacks have singularly adapted to the pursuit of urban politics, a tactic moreover which represents an integral and widely accepted part of the institutionalized political life of the black community.

Protest for whites involves a more eclectic focus, for their efforts are not confined to the local political arena. In any given political setting, white protest is a relatively rarer phenomenon, carried out by a small, socioeconomically privileged segment of the population. White protest is, then, extraordinary, noninstitutionalized, and deviant from the norms of the broader community.

Some Conclusions

While one might argue that even the most casual observers have known for a long time that blacks support[34] and use protest, the data presented above not only specify and describe the scope and nature of that support for the first time but also suggest its *integral* as opposed to its *extraordinary* role in the black community. This sort of modification and specification of commonly held interpretations is useful in itself. The data and analysis offered here, however, also throw light upon several other problems. These are: (1) the relationship of socioeconomic status to protest participation; (2) the relationship of the different social locations of black and white protesters to the differing racial attitudes toward protest; and (3) the implications of the institutionalization of protest among blacks.

(1) *Social Status and Protest Participation.* If we examine protest participation as a function of individual social characteristics, it is clear (at least in Milwaukee) that protesters themselves cannot be called "poor." They rank higher on every measure of social status than do their nonprotester fellows in their respective racial communities. To what extent can we accept, then, the alternative interpretation that protesters are at least drawn from "poor" groups, that is, in the words of Lipsky, those groups which are powerless by virtue of their lack of conventional resources?

This interpretation is of little use in understanding the roots of white protest, for the latter cannot be viewed as an expression by advantaged members of a socioeconomically disadvantaged group. There are at least two other interpretations, both more satisfying logically and empirically. On the one hand we might view white protest as an expression by members of groups which lack power either because they lack legitimacy or because their members do not occupy positions of authority and power. At least one fourth of the instances of protest cited by whites were connected with the peace movement or were organized by college students. These represent subgroups which, though high in social status, do not command universal attributions of legitimacy and whose ability to gain their ends is very much a function of the fact that their members do not occupy positions of formal power. On the other hand we may see white protest as a means of representing or supporting disadvantaged groups to which the white protesters themselves do not belong. This interpretation can help to explain why approximately half the instances of white protest took place on behalf of black groups and black causes. Viewing protest as a means of supporting or representing other groups, whose powerlessness may or may not be a function of the lack of conventional socioeconomic resources, scarcely permits one to call white protest a tactic of the poor.

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[34] Gary Marx published data documenting black *support* for protest as early as 1967. *Protest and Prejudice* (New York: Harper and Row, 1967), pp. 15–17.

Now in the case of black protest it is at first glance intuitively acceptable to view protest as a tactic of the relatively powerless whose condition is a function of their low status in the socioeconomic hierarchy. That is to say, we know that blacks as a racial group are socioeconomically disadvantaged in relation to whites as a group and thus are relatively powerless. Yet within that disadvantaged group, those who are most likely to take part in protest are better off. But the problem with interpreting black protest as a tactic of a poor collectivity is that it is not clear that the *powerlessness* of blacks is any *more* a function of their collective relative poverty than it is of their lack of legitimacy (a function simply of being black or of adherence to racially threatening ideologies) or of their lack of formal power in political institutions. In short, the relationship between the lack of conventional socioeconomic resources on the part of any given group and the tendency of that group's members to take part in protest is not at all clear. Previous formulations of protest as a tactic of the powerless (defined as a function of the lack of conventional resources), the under-classes, or the poor require further specification and clarification and may in many cases be plainly wrong.

(2) *Social Location and Attitudes Toward Protest.* In one part of this article data were presented concerning attitudes toward protest. Later we examined data which established the social status of the protesters, and more broadly, their social location in their respective racial communities. It is possible to argue—speculatively, at least—that the patterns of racial attitudes are related to the somewhat different social locations of black and white protesters.

As we have seen, the two racial communities in Milwaukee regard protest in substantially different terms. By engaging in protest, both blacks and whites violate certain conventions of the dominant white community and thereby become subject to whatever social costs that community imposes—costs such as public disapproval, vilification, rebuke, ostracism, and even assault.[35] But the potential burden on black protesters is eased to some extent, for blacks find much support for protest in the attitudes of the black community. Black community support may in some sense serve as a countervailing force to the negative white attitudes toward protest. In short, protesters of different races face potentially different net social costs for their unconventional political expression.

Two points are needed to elaborate and qualify this argument. One is that social costs are not the only sanctions to which protesters might be subject. Economic reprisals (loss of job) and legal sanctions represent additional burdens which members of the society may choose to impose on protesters. These particular costs may in fact be potentially higher for black protesters than for white protesters, although we have little hard evidence to indicate that this is so. Nevertheless, it is a possibility. In the absence of data, however, the discussion here must be limited to an exploration of the impact of differential social costs.

Second, whites surely do not enter protest actions totally bereft of any social support; rather they generally do so with the approval of their reference groups, such as the student community or the peace movement. The support they derive from these subgroups may buffer the disapproval of the larger society just as the black community's social norms provide a buffer for black protesters.

The difference, however, is that blacks enter protest with the *broad-based* social support of their racial community. This is a general sort of social approval which *transcends* the distinctions between protesters and nonprotesters, and between black society at large and its subgroups. The support given white protesters on the other hand comes from subgroups which *themselves* frequently command only modest legitimacy or incur deep and wide-spread ambivalence in the white society at large. Furthermore, white protesters must eventually leave the protective cover (such as it is) of their supporting subgroups and come to terms with the greater society. College students graduate from school; the peace movement, diffuse in the first place, has seldom supplied an all-encompassing or permanent life environment. Blacks, however, are always members of the black community and may always draw on the support it provides.

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[35] Merton discusses political nonconformity and the "almost inevitable punishment" by the group which results. *Social Theory and Social Structure* (Glencoe, Illinois: The Free Press, 1957), p. 365.

Given (1) that white protesters are subject to white norms and expectations, which are hostile in regard to protest; and (2) that blacks are subject to both black and white norms, which tend to neutralize one another to some degree, it is plausible to expect that these differences should have different consequences for the individual protester, depending on his race. It is highly probable that those who must potentially incur the greater social costs (whites) must have compensating resources and characteristics, while those who face much lesser costs (blacks) have less need of compensation. Now let us recall that white protesters stand significantly above the socioeconomic norm in their community, while blacks hover much closer to the norm in theirs. The reason for such a pattern, I would speculate, is that social status resources may provide the sort of compensation necessary to offset social disapproval.[36] Since whites face higher net costs, they have need of greater resources. Black protesters, too, are somewhat better off than the average member of their racial community, although they are not as socioeconomically secure or differentiated as their white counterparts. They have need of compensating resources as well, however: although they enter protest with the support of their community, they must also deal with the disapproval of the larger community in which they live.

Consider now the problem for whites of the costs of defying white norms from the perspective of the data on home ownership and length of residence. It can be argued that defiance is possible not only if one has compensating resources but also if one's place in the community is relatively peripheral or less secure. It seems reasonable to assume that the more tenuous the roots one sinks in a community, the less likely he is to feel bound by its social conventions. Thus, we find that whites who take part in protest are less likely to own than to rent their home, and they have not lived in the community as long as white nonprotesters have.

This argument as a whole has some curious implications. Given the potentially greater social costs for white protesters, and assuming that the value placed on social approbation does not vary systematically by race, it would seem that *on the average* white protesters must feel more intensely about the issues which draw them into demonstrative politics than black protesters do. Given the liberal conviction, perhaps, that the injustices blacks have protested against in the last decade stand at the top of the list of injustices in America, this is not a conventional interpretation of the domestic political scene. But it would seem to be a logical one in the context of this analysis: if whites must pay higher net social costs for their participation, then they will participate only when their feelings are deeply enough felt to make the expense worth it. Blacks, who risk less in terms of such costs, can afford to protest more casually.

Now, simply being white probably confers certain advantages in political contests, advantages which relate to everything from the manner in which an official receives petitioners to the likelihood of a favorable response. Blacks employed protest early in the 1960s in part to overcome such white advantages, for protest is a device which cuts through some of the subtle biases in the contest to influence public decisions. But if the foregoing analysis is correct, then whites, by their greater intensity of feeling in protest, have simply established yet another advantage over blacks in political conflicts in which the races are competing for the same resources or for an official's ear, insofar as intensity of feeling enters the calculus of decision making.

(3) *The Institutionalization of Protest in the Black Community.* It is a standard observation that the institutionalization of a process leads to its routinization, a reduction of its initial novelty, excitement, and impact. Protest among Milwaukee blacks is relatively institutionalized in relation to that among whites. If it is not yet routine, then it faces the danger of becoming so. After more than a decade of experience, the value of mass protest persists

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[36] While this proposition remains here in hypothetical form, there is evidence that those with resources such as education and a prestigious occupation *value* those resources more than those without them. There is also some slight evidence that people without such resources (i.e., the working class) compared to those with such resources are more likely to feel dependent on others' opinions than on their own capabilities for economic advancement. Herbert Hyman, "The Value Systems of Different Classes: A Social Psychological Contribution to the Analysis of Stratification," in *Class, Status and Power,* ed. Reinhard Bendix and Seymour Martin Lipset, 1st ed. (Glencoe, Ill.: The Free Press, 1953), pp. 430, 434, 437. These findings do not suggest that social resources may compensate for the loss of social approval, but they indicate that protesters, because they are better-off, may be more independent of the opinions of others and more personally satisfied with the status resources they control than are those who are both nonprotesters and less well off.

only so long as protest is organized more or less spontaneously for the issue at hand. Long protest campaigns and long-lived, stable protest organizations are not likely to maintain their status as frightening, forceful, galvanizing devices. To lose the capacity to shock, and thereby to gain media attention, is to lose a means of communicating with what Lipsky has called "third party reference publics," who serve as potential allies.[37]

In such a situation the protest organizer will be under pressure to escalate his tactics to break the routine and re-establish the credibility of his passion. Escalation ultimately leads to the necessity to consider violence, a step which most practitioners of protest do not wish to take. So they are caught in a dilemma, the resolution of which is not at all clear. In any event, the routinization of protest implies that the more it is used, the less effective it will be. This means that black urban communities which have relied upon protest as an important means of wielding influence in the city may strip themselves of power by their own efforts to gain it through protest. To weaken oneself despite the intensification or persistence of one's own struggle is perhaps both the irony and the true meaning of powerlessness.

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[37] Lipsky, pp. 1145–1146.

The Friendship Group as a Protective Environment for Political Deviants*

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There is a limit to the legitimate interference of collective opinion with individual independence: and to find that limit, and maintain it against encroachment, is as indispensable to a good condition of human affairs, as protection against political despotism.[1]

The importance of groups as mediators between individuals and the larger society is a persistent theme of theories of political democracy. From the perspective of the development of the individual's political role, groups are typically seen as socializing agencies, affecting the basic values, attitudes, and political skills of their members.[2] At the same time groups are credited with playing a vital role in preventing political totalitarianism by insulating individuals from the direct influence of the state and by providing arenas for the development and expression of minority points of view.[3] Thus, the relationship between the individual and the group may be viewed from at least these two complementary perspectives: (1) the influence of groups on the attitudes and behavior of members, and (2) the process by which individuals join together in groups to express and protect their political beliefs. Depending on the perspective adopted, the hypotheses which are important to study will vary considerably.

Drawing on the first perspective, the present research began as an attempt to specify the conditions under which small, informal, ostensibly nonpolitical groups affect the political attitudes and behavior of members. The basic assumption was that primary groups tend to transmit and reinforce the norms and values of the larger culture of which they are a part. Because the influence of groups on their members depends on the existence of affective ties among members, cohesive groups are more effective in transmitting norms than weakly integrated groups, where deviant opinions are more likely to go unsanctioned or sanctions are less likely to be effective because of the low psychological investment of members in the group. For example, Putnam found that people who are socially well integrated in their communities tend to be much more similar to the community as a whole in their voting patterns than are people who are less well integrated. Community influence was found to operate through social interaction in primary friendship groups and secondary associations.[4]

It was therefore expected that the greater the integration of members into their primary groups, the more they would be influenced by the group and the more their attitudes would be consistent with the norms and values of the larger cultural groups to which these primary subgroups belong. In fact, however, high integration within the primary group was associated with holding *deviant* political opinions. This and other initial findings suggested that for the particular groups and attitudes studied, the relationship should be viewed from the second perspective, emphasizing the *effect of attitudes on individual integration into social groups*.

Derivations from available theory led to further exploration of the data which suggested that the propensity to form small friendship groups is a function of the perception of oneself as a political deviant. That is, the relationship between attitudinal similarity and the forming of friendships is not a simple one. Political deviants, but not political conformers, were motivated to form friendships on this

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[1] J. S. Mill, *On Liberty*, cited in *Group Dynamics*, ed. Dorwin Cartwright and Alvin Zander (New York: Harper & Row, 3rd ed., 1968), p. 141.

[2] Sidney Verba, *Small Groups and Political Behavior* (Princeton: Princeton University Press, 1961), pp. 21ff. The classic statements of this position in the voting behavior literature are Paul Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* (New York: Duell, Sloan, and Pearce, 1944) and Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: The University of Chicago Press, 1954).

[3] William Kornhauser, *The Politics of Mass Society* (New York: The Free Press, 1959).

[4] Robert D. Putnam, "Political Attitudes and the Local Community," *American Political Science Review*, 60 (September, 1966), 640–654.

basis. Thus, while the study began with an image of the individual being acted upon by group forces, the analysis suggests, rather, that individuals are active agents in the construction of their social groups, and that these social groups are formed on the basis of prior attitudinal factors. The groups may then serve as protective environments for individuals whose attitudes deviate from those of the culture around them.

Primary Groups in the Attitude Formation Process

A basic way that groups influence the attitudes and values of their members is by generating a group culture, which defines certain values as correct or proper. Through the selective application of sanctions, "correct" (i.e., group-supported) attitudes and values are then rewarded, and "incorrect" ones are punished. Basic dependence on groups for social approval usually makes this process very effective. The threat of sanction often is not even necessary to induce conformity to group norms. Numerous experiments (by Asch and others) demonstrate that the mere expression of unanimity by other group members induces conformity in most subjects even when the group has no history or continuity and the subject's own sense perceptions tell him that the group is wrong.

In persuasive interactions, primary groups are more influential than larger groups because strong affective bonds tend to develop among primary group members. Expulsion from the group or isolation from other members is then particularly painful. For example, Langton and Jennings found that a major reason that mothers generally have a stronger influence than fathers in the political socialization of children is that most children feel a stronger sense of attachment to their mothers.[5] Similarly, Middleton and Putney report that students who felt distant from or hostile to their parents were much more likely to deviate from their parents' political positions than were students who had warm affective relationships with their parents.[6] In general, then, primary groups that develop relatively strong bonds among members are more effective in influencing them and in encouraging normative and attitudinal conformity and homogeneity. From the individual perspective, group members who develop strong bonds with others in the group should be more affected by the normative climate of that group.

Since the norms that primary groups enforce are usually those of the larger social structure of which they are a part, many large organizations establish networks of smaller, more intimate primary groups as a way of propagating organizationally supported norms. The Communist party, through the cell structure, is perhaps the ideal-typical example of the use of primary groups to transmit organizational norms. The basic process also is exemplified however, in the subcommittees, task forces, and social and educational subgroups set up by churches, unions, business firms, and other organizations in all societies.

The Data

The data were collected in early 1961 as part of a study of political communication during the Kennedy-Nixon presidential campaign.[7] The primary groups studied were work groups in the large automobile plants in the Detroit area. All work groups within all plants in the Western Greater Detroit Region of the United Auto Workers union were defined as the universe of interest. A "work group" was defined as all individuals supervised by a given foreman. Twenty-four work groups of from 10 to 45 members were selected for the sample.[8] Attempts were made to interview every member of each selected work group. The overall response rate was 79 per cent; the median group rate was 82 per cent. The final N was 419. Twelve individuals who did not provide sociometric data were omitted, so that the basic N for most of the present report is 407.

Operationalizing Integration into Social Groups

To some extent, measures of individual integration into social groups depend upon how one conceptualizes cohesion at the group level. Once group cohesion is defined, the degree to which an individual shares in a group's co-

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[5] Kenneth P. Langton and M. Kent Jennings, "Mothers Versus Fathers in the Formation of Political Orientations," in *Political Socialization*, ed. Kenneth P. Langton (New York: Oxford University Press, 1969), pp. 67–69.

[6] Russell Middleton and Snell Putney, "Political Expression of Adolescent Rebellion," *American Journal of Sociology*, 68 (March 1963), 527–535. Herbert McClosky and Harold Dahlgren report similar findings in their "Primary Group Influence on Party Loyalty," *American Political Science Review*, 53 (September 1959), 757–776.

[7] The sample was designed by and the data collected under the direction of Donald E. Stokes and Warren E. Miller. I served as Assistant Study Director. The data were collected through the Detroit Area Study of the University of Michigan.

[8] The universe of work groups was stratified by company, shift, political activity level of local union leaders, and ability of the work group to talk informally during the working day. The work groups were then chosen randomly to balance the stratification variables.

Table 1. Validation of Number of Reciprocated Choices as a Measure of Friendship Integration

	Number of Reciprocated Choices				Total
	None	One	Two	Three+	
A. Length of time on current job					
Less than three years	30%	27	27	16	100% (140)
Three years or more	20%	20	28	32	100% (267)
Gamma = .27					
B. Is this the kind of job where you can talk with people around you while you are working?					
No	29%	23	26	22	100% (121)
Yes	21%	22	28	29	100% (286)
Gamma = .15					
C. During the last campaign did you discuss politics with any of the people who work under your foreman? (Number discussed politics with)[a]					
None	26%	27	26	21	100% (160)
1–4 people	15%	25	34	26	100% (116)
5 or more people	14%	14	31	41	100% (104)
Gamma = .28					
D. Work group size					
8–16	21%	24	29	26	100% (137)
17–21	31%	22	20	27	100% (149)
22–32	17%	21	36	26	100% (121)
Gamma = .04					

[a] Twenty-seven individuals who came into their current work groups after the election campaign are omitted from this table.

hesiveness may then be derived from the group level concept. In this study, group cohesiveness was conceptualized as the strength of interpersonal ties among group members.[9] The measure of the individual's integration into the group therefore had to indicate the extent to which he interacted with other group members.

As part of a battery of questions on social relations in the plant, each worker received a card listing all other members of his work group and was asked, "Of the people who work under your foreman, which five would you say you are most friendly with?" These friendship choices were the raw data from which sociometric matrices were constructed for each group. The number of *reciprocated* choices for each individual is the operational measure of integration into the work group.[10] Since it is based on a question specifying "friendliness" as the choice criterion, the measure is designated as "friendship integration." The friendship integration scores ranged from 0 to 10, with a mean score of 1.79.

While the number of reciprocated friendship choices has high face validity as a measure of interpersonal communication and the individual's integration into the group, further validation tests were undertaken (Table 1). First, the number of reciprocated choices increases as a function of time spent on the job. Second, jobs which permit talking with others tend to facilitate the development of friendships. The most directly relevant validation test, however, was the association between number of reciprocated choices and political discussion in the work group. This relationship also suggests the specifically *political* relevance of friendship choices, which is a central theme of this analysis. Finally, since sociometric measures may sometimes yield scores that reflect the number of people available for choice rather than substantive concepts,[11] the absence of a correlation between the number of reciprocated choices and group size was reassuring.

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[9] This is similar to Landecker's definition of group cohesion as "communicative integration." Werner S. Landecker, "Types of Integration and Their Measurement," in *The Language of Social Research*, ed. Paul F. Lazarsfeld and Morris Rosenberg, (Glencoe: The Free Press, 1955), pp. 19–27.

[10] Some measurement error results from the fact that response rates were not 100 per cent for any of the work groups. This meant that some respondents gave one or more of their choices to individuals with whom we did not secure interviews, so that reciprocity was precluded. For these respondents, the number of reciprocated choices may underestimate the number of friends they actually had. An additional possible source of difficulty is that the groups varied in size. Because respondents were asked to choose five friends from the list of persons in their work group, the measure appears to favor individuals in smaller-sized groups, where the likelihood of reciprocation is greater. A number of corrections to the raw score were attempted to adjust for this possibility, but none of these measures had as good validity as the raw score itself. And, as reported in Table 1D, the anticipated difficulty of a negative correlation between the raw number of reciprocated choices and group size did not materialize. This fortunate result appears to have been caused by an interesting "natural" correction mechanism in the sociometric question, whereby people in smaller groups tended to give fewer choices than we requested, while those in larger groups insisted on giving more (all choices were coded). Thus, the probability of a reciprocated choice in the different sized groups was substantially equalized by this phenomenon.

Friendship Integration and Political Attitudes

A basic working assumption of the study was that the more integrated an individual is into his work site primary group, the more subject he is to its political influence. In a pluralistic society, however, we need to ask with which larger social group or organization will a given primary group associate itself as a source for its political norms?[12] For the work groups studied here, political norms of the major secondary groups to which the workers belong converged substantially toward support for the Democratic party. Each such additional source of support for the Democratic position should then serve to reinforce the others. In the absence of opposing attitudes, primary group integration should therefore increase support for these consensual norms.

The workers belonged to at least two dominant subcultures favoring the Democrats. The first is the blue-collar, working-class subculture in general. The historical association of the working class with the party of Roosevelt is well known, and in 1960–61, this association was still strong. For example, 43 per cent of the labor sample described themselves as Strong Democrats as opposed to only 21 per cent of the Survey Research Center national sample of the same year. Eighty-two per cent of the labor sample preferred Kennedy over Nixon in the 1960 presidential election, while the national sample split 49.5 per cent to 50.5 per cent. Second, the United Auto Workers had given consistently active support to Democrats in national and local elections, donating both money and campaign workers. Media support also was strong. In addition to the national newspaper, received by all union members, all of the locals had newspapers that supported Democratic candidates. The UAW also sponsored a twice-daily radio program (Guy Nunn's "Eye-Opener" and "Shift Break") which presented heavily anti-Nixon, pro-Democratic material. In sum, the major membership groups of the respondents were strongly Democratic. Because their work groups were viewed as subgroups of this Democratic political environment, I hypothesized that the level of friendship integration in the primary group would be positively associated with pro-Democratic attitudes.

Three related indicators of pro-Democratic attitudes were used to test the hypothesis: support for the 1960 presidential, gubernatorial, and senatorial candidates of the Democratic party. In each case, not only did the data fail to support the hypothesis, but the relationships were strong enough to suggest a stable pattern in the *opposite* direction—that is, that friendship integration was associated with *decreasing* support for Democratic candidates. The pattern clearly suggested that the workers with higher levels of friendship integration were simply more likely to be Republicans than were the less integrated workers. This was indeed the case. For example, of workers with no reciprocated choices at all, 60 per cent were strong Democrats and only 3 per cent were Republicans, while of those who had three or more reciprocated friendships, only 31 per cent were strong Democrats and 15 per cent were Republicans. Thus, as the level of friendship integration increased, the proportion of Democrats became smaller and the proportion of Republicans larger.

The discovery that friendship integration was negatively related to identification with the Democratic party threw into question not only the initial hypothesis relating integration to political attitudes, but also the underlying assumption that friendship integration should be viewed as an independent variable and political attitudes as the dependent variable. It seemed highly unlikely that basic partisan loyalties, known to be among the most stable of all political attitudes,[13] would be disrupted by friendships formed as an adult. Indeed, only 22 per cent of the total sample indicated that they had ever had some other party identifica-

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[11] Joan Criswell, "The Measurement of Group Integration," *Sociometry*, 10 (August 1947), 259–267. See footnote 10 for further discussion of the relationship between group size and number of reciprocated choices.

[12] For a general discussion of the conditions under which primary groups support or oppose the larger organization of which they are a part, see Verba, *Small Groups and Political Behavior*, 54–60.

[13] Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David Apter, (New York: The Free Press, 1964), pp. 238–241.

Table 2. Party Identification and Friendship Integration[a]

Reciprocated Choices	Strong Democrats	Weak Democrats	Independent Democrats	Independent Independents	All Republicans[b]
None	32%	22%	19%	9%	7%
One	23	17	12	34	38
Two	26	28	47	20	15
Three or more	19	33	22	37	40
	100%	100%	100%	100%	100%
	(174)	(94)	(59)	(35)	(40)
Gamma = .22					

[a] Party identification is measured with the question, "Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent or what?" Party identifiers are then asked if they are strong or not very strong Republicans or Democrats. Independents are asked to which party they feel closer. Independent Republicans and Independent Democrats are those Independents who feel closer to the named party. Independent Independents are those Independents who feel close to neither of the major parties. Five individuals who either had no party identification or refused to state it are omitted from this table.

[b] To achieve a more stable estimate, Strong Republicans (15), Weak Republicans (15), and Independent Republicans (10) are combined in this category.

tion, and since this group includes Independents who previously identified with a party, the amount of cross-party conversion is overstated. Moreover, of those individuals who indicated that their basic partisan predisposition had changed, only 35 per cent said that this change had occurred in the previous seven years, the average length of time on the current job. Thus, to the extent that friendship integration is associated with Republican partisanship, friendships within the work group probably did not affect these attitudes, particularly the basic party identification. Work group relationships may be capable of reinforcing dominant political attitudes, as the original hypothesis suggested, but it seems less likely that they could cause counternormative change. Rather, the relationship suggests that political attitudes operate in some way to affect friendship integration.

As a check on the presumed temporal order of this relationship, a measure of *past* political behavior (the individual's vote in 1956) was used. Even though many more people voted Republican in 1956 than in 1960, the magnitude of the relationship between 1956 presidential vote and friendship integration was about the same as the relationships involving current political behavior, and it was in the same direction. However, 45 per cent of the sample did not hold their present jobs during the 1956 election so that many, if not most, of the present friendship ties could not have existed then (barring joint movement of friends from job to job). If most of the present friendship ties were not in existence in 1956, they could not have influenced 1956 voting behavior; the friendship ties could, however, be a *product*, developed in the intervening years, of attitudes associated with a Republican vote in 1956. This relationship can therefore be interpreted as further evidence that political attitudes probably influence friendship patterns rather than the reverse.

The basic relationships between friendship integration and both party identification and presidential preference, viewed from this new perspective, are presented in Tables 2 and 3. As we traverse the party identification scale toward the Republican end, the level of friendship integration rises fairly consistently. Similarly, Nixon supporters tend to have more friends than Kennedy supporters. What can account for the differences in levels of friendship integration among these political groups?

Table 3. Presidential Preference and Friendship Integration[a]

Reciprocated Choices	Voted for or Preferred	
	Kennedy	Nixon
None	28%	5%
One	20	33
Two	28	29
Three or more	24	33
	100%	100%
	(322)	(70)
Gamma = .26		

[a] Fifteen individuals who did not know or refused to state their presidential preference are omitted from this table.

Republican Auto Workers as Political Deviants

In viewing partisan attitudes as the independent variable in the relationship between attitudes and friendship integration, it is important to understand the meanings which political attitudes have for the individuals who hold them. By considering how attitudes are useful to individuals, the ways in which they motivate behavior can be clarified. While social psychologists have posited many needs that attitudes fulfill, two are particularly germane in this context. Smith, Bruner, and White suggest that opinions play a critical role in people's social adjustment and in facilitating their relations with others: "It is by holding certain views that one identifies with, or indeed, differentiates oneself from various 'reference groups' within the population. . . . The act of holding certain opinions . . . is an act of affiliation with reference groups. It is a means of saying, 'I am like them'."[14] This act of affiliation does not merely express a need for conformity:

> A wide variety of psychological mechanisms is at work, motivating us to relate our destinies to those of the concrete membership groups around us and to those of the more remote reference groups to which we adhere. Requirements of ego defense, dependency needs, drives for autonomy, hostility, drives for status, and many other dynamisms may be involved.[15]

Similarly, Rosenberg posits that a prime gratification of political behavior is the use of political orientations as "unification devices" to affirm group cohesion, to "lubricate social relationships," and to establish interpersonal relations. Additionally, politics may serve as a source of gratification by providing "personal strength by immersal in movements which give support and a feeling of belongingness to individuals."[16]

These needs that attitudes serve suggest an explanation for the tendency of Republicans and Independents to have higher friendship integration scores than Democrats. As indicated above, the political environment of this sample is overwhelmingly Democratic. Not only is this true of the work situation, but also of the local area in which most workers live. In 1956–1960, Wayne County was 15 per cent more Democratic than the state of Michigan in virtually every election contest. The Democratic share of the two-party vote usually ran around 67 per cent. Even in Eisenhower's 1956 victory, in which he received 56 per cent of the state's vote, Wayne County gave 58 per cent of its votes to Stevenson. In 1960, when Michigan reflected the national split by voting 51–49 for Kennedy, the county gave Kennedy 66 per cent of its votes. In this context, the farther one departs from identifying as and voting Democratic, the more one objectively becomes a political deviant and the more likely one will so perceive oneself. If attitudes do serve to unite individuals to groups in their environment, this function clearly cannot be served by a Republican or Independent stance in an environment where most people are strongly Democratic. Republicans in such a situation may therefore have little political contact outside the work group and may attempt to compensate for this by increasing work group friendships. This seems somewhat paradoxical because the work group itself, of course, is made up predominantly of Democrats. Yet it is a social situation in which people are together for long periods of time with extended opportunities for interaction, and therefore one in which individuals may exert special efforts to serve a variety of personal needs.

Tables 4A and 4B show that Republicans are much less likely than Democrats to talk about politics with friends outside the factory or in the voluntary organizations to which they belong. (Data not presented here indicate that the same pattern holds for political discussion in the family and among neighbors and relatives.) It is particularly interesting that Republicans are more likely than Democrats to belong to voluntary organizations (last line of Table 4B), but the relationship between partisan loyalty and political discussion suggests that these organizations are not likely to provide outlets for political expression. From these relationships we can infer that as deviance increases, opportunities for sociopolitical communication tend to decrease. This is consistent with Putnam's conclusion that "potential minority voters have a disproportionately difficult time finding social support for their deviant views, especially in communities which are relatively one-sided."[17]

Despite its consistency in nonwork contexts,

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[14] M. Brewster Smith, Jerome S. Bruner, and Robert W. White, *Opinions and Personality* (New York: John Wiley and Sons, Inc., 1956), pp. 41–42.

[15] *Ibid.*, p. 43.

[16] Morris Rosenberg, "The Meaning of Politics in Mass Society," *Public Opinion Quarterly*, 15 (Spring, 1951) 6–7 and 8. Rosenberg credits Erich Fromm, in his *Escape from Freedom* (New York: Rinehart, 1941), with identification of this last source of gratification.

[17] Putnam, "Political Attitudes and the Local Community," 648–649.

Table 4. Party Identification and Extent of Political Discussion

	Strong Democrats	Weak Democrats	Independent Democrats	Independent Independents	All Republicans
A. With Non-Work Friends[a]					
Not much/None	61%	68%	68%	69%	82%
Some/A lot	39	32	32	31	18
	100%	100%	100%	100%	100%
N	(174)	(94)	(59)	(35)	(40)
Gamma = −.19					
B. With Members of Clubs, Groups, or Organizations[b]					
Not Much/None	72%	82%	85%	80%	94%
Some/A lot	28	18	15	20	6
	100%	100%	100%	100%	100%
N	(58)	(22)	(13)	(15)	(17)
Gamma = −.34					
% Belonging to a Club, etc.	33%	23%	22%	43%	42%
C. In the Work Group[c]					
No	48%	42%	27%	47%	32%
Yes	52	58	73	53	68
	100%	100%	100%	100%	100%
N	(157)	(89)	(55)	(34)	(40)
Gamma = +.17					

[a] "And how much did you talk about politics with your friends who aren't neighbors or don't work at your plant?"

[b] Body of table includes only the 125 respondents who belonged to any club, social group, or other organization. These respondents were asked, "How much do you talk about politics with the members of this group?'

[c] "During the last campaign, did you discuss politics with any of the people who work under your foreman?'

the relationship between party identification and political discussion does *not* hold in the work group. Of the six contexts for political discussion about which respondents were queried, only in the work group did Republicans report as much political discussion as Democrats; furthermore, discussion about politics tended to increase with political deviance (Table 4C). This departure from the previous relationships indicating less discussion by Republicans in other social contexts suggests that the work group may be serving a special function for individuals who deviate from the prevailing political norms of the larger culture. Far from reinforcing the predominant political norms, the work group apparently provides a haven and source of reintegration for those whose political views differ from others' in their nonwork social environments. While it might seem easier for deviants to find like-minded friends in contexts less Democratic than the factory, Tables 4A and 4B demonstrate that they apparently do not. Perhaps the relatively large number of hours spent at work encourages a stronger effort here than elsewhere. We will return to this general question below.

The Motivational Dynamics of the Relation Between Deviance and Integration

Given the affiliative function served by attitudes, at least three complementary theories can help explain why deviants should be more motivated than conformers to seek friends in their work groups:

(1) Deviating from group norms increases *anxiety* and a sense of isolation because the deviant is rejected by the group. He therefore seeks relationships in other groups to reduce anxiety and isolation and to increase social support.[18]

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[18] A. Pepitone and C. Wilpizeski, "Some Consequences of Experimental Rejection," *Journal of Abnormal and Social Psychology*, 60 (May 1960), 359–364. See also Stanley Schachter, *The Psychology of Affiliation* (Stanford: Stanford University Press, 1959). Schachter stimulated anxiety by threat of electric shock. Further experimental research supports the positive relationship between threat and desire for affiliation using both economic and social threats. See Mauk Mulder and Ad Stemerding, "Threat, Attraction to Group, and Need for Strong Leadership," *Human Relations*, 16 (November 1963), 317–334. Notable examples of the manipulative use of this relationship are reported by Franz Schurman and Edgar Schein. Schurman points out that the criticism program in the

(2) Deviance from a group norm leads the person to question his opinions, and motivates an *evaluation* of them. Evaluation is achieved through comparison of one's own opinions with those of other people.[19] Therefore, the need for evaluation motivates affiliative behavior.

(3) Deviance from a group norm arouses *cognitive dissonance*. One way of reducing dissonance is to seek additional social support for the deviant opinions, thereby adding new information consonant with one's opinions.[20] New cognitions to bolster deviant opinions might be found by increased interaction with others who hold similar attitudes.

These explanations are not contradictory in any way, and all three processes probably operate for the political deviants in this sample. Their decreased political communication outside the work group suggests that they may be rejected by others in a variety of social contexts.[21] They are therefore likely to take advantage of the extended opportunities for interaction in the work group to satisfy their psychological needs for affiliation and social support. This is done by forming many more friendships than others not so deprived in other social groups and unencumbered by the psychological needs of the deviant.

For Democrats in this sample, the work group is politically and psychologically less important. Needs for political expression can be satisfied by talking with family, friends, and members of clubs and other groups to which they belong. But for Republicans, the work group is an important source of political association. By establishing friendship relations there, they reduce social anxiety at the same time that they are able to express and evaluate deviant political opinions and to receive support for them.

"Systematic Replication" of the Findings

How much confidence can we have in these *ex post facto* explanations of the relationship between political attitudes and friendship integration? Since the explanatory ideas were generated from findings on this data set (even though an extensive body of theory supports the explanation offered and presumably could have been used to generate the correct hypotheses beforehand), the data cannot then be claimed to support the ideas. The conventional solution to this problem is to collect a fresh set of data on which the hypotheses can be tested (or to implore others to do so).

Because the type of data necessary to test these hypotheses is relatively difficult to collect (i.e., individuals in natural groups), and because some immediate sense of the reliability of the findings is inherently useful, a way to replicate the findings and test the explanation on the present body of data would be particularly desirable. The method chosen is the one used so powerfully by Durkheim in his classic study of suicide, that of subgroup analysis and comparison.[22]

Chinese Communist study groups, designed to identify individual deviance from group norms, evokes great anxiety and insecurity, and sets up a need for reintegration with the group ("Organization and Response in Communist China," *Annals*, 321 [January 1959], 51–66). From his studies of prisoners in the Korean War, Schein ("Interpersonal Communication, Group Solidarity, and Social Influence," *Sociometry*, 23 [June 1960], 148–160) argues that lack of interpersonal communication induces strong motives for reintegration and search behavior designed to re-establish communication through new social relationships. The Chinese Communists used this tendency to encourage prisoners, who were denied communication with each other, to develop relationships with the Communists or with cooperative prisoners. The type of interpersonal isolation implied by the present findings obviously is hardly comparable to the extreme, extended, and enforced isolation endured by the prisoners of war. Nevertheless, in relative scale, the motive for reintegration may also be present among political deviants who find few people in their social environments with whom to share political communication. In a less extreme situation, Alexander and Campbell found that lack of parental support for an adolescent's attitudes is associated with a stronger network of friendships among children. They suggest that the lack of support from parents leads to an imbalance which is resolved through greater support of friends. C. Norman Alexander, Jr. and Ernest Q. Campbell, "Balance Forces and Environmental Effects: Factors Influencing the Cohesiveness of Adolescent Drinking Groups," *Social Forces*, 46 (March 1968), 367–374. In the search for social support, the friendship group of two persons may be especially important, for the dyad is the only size group in which supportive interactions exceed aggressive interactions. Carl N. Zimet and Carol Schneider, "Effects of Group Size on Interaction in Small Groups," *The Journal of Social Psychology*, 77 (1969), 177–187.

[19] Leon Festinger, "A Theory of Social Comparison Processes," *Human Relations*, 7 (1954), 117–140.

[20] Leon Festinger and Elliot Aronson, "Arousal and Reduction of Dissonance in Social Contexts," in Cartwright and Zander, *Group Dynamics*, p. 130.

[21] Some research suggests that deviants also reject conformers. W. F. Norrison and D. W. Corment, "Participation and Opinion Change in Two Person Groups as Related to Amount of Peer Group Support," *British Journal of Social and Clinical Psychology*, 7 (September 1968), 176–183.

[22] For a discussion of Durkheim's methods of subgroup comparison as a type of replication, see Hanan C. Selvin, "Durkheim's *Suicide:* Further Thoughts on a Methodological Classic," in *Emile Durkheim*, ed. Robert A. Nisbet (New York: Prentice-Hall, 1965), 113–136. For a discussion of a variety of statistical procedures for "pseudoreplication" of findings on the same data set, see Bernard M. Finifter, "The Generation of Confidence: Evaluating Research Findings by Random Subsample Replication," in *Sociological Methodology 1972*, ed. Herbert L. Costner, (San Francisco: Jossey-Bass, 1972), 112–175. Since the basic pur-

The explanations offered for the relationship between political deviance and friendship integration can be used to deduce hypotheses about the conditions under which the original relationship should be weaker or stronger, and in this way the hypothesis developed on the basis of the findings can be tested on the same body of data which generated it. This procedure satisfies to a significant extent the reservation mentioned above. We will be "replicating" by analyzing the same relationship in several different subgroups. These replications are "systematic" because the subgroups used are chosen on the basis of certain theoretical properties which are relevant to a test of the explanation offered for the original relationship.[23]

The logic of the explanatory propositions offered above suggests that the strength of the relationship between political deviance and number of friendships in the work group should vary as a function of the extent to which the attitudes are in fact deviant for particular individuals. If the worker belongs to social groups in which having pro-Democratic attitudes is not a strong norm, other partisan attitudes are less likely to be defined as deviant and to lead to anxiety, dissonance, or a need for opinion evaluation.

The replication strategy used here is therefore to locate *especially* pro-Democratic social groups within the overall sample, and to examine the political deviance-friendship integration relationship in these groups in comparison with others in which support for the Democrats is weaker. Since the explanatory hypotheses rest on the assumption that objective deviants perceive themselves as deviants, and that it is this self-perception that motivates affiliative behavior, it follows that, where the pro-Democratic norm is stronger, the affiliative reaction to deviance should be greater.

This hypothesis and replication strategy can be applied to several (to some extent overlapping) groups:

(1) *Race:* Since the Depression era, the proclivity of blacks to vote for the Democrats has been steady and reliable. In the 1956 election, 33 per cent more blacks than whites living outside the South voted for the Democratic presidential candidate.[24] While the 1960 election actually represents one of the low points of black support for Democratic candidates in the post-War era, 68 per cent of blacks did vote for Kennedy, as compared to only 49 per cent of whites.[25] Given this relatively strong pro-Democratic tendency, we would expect that Republicanism among blacks should motivate friendship integration to a greater extent than among whites.

(2) *Religion:* While the religious polarization of the vote decreased between 1948 and 1956,[26] Catholics were still considerably more inclined than non-Catholics to vote Democratic in 1956; in 1960, when the popular vote was almost evenly split between the candidates, Catholics split 80–20 in favor of John Kennedy.[27] Therefore, we expect that political deviance among Catholics will have greater effects than among non-Catholics.

(3) *Social class:* The inverse relationship between measures of social class and Democratic partisanship has been demonstrated many times. Occupation, the status variable usually most strongly related to voting behavior, is virtually a constant in the present study. Formal education and income, however, vary somewhat and are also moderately related to partisan attitudes. Therefore, it is hypothesized that because of their stronger pro-Democratic norms, the positive relationships between deviant partisan attitudes and friendship integration will be stronger in the lower educational and income groups than in the upper groups.

(4) *Membership in the UAW:* Since the union has a history of vigorous support for Democrats, we can expect that the pro-Democratic norm grows stronger the longer one has been exposed to this union activity. It is therefore hypothesized that the relationship between political attitudes and friendship integration is stronger among longer-term than for short-term members of the UAW.

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pose of significance tests is to suggest the degree of eliability of a finding, Finifter argues that replications erve this purpose without making distributional and ther assumptions. Following this logic and because of he nature of the present sample, I have refrained from sing significance tests except in the section on the omposition of the friendship groups, below.

[23] For a discussion of "systematic" and other types f replication, see Murray Sidman, *Tactics of Scientific esearch: Evaluating Experimental Data in Psychology* New York: Basic Books, 1960).

[24] Even after controls for ten life-situation variables were introduced (representing region, size of place of residence, and social class), nonsouthern blacks were almost 12 per cent more Democratic in their voting behavior than were comparable whites. Angus Campbell, Phillip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley and Sons, Inc., 1960), pp. 300–306.

[25] Stanley Kelley, Jr., "The 1960 Presidential Election," in *American Government Annual, 1961–1962,* ed. Ivan Hinderaker (New York: Holt, Rinehart and Winston, Inc.), pp. 57–75.

[26] Campbell et al., *The American Voter,* pp. 298–306.

[27] Philip E. Converse, Angus Campbell, Warren E. Miller, and Donald E. Stokes, "Stability and Change in 1960: A Reinstating Election," *The American Political Science Review,* 55 (June 1961), 273.

Table 5. Relationships (Gamma) Between Political Attitudes and Friendship Integration, in the Total Sample and in Selected Demographic Subgroups[a]

	Friendship Integration and	
	Party Identification	Presidential Preference
Total Sample (*N*=402; 392)[b]	.22[c]	.26
Race		
Blacks (54; 52)	.38	.51
Whites (348; 340)	.19	.22
Religion		
Catholic (155; 154)	.31	.43
Non-Catholic (247; 238)	.16	.22
Education		
Non-High School Graduates (277; 271)	.25	.38
High School Graduates (125; 121)	.02	−.02
Hourly Rate of Pay		
Less than $3.00/hour (286; 279)	.24	.26
$3.00+/hour (112; 109)	.12	.12
Length of Membership in UAW		
Fifteen years or more (300; 292)	.25	.35
Less than fifteen years (102; 100)	.05	.01

[a] Friendship integration, party identification and presidential preference are coded as in previous tables. Pro-Republican attitudes received higher values so that the correlations may be considered as measures of the relationship between political deviance (i.e., Republicanism) and friendship integration.

[b] The base Ns for the party identification and presidential preference correlations are 402 and 392, respectively. Subcategory Ns are provided in the same order. There were four cases of missing data for hourly rate of pay.

[c] For example, the relationship (gamma) between friendship integration and party identification in the whole sample is .22; for blacks it is .38, etc. The relationship between friendship integration and presidential preference in the whole sample is .26; for blacks it is .51, etc.

If political deviance is what leads to an intensified motivation for friendships in the work group, then, for each of the social classifications mentioned above, the relationship between political attitudes and friendship integration should be stronger in those subgroups that have the stronger Democratic norm. Thus, five subhypotheses have been generated from the major explanatory hypothesis. The relationship between political attitudes and friendship integration in the work group will be:

(1) stronger for blacks than for whites

(2) stronger for Catholics than for non-Catholics

(3) stronger for individuals with less education than for those with more education

(4) stronger for individuals with lower incomes than for those with higher incomes

(5) stronger for individuals who have belonged to the UAW for a longer period of time than for those who have belonged for fewer years.

The data testing these hypotheses are presented in Table 5. The table is arranged so that the more Democratically-oriented group in each classification appears on the upper line. Thus, for each attitude and each classification, the prediction is that the correlations will be larger on the first than on the second line of each pair. Without exception, the hypotheses are supported.[28]

Another way of looking at the data is presented in Table 6. Given the considerably stronger relationships in each of the Democratically-oriented groups, mean scores on the friendship integration index were computed for Republicans and Democrats in each of these groups. (This analysis omits the middle category of Independent Independents.) The mean scores perhaps make clearer than the measures of association the extent to which deviants in each group are more highly motivated to affiliate than conformers. The data reported in Tables 5 and 6 therefore suggest

Table 6. Mean Friendship Integration Scores for Republican and Democratic Members of Groups Favoring the Democrats

	Republicans	Democrats
Blacks	1.67 (3)	1.16 (44)
Catholics	3.00 (4)	1.72 (142)
Non-High School Graduates	2.75 (20)	1.60 (237)
Less than $3/Hour	1.90 (21)	1.65 (240)
15 Years or longer in UAW	2.44 (25)	1.59 (251)

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[28] Technically, this is a "specification" analysis, in which the focus is on identifying the conditions under which the initial relationship is weakened or strengthened. The reader should note, however, that in controlling for the specification variables we are also in effect checking on the possibility of a spurious relationship resulting from any association between the control variables and the independent variables. That the resulting relationships become both stronger and weaker in a systematic pattern suggests that the spurious relationship type of interpretation is inappropriate. An analysis which presented only one partial correlation for each control variable (interpretable as an average relationship in the subcategories of the control variable) would obscure this pattern.

that the motivating dynamic in the relationship between political attitudes and level of friendship integration is the sense of deviance that accompanies attitudes that are not pro-Democratic.

Membership Groups as Reference Groups

A basic assumption on which this analysis rests is that the "social groups" I have been discussing actually do serve as sources of political norms for their members. Even though these demographic or sociological membership groups (blacks, Catholics, particular educational groups, and so on) do not necessarily engage in the interaction and communication with others that defines a social group, I will argue that we can still treat them as such, at least at the minimal level necessary to support the proposition that they are identified with certain political norms that guide the attitudes and behavior of their members.

This argument is based on three related propositions: (1) Many people do in fact identify psychologically with these membership groups; (2) these identifications motivate a responsiveness to normative cues coming from the groups; and (3) the levels of friendship integration between those who do identify with these groups and those who do not differ systematically. The differences referred to in the third proposition are consistent with the assumption that the membership groups function as reference groups, and with the hypothesis that perception of oneself as a deviant from one of these groups motivates affiliative behavior in the work situation. These data will be presented below.

In a commentary on the Index of Political Predisposition developed in *The People's Choice,* Berelson outlined the process by which social category groupings exert normative influences on individuals' behavior. The index was successful in predicting voting behavior because the IPP score "ranks people according to how strongly their *demographic location predisposes them to receive social influences . . . to vote for one or the other of the major parties. . . . The chances of social support for given political choices . . . vary with the distribution of such preferences in the particular segment of the community.*"[29] Thus, the analysis turns on the notion that demographic categories (membership groups) are likely to be associated with a set of socialization and social interaction experiences that convert *statistical* norms (e.g., the frequency of particular behaviors or attitudes) into *internalized* norms, that is, felt guidelines the individual accepts as his own and responds to in forming his attitudes and deciding on his behavior. To the extent that this process occurs, membership groups are also reference groups. The treatment of membership groups as reference groups therefore depends on the frequency of occurrence of this identification process, and on showing that the behavior of identifiers differs from that of non-identifiers in theoretically consistent ways.

Data from this study suggest that this identification process and the recognition of group norms associated with it are, indeed, frequent in these membership groups. For example, 89 per cent of the blacks stated that they had "quite a lot" of interest in how Negroes are getting along in this country, and 82 per cent believed that more Negroes were for Kennedy than for Nixon. Catholics were somewhat weaker in their group identifications. Only 48 per cent described themselves as "strong" Catholics. On the group-interest question, however, 73 per cent said that they had "some" or a "good deal" of interest in how Catholics as a whole were getting along. With reference to the social class categories, 80 per cent of the sample said that they thought of themselves as belonging to either the middle or working class, and almost the entire sample (96 per cent) were able to so classify themselves when specifically asked to do so. The same figures held for those in the lower education and income brackets (i.e., those who are not high school graduates, earn less than $3 per hour, or both). For those in this lower socioeconomic group who identified with the working class (83 per cent), 54 per cent had "a good deal" of interest in how working class people were getting along and an additional 38 per cent had "some" interest. Seventy per cent believed that the majority of the working class voted Democratic. Group identification was also high among individuals who had been members of the UAW for fifteen or more years, as was attribution of political legitimacy to the union. Fifty-two per cent think it is all right, without reservation, for unions to help political candidates (and an additional 20 per cent think "it depends"); on a different question only 27 per cent believe they should disregard the union's views on candidates.

These data support the assumption that social category membership groups may be treated as norm-bearing reference groups. If the individual knows that his political views are different from those of others in these membership groups, he is therefore likely to perceive himself as a deviant. Note that in this case the group does not cease to be a reference group for him. Instead, it becomes a *comparative* rather than a *normative* reference group.[30] Only because Republicans do identify with the membership group and do use it as a reference group do they come to perceive themselves as deviants. Identification with the membership group (1) increases the likelihood of the individual's knowing the attitudes and behavior patterns of other group members; (2) simultaneously increases his dissonance, discomfort, and anxiety if his own attitudes and behaviors are different,[31] and (3) increases the likelihood of his questioning the correctness of his deviant opinion. These factors then motivate a search for friendship support in other groups—in this instance in the work group.

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[29] Berelson et al., *Voting,* p. 126. Emphasis added.

The proper test of this reference group hypothesis, then, is among Republicans only. The hypothesis actually explains why some Republicans do not have as high friendship integration scores as others. If the motivational dynamics implied by the explanation are correct, then among political deviants (Republicans in Democratic membership groups), as identification with the membership group increases, friendship integration should also increase. Among those Republicans who are in membership groups with which they do not identify or with whose normative standards for political action, they are unfamiliar, there can be no sense of deviance. In this case, it would be unwarranted to expect that statistical deviance would result in behavior motivated to increase social support and evaluation opportunities.

A practical problem in testing this hypothesis obviously is one of sample size. As it is, the number of deviants (by definition as well as empirically) is small. To further subdivide them on the basis of membership group identifications jeopardizes the reliability of the findings. Notwithstanding this difficulty, but bearing in mind the possibility of fortuitous results, the findings of this phase of the analysis are reported because of their importance to the proper development and test of the theory. Because of extremely small subgroup Ns, the findings are offered only as tentative support for the hypothesis and to suggest how more reliable tests might be performed in further studies.

Table 7 presents the mean friendship integration scores for the members of each social group by their level of identification with the group.[32] Where questions measuring attribution of political legitimacy to a group or perception of a group political standard were available, they are also used. In each instance, the hypothesis predicts that, among political deviants, the stronger the identification with the group or the clearer the group's political norm, the higher the level of friendship integration.[33]

The table is arranged to contrast levels of identification within each group, by party. Attention should be focused first on the data for Republicans. In each comparison, the members with stronger identification are on the upper of the two lines, and therefore we expect that the friendship integration scores will be higher on the upper line of each pair than on the lower. For example, among the lower educational and income groups (category B), the relevant reference group is the social class.[34] Since the pro-Democratic norm is stronger in the working class than in the middle class, the effect of deviance should be more pronounced among working-class identifiers. Consistent with this hypothesis, the mean friendship integration score for those who identify with the working class is 2.57 as compared with 1.67 for middle-class identifiers. Similar results appear for Catholics and long-time UAW members. Since the impact of group norms is strongest among strong identifiers, more refined comparisons are made among class-conscious working-class identifiers (B.1, 2, and 3), and for long-time union members, among those strongly identified with the union.

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[30] Briefly, normative reference groups set standards which the individual is motivated to *follow;* comparative reference groups are used by the individual to *evaluate* himself and others. The same group may also perform both functions. See Harold H. Kelley, "Two Functions of Reference Groups," in *Readings in Reference Group Theory and Research,* ed. Herbert H. Hyman and Eleanor Singer, (New York: The Free Press, 1968), pp. 77–83.

[31] Festinger's hypothesis, which has been supported by experimental evidence, is that dissonance as a function of disagreement with a group increases with the importance of the group to the individual. Festinger and Aronson, "Arousal and Reduction of Dissonance," p. 130.

[32] The subgroup size difficulties are amply illustrated in the impossibility of doing a within-group analysis of black Republicans. There were only three of these and all were highly identified with their racial group and perceived it to have a Democratic political norm.

[33] Previous analyses have shown that group identification increases compliance with group norms. See, for example, Campbell et al., *The American Voter,* pp. 295–332. The present analysis in a sense focuses on one aspect of the unexplained variance in this relationship, i.e., what is the effect on high identifiers of *not* conforming?

[34] Because the same reference group questions are used for both of these social categories, they are combined in the analysis. For the total sample, the gamma between income and education is .60.

Table 7. Mean Friendship Integration Scores by Subjective Identification with Membership Group and Party Identification, for Members of Groups with Pro-Democratic Norms

	Republicans	Democrats
A. *Catholics*		
Strong Group Identification[a]	3.67 (3)	1.57 (61)
Weak Group Identification	1.00 (1)	1.86 (50)
B. *Non-High School Graduates OR Earn Less Than $3/Hour*		
Working-Class Identifiers[b]	2.57 (21)	1.66 (245)
Middle-Class Identifiers	1.67 (6)	1.59 (34)
Among Class-conscious Working Class Only[c]		
1. Good deal of interest in working class[d]	3.00 (7)	1.79 (112)
Some interest in working class	1.86 (7)	1.41 (75)
2. Perceive working class as Democratic[e]	2.56 (9)	1.66 (148)
Perceive working class to be split	2.20 (5)	1.63 (54)
3. Good deal of interest, perceive working class as Democratic	3.33 (6)	1.66 (89)
Compare with middle-class identifiers above	1.67 (6)	1.59 (34)
C. *15 Years or Longer in UAW*		
Strong Group Identification[f]	3.17 (12)	1.73 (138)
Weak Group Identification	1.77 (13)	1.42 (113)
Listen to or watch UAW programs[g]	3.11 (9)	1.68 (153)
Never attend to UAW programs	2.06 (16)	1.45 (98)
Among Strong Identifiers Only		
Union is legitimate source of voting cues[h]	3.40 (5)	1.53 (101)
Union is *not* a legitimate source of voting cues	3.00 (7)	2.24 (37)

[a] This index is composed of two questions: "Would you call yourself a strong Catholic or not a very strong Catholic?" and "How much interest would you say you have (a good deal, some, or not much at all) in how Catholics as a whole are getting along in this country?" The index was dichotomized so as to yield the most nearly equal number of cases in the strong and weak categories. "Strong" identifiers described themselves as strong in answer to the first question *and* had at least some interest in Catholics as a whole. All others were classified as "Weak" group identifiers.

[b] Class identification is based on the final response to the following sequence of questions: "There is quite a bit of talk these days about different social classes. Most people say that they belong to either the middle class or to the working class. Do you think of yourself as being in one of these classes?" and "Which one?" or "Well, if you had to make a choice, would you call yourself middle class or working class?"

[c] Working class identifiers who responded "Yes" to the first class identification question (see note b above), are defined as "class-conscious."

[d] "How much interest (a good deal, some, or not much at all) would you say you have in how working class people are getting along in this country?"

[e] "Do you think more working class people vote Republican, they are evenly split, or do more vote Democratic?"

[f] This index is composed of two questions: "How much interest (not much, some or quite a lot) do you have in how other members of the United Auto Workers are getting along?" and "And how much interest do you have in how other members of your own local union are getting along?" The index was dichotomized so as to yield the most nearly equal number of cases in the strong and weak categories. "Strong" identifiers had "a lot" of interest at one level (either UAW or local) and at least "some" at the other.

[g] "How often do you listen to or watch any programs on radio or television that are regularly sponsored by the UAW?"

[h] "In deciding how to vote, how much attention (none at all, not much, some, quite a lot) do you think union members should pay to what the union says about the candidates?" Any response except "none at all" is classified as according the union political legitimacy.

The large number of tests performed in this analysis compensates, at least partially, for the possible unreliability of findings based on any one test, in view of the small subgroup Ns. In each test, among Republican identifiers belonging to pro-Democratic membership groups, identification with the membership group is associated with higher levels of friendship integration in the work groups. Presumably the subjective identification converts a mere membership group into a comparative reference group; an individual's deviance from its norms then has motivational significance for his social behavior. Perception of the pro-Democratic tendencies of the group or legitimation of the group as a political agent is also associated with higher friendship integration levels. These factors apparently increase tension for the deviant by making the group norm clearer and more salient.

Consideration of an Alternate Explanation of the Findings. An alternative interpretation is that these group identifications are basically measures of gregariousness and that strong identifiers will therefore tend to have more friends regardless of whether or not they are or perceive themselves as political deviants. To investigate this alternative hypothesis, the subgroup tests reported above for Republicans were repeated for the Democrats.

If the group identification measures are indeed measures of gregariousness, then the relationships between identification and friendship integration should be as strong for Democrats as they are for Republicans. The relationships among Democrats are reported in the final column of Table 7. While all of the relationships had been in the hypothesized direction for Republicans, the trend for Democrats is much less clear. Two of the relationships are in the opposite direction (i.e., identifiers have fewer friends), and all of the others are considerably smaller than the corresponding relationships among Republicans. Thus, while some part of the group identification-friendship integration relationships may be due to gregariousness, this factor appears to be relatively minor. More striking is that the hypothesized relationships hold uniformly and with considerably larger differences for Republicans, as compared to the erratic and much weaker relationships among Democrats.

Composition of the Friendship Groups

The explanations developed above for the stronger tendency among political deviants than among conformers to form friendships suggest also that deviants will choose as friends like-minded others. While much research suggests that friendships form on the basis of attitudinal similarity, the analysis already presented suggests that we may expect differential effects depending upon whether the attitudes are deviant or conforming. We therefore need to analyze the *composition* of these friendship dyads to see under what conditions choices do tend to be intraparty specific.

The measure of friendship integration is based on reciprocity of friendship choices *within work groups*. Since the number of Republicans in the entire sample is small, some groups have no Republicans or only one. These work groups were therefore omitted from this analysis because they provide no possibility of homogeneous Republican friendship dyads. Of the twenty-four original work groups, nine, with from two to eight Republicans in each, remained.

In each of these nine groups, two null hypotheses were sequentially investigated:[35]

H_1. There is no tendency to reciprocate choices within work groups.

Once this hypothesis can be rejected, the hypothesis of more direct interest to us can then be tested:

H_2. Given a tendency to reciprocate, friendship dyads tend to be uniform throughout the work group rather than being related to partisan identification.

For each analysis, a chi-square test was used to compare the expected and observed distributions of friendship dyads described by the partisan identification of the dyad members. An example, presented in Table 8, from the group with the largest number of Republicans (but not the strongest relationships) will make the procedure clear. There are several points of interest in this table. First, for this particular group, H_1 may be rejected ($df = 4$, $p = <.001$) but H_2 may not be ($p = .4$). The first section of the table demonstrates that the ratios of observed to expected choices are much larger in the dyads where partisanship is homogeneous (*RR, DD*) than in the mixed dyads (*RD* and *IR, ID*) (where R = Republican, D = Democrat, and I = Independent). Thus, Republicans tend to overchoose one another, and Democrats do the same. However, more noteworthy for present purposes is the finding, signalled by both the O/E ratios and the dyad contributions to chi^2, that once the calculation of expected frequencies is adjusted for the general tendency to reciprocate (in the bottom half of the table), it becomes clear that Democrats choose each other only about as often as we would expect by chance, but that Republicans choose each other more than twice as often as would be expected by chance.

The equivalent of Table 8 was calculated for each of the nine work groups with at least two Republicans. Then a pooled chi^2 value was computed for each hypothesis.[36] The

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[35] I am grateful to Leo Katz, Director of the Statistics Laboratory at Michigan State University, for advice on this analysis.

[36] The pooled chi^2 value is simply the sum of the separate chi^2 tests and the sum of the degrees of freedom, which is then evaluated in the usual way. "In effect, we are saying that if a relationship comes out roughly the same each time but the probabilities of the separate results are each greater than .05, we still may ask ourselves how likely such a combination of

Table 8. Expected and Observed Composition of Friendship Dyads in an Illustrative Work Group[a]

A. Null H_1: There is no tendency to reciprocate choices within work groups.

Dyad Type	*E (reciprocation)*[b]	*Observed*	*$(fo\text{-}fe)^2/fe$*	*O/E*
RR	.73	4	14.65	5.48
RD	4.37	8	3.02	1.84
DD	5.46	13	10.41	2.38
IR, ID	2.26	4	1.34	1.77
II	.08	0	.08	0
	12.90	29	$\chi^2=29.50$	2.25

B. Null H_2: Given a tendency to reciprocate, friendship dyads tend to be uniform throughout the group rather than being related to partisan identification.

Dyad Type	*E (reciprocation)*[b]	*Observed*	*$(fo\text{-}fe)^2/fe$*	*O/E*
RR	1.64	4	3.40	2.44
RD	9.82	8	.34	.81
DD	12.28	13	.04	1.06
IR, ID	5.08	4	.23	.79
II	.18	0	.18	0
	29.00	29	$\chi^2=4.19$	

[a] This work group has the following characteristics: General Motors, day shift; natural size 38, interviews attained 32; 21 Democrats, 8 Republicans, 3 Independents.

[b] The subtables differ only in the computation of the chance expectation. The Es for the first table are based on the assumption of no tendency to reciprocate. The expected number of choices possible for each dyad type is equal to the product of the number of dyads *possible* for that type and the probability of a chance reciprocation in a group of that size. For example, given 8 Republicans in the group, the number of RR dyads *possible* is $8(7)/2=28$; the probability of a reciprocated choice in a group of 32, where each member is allowed 5 choices is $(5/31)^2=.026$; $28(.026)=.73$. More generally, the possible number of dyads of a *homogeneous* type is equal to the number of combinations of the type *n*, e.g., $\binom{8}{2}$ for Republicans. The possible number of dyads of a *heterogeneous* type is equal to the product of the two type *n*'s. The probability of a reciprocated choice in a group is $(d/N-1)^2$, where $d=$ the number of choices permitted and N is decreased by 1 because the member cannot choose himself. For the second subtable, which takes as a given the tendency to reciprocate, the expected number of choices possible for each dyad type is equal to the probability of a reciprocated choice of this type given the number of dyads possible for the type *and* the total number of reciprocations possible in the work group $\binom{N}{2}$, multiplied by the number of reciprocated choices actually occurring in the work group. E.g., the number of RR dyads possible is 28 (from above); the number of reciprocated choices possible in the group as a whole is $\binom{32}{2}=496$ (this can also be calculated by summing the number of dyads possible of each partisan type); the number of reciprocated choices actually observed in the group as a whole is 29. Given a tendency to reciprocate, we would therefore expect that of the 29 choices, 28/496, or 1.64, would be of the RR type

pooled chi² for Hypothesis 1 is 208.19 (31 *d.f.*, $p = <.001$).[37] The pooled chi² for Hypothesis 2 is 49.39 ($p = <.02$). Thus over the work groups as a whole, both null hypotheses may be rejected. Examination of the most prevalent patterns of dyads reinforces the conclusions suggested by the illustrative example in Table 8; namely, that friendship dyads do tend toward partisan homogeneity, but that once the appropriate allowance is made for the general tendency toward reciprocation, choice on the basis of partisanship occurs mainly among political deviants. Table 9 summarizes the data supporting this conclusion by reporting the mean ratios of observed to expected choices by dyad type over the nine work groups. Note that regardless of whether we use H_1 or H_2 as the base, the largest ratios occur among the deviant homogeneous types of *RR* and *II*. Given the strong tendency for reciprocation

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outcomes would be if there were no relationship in any of the . . . tables." H. M. Blalock, Jr., *Social Statistics*, (New York: McGraw-Hill, 2nd ed. 1972), pp. 309–310. In the present case, 8 of the 18 individual relationships were also separately significant. Chi square is, of course, highly sensitive to N, and the Ns for these analyses are rather small. In fact, the use of chi square in the individual work groups is technically inappropriate because the expected values for dyad types involving Republicans and Independents frequently fall below the generally accepted minimum. Nor does any combination of categories that is theoretically meaningful solve the problem. The results should therefore be interpreted in a tentative and exploratory spirit.

[37] Some work group tables had fewer than four degrees of freedom because they had no or only one Independent, thus eliminating the II and/or ID, IR lines.

Table 9. Mean Ratios of Observed to Expected Choices for Dyad Types

	Homogeneous Dyad Types			Heterogeneous Dyad Types	
	RR	DD	II	RD	IR, ID
Null Hypothesis 1	3.32	1.61	3.15	1.90	1.68
Null Hypothesis 2	1.67	1.01	2.22	.96	.89

of friendship choices to occur in the work groups, the second line makes clear that the political content of these choices is produced entirely by the proclivity of both Republicans and Independents to make choices within their own partisan groups. *DD* choices occur only as frequently as one would expect by chance, given the tendency to reciprocate, and the heterogeneous combinations of *RD* and *IR* or *ID* occur about as often as or slightly less often than expected. Thus, it is the political deviants who contribute disproportionately to the tendency toward friendship group formations in work groups and, when possible, they tend to choose as friends those with similar partisan attitudes.[38]

This substantive point must be stressed: The major finding of this paper is not that partisans tend to choose each other as friends, but that deviant partisans both have more friends and choose attitudinally similar friends much more often than conforming partisans. Thus, the analysis goes beyond the familiar finding that people form friendships on the basis of similar attitudes to specify a condition (attitudinal deviance) under which this tendency is accelerated. This specification contributes to an understanding of why the phenomenon occurs at all in suggesting that the interpersonal attraction that follows perceived attitudinal similarity is at least partially an attempt to ward off threat from potentially hostile nonbelievers.

Political Deviance, Friendship Support, and Political Participation

If political deviants do, indeed, feel threatened by a hostile political environment, they may be dissuaded not only from the informal expression of political attitudes but also from active political participation.[39] To the extent that friendships provide supportive environments for deviant attitudes, they should serve a similar function in encouraging political participation. Thus, we can expect to find a positive relationship between friendship integration and political participation among political deviants. Since friends do not serve the same supportive function for political conformers, friendship integration should not be related to participation among Democrats.

We would expect this effect on participation to occur primarily with respect to campaigning and other active types of participation in which the deviant's political position becomes subject to public scrutiny. Without the psychological support of friends, a deviant should be less willing to risk this sort of public exposure. But to the extent that he feels threatened in any expression of political views and therefore accustoms himself to political withdrawal, even the secret form of participation represented by voting should be affected.

Table 10 presents data to test this hypothesis. Voting in the 1960 election is the indicator of participation. While the small number of cases in the Republican sample again suggests caution, the data do tend to support the hypothesis. Among Republicans, voting participation is diminished unless friendship integration is at least moderate. Among Democrats, in contrast, voting is independent of friendship integration. Those in the political mainstream apparently need no special psychological support to encourage the expression of their political views.

Summary and Discussion

The research reported here began with the unanticipated finding that Republicans tended to have more friends in their work groups than Democrats did. Since the political context of the study sample is heavily Democratic, Republicans in this environment were viewed as political deviants, whose feelings of social insecurity and anxiety, dissonance, and need for opinion evaluation motivated a search for social support, protection, and affiliation. The work-group friendships Republicans are more likely to establish were interpreted as sources for filling these needs.

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[38] Lone Republicans in the remaining groups had slightly fewer friends than Republicans in these groups where like-minded partisans were available (although still more than Democrats); they demonstrated their partisan selectivity by disproportionate friendships with Independents over Democrats.

[39] Cartwright and Zander, *Group Dynamics*, argue that "public behavior is more likely to be conforming than private behavior" because of the greater likelihood of sanctions being invoked when deviance becomes known (p. 145). Some Wallace campaign activists, for example, reported in the media that during the 1972 presidential primaries there were numerous cases of individuals who covertly expressed support for Wallace but who could not be induced to participate in his campaign until they were persuaded that their "friends and neighbors" were for him too.

Since this was an *ex post facto* explanation of the findings, a large part of the analysis was devoted to testing hypotheses deduced from the suggested explanation in order to increase confidence in the original finding and the explanation. These hypotheses constitute a small set of interrelated propositions about the behavior of political deviants.

First, it was hypothesized that deviants tend to be isolates in their nonwork social environments and therefore utilize their work situation for political integration and self-evaluation. The supportive finding was that Republicans have fewer political discussions outside the work group and more inside the work group than Democrats.

Second, on the asumption that deviance becomes more problematic and anxiety-provoking as the opposing political norm becomes stronger, it was hypothesized that reactions to deviance increase with this condition. This hypothesis was supported by the finding that the strength of the relationship between party identification and friendship integration is more pronounced in membership groups where the pro-Democratic norm is stronger than in groups where the norm is weaker.

Since the second hypothesis assumes that membership groups serve as reference groups, it was further hypothesized that anxiety and dissonance resulting from deviance increase with subjective identification with the membership groups supporting the norm which the deviant opposes. Consistent with this hypothesis was the finding that friendship integration among Republicans (but not among Democrats) increases as a function of reference group identifications with pro-Democratic membership groups.

A fourth supported hypothesis was that deviants tend to choose other deviants for friends while attitudinal similarity has less effect on the friendship choices of conformers. The familiar relationship between attitudinal similarity and attraction therefore appears to result at least partially from threat potential from supporters of opposing viewpoints.

Finally, it was hypothesized that deviant status discourages the expression of political views unless the deviant has supportive social ties. Here it was found that among Republicans (but not among Democrats), political participation increases with friendship integration.

While Ns were small in several of the tests, the second and third hypotheses were operationalized in numerous ways and in several different subgroups. The consistency of the findings in all tests lends credence to the results in spite of the small subgroup sizes.

This paper has described several changes in the original hypothesis that integration in the primary work group would lead to support for pro-Democratic norms. First, the hypothesis was turned around causally—the independent variable became dependent and vice versa. Second, the explanatory variable, deviance, was introduced and its effects tested for in a variety of ways. In spite of these apparently extreme changes, however, the theoretical orientation remains the same, and no basic challenge is posed to the initial theory. The original hypothesis assumed that primary groups enforce political norms through group sanctions of rejection and isolation. The new hypothesis is still predicated on this notion, but it now begins with the fact of deviance and specifies that, in order to cope with their deviance and the rejection and isolation to which they are

Table 10. Friendship Integration and Political Participation, by Party Identification

A. Republicans

	Friendship Integration	
	None–Low[a]	Medium–High
Didn't Vote	17%	0%
Voted	83	100
	100%	100%
	(18)	(22)
Gamma = 1.0		

B. Democrats

	Friendship Integration			
	None	Low	Medium	High
Didn't Vote	10%	20%	7%	10%
Voted	90	80	93	90
	100%	100%	100%	100%
	(87)	(64)	(99)	(77)
Gamma = .10				

[a] Categories were combined because of small Ns. The "None" category included only 3 cases; of these one didn't vote. The "Low" category included 15 cases; of these two didn't vote.

subjected in *other* social groups, deviants become more integrated into their work groups. The major alteration, therefore, is the identification of the *outside* groups as the salient ones for group pressure rather than the *work* group, and the identification of the work group as a residual protective environment to which the isolate can retreat under stress. The same basic theories continued to guide the research, by suggesting appropriate tests for an explanation of the unanticipated finding.

Why is the work group the setting in which the affiliative needs of deviants can be fulfilled instead of being an additional source of pressure toward conformity with dominant norms? In part, it may be because of the limited real social importance of the work group for most of these workers, and the consequent limitation on its potential for influence. For example, while 77 per cent of the workers have at least one reciprocated friendship in the work group, only a quarter of them ever socialized with fellow workers outside of the plant. At the same time, workers spend more time at work than in any other social context. This makes the work group eminently available to serve affiliative or other social psychological needs that workers bring to their jobs.

More important is the work group's capacity for *political* influence. If this is limited, the group is not likely to serve as a socializing agent for the union and for the pro-Democratic political culture in general, as originally assumed. In retrospect, the extent of politicization of the work group is not impressive. In the midst of a very close presidential election campaign, in which their national and local unions were all active, only 58 per cent of the workers reported ever discussing politics with fellow workers. Of several hundred free-answer responses to questions asking for evaluations of the international and local unions, only ten used nonunion politics as a basis of evaluation, and seven of these were critical of the union's political involvement. Nor does the union's political posture win overwhelming approval on direct questioning. While most workers feel that it is "all right" for the union to work for laws favorable to union members, only slightly less than half approve the union's helping candidates and most disapprove spending dues money for this purpose. In general, the worker's view of the union's role is limited to the traditional bargaining, grievance, and personal service functions. Moreover, political penetration of the work group by the party and union elite is not particularly effective. While virtually all the workers reported speaking with their steward at least occasionally, less than 2 per cent reported that politics was a usual topic of conversation, and only 24 per cent reported a political contact by their steward during the presidential campaign. Aside from such infrequent campaign contacts, it seems doubtful that the union's political position is really salient to very many workers. Thus, because the work group is not a positive source of political norms, it can be penetrated with deviant normative positions.

The work group may also serve as a "contraculture"[40] within the UAW. Data not presented here suggest that the more cohesive the work group, the more alienated the members are from the union and the less satisfied they are with the union's efforts on their behalf. While alienation may well be just a reflection of the stronger Republican allegiances of well-integrated groups, the analysis suggests that the groups become cohesive because of these anti-Democratic and anti-union attitudes.

Implications

The findings reported here have potentially profound implications for political democracy. Concern with constitutional and procedural protection for the rights and expressions of views of political minorities may be of little practical consequence if basic social psychological processes act to discourage minorities from holding or expressing deviant views. The ways in which minorities deal with the social pressures for conformity are therefore of considerable importance.

Golembiewski has pointed out that "the group may function as a defense against the extra-group environment." In the event of threat, cohesive groups can stand and "fight," whereas unintegrated groups or isolated individuals "flee" or "disintegrate."[41] This is obviously a primary concern of "mass society" theorists, who stress the breakdown of interpersonal association and the consequent atomization of individuals in totalitarian societies.[42] Much experimental evidence indicates that subjects in friendship groups are better able to resist pressures for conformity than are subjects receiving support from strangers.[43] The present analysis, a study of natural groups, suggests that hypotheses based on laboratory research in this area may be useful for understanding real-world attitudes and behavior.[44]

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[40] J. Milton Yinger, "Contraculture and Subculture," *American Sociological Review*, 25 (October 1960), 625–635.

[41] Robert T. Golembiewski, William A. Welsh, and William J. Crotty, *A Methodological Primer for Political Scientists* (Chicago: Rand McNally and Co., 1969), p. 78, p. 81.

[42] See, for example, Kornhauser, *The Politics of Mass Society;* also Verba, *Small Groups and Political Behavior*, pp. 52–54.

The apparent irrelevance of tasks frequently used in experimental research (for example, the Asch situation) may discourage generalization of the findings to attitudes which have significance for political systems. A recent series of experiments demonstrating the operation of conformity pressures in tasks involving cruel interpersonal behavior makes the connection clear. Milgram has shown that the support provided by a social group may be vital to the individual's ability to withstand pressures for compliance with a destructive task. When instructed to administer supposedly severe shocks to an innocent person, the large majority of subjects comply in spite of cries of pain coming from the "victim." However, placing a subject in a group with two confederates who defy the experimenter reduces compliance very substantially. Among other reasons for this effect, Milgram reports that many subjects never even think of defying the experimenter until a confederate suggests it. The confederates also provide social reinforcement of the subject's belief that harming the innocent is wrong; and they give the subject moral support in defying the strong pressures to conform to the authoritative wishes of the experimenter.[45] All of these factors are related to the present discussion of the ways in which groups function to protect political deviants. That the group is necessary to support even a socially approved and deeply ingrained value points to its critical importance. Group support reduces needs to gain approval from others and therefore makes people less responsive to conformity pressures. On the other hand, isolation is a punishing experience which people try to avoid by conforming to group norms.[46]

This analysis suggests the importance of the immediate environment as a social context for political behavior. Obviously, Republicanism could not be considered deviant in many other social contexts or geographic areas. Similarly, there is temporal, as well as spatial, variation in normative contexts. Democratic support among workers such as these is much less substantial now than it was earlier. In many auto factory work groups in 1972, McGovern Democrats might well have been the political deviants. Further studies therefore need to utilize measures of deviance appropriate for the time and place studied; one would not necessarily expect that the particular indicators used in the present research would be valid measures of deviance and conformity today. A theoretical problem highlighted by the temporal changes in normative contexts is the need to understand more about how the political norms of social groups change over time, given the pressures for conformity discussed in the present study.

Casting this analysis in terms of a theory of deviance and conformity should not obscure the fact that the deviant views examined here are far from esoteric, unpatriotic, or in any way disparaged by the society at large. We are dealing with self-identified members of a mainstream political party that controlled the executive branch of the federal government for the eight years preceding the data collection. The difference between these workers and many other Republicans lies simply in their living and working in a political environment that was heavily Democratic. That this relatively minor party difference can have noticeable behavioral effects suggests the severity of the strains and the importance of the adjustive mechanisms used by real political deviants. Understanding them seems critical to an appreciation of the social psychological bases of pluralist societies.

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[43] See, for example, Nicholas P. Pollis and Joseph A. Cammalleri, "Social Conditions and Differential Resistance to Majority Pressure," *Journal of Psychology,* 70, 1 (September 1968), 69–76; and Vernon L. Allen and John M. Levine, "Social Support, Dissent and Conformity," *Sociometry,* 31, 2 (June 1968), 138–149.

[44] While the experimenter's ability to control and manipulate variables is enviable, one must not forget his concern that the artificiality of the laboratory situation may severely limit the generalizability of findings. See, for example, Robert R. Blake and Jane S. Mouton, "Conformity, Resistance and Conversion," in *Conformity and Deviation,* ed. Irwin A. Berg and Bernard M. Bass (New York: Harper and Brothers, 1961), esp. pp. 5–7 and 24–25. Replication of experimental findings in real-life situations is thus especially necessary.

[45] Stanley Milgram, "Liberating Effects of Group Pressure," *Journal of Personality and Social Psychology,* 1, 2 (February 1965), 127–134.

[46] Peter M. Blau, "Patterns of Deviation in Work Groups," *Sociometry,* 23, 3 (September 1960), 245–261.

Nonattitudes and American Public Opinion: The Examination of a Thesis

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The status of the individual American's concern about political issues has long intrigued political scientists. Two core questions of this interest are the degree to which Americans express beliefs about political objects, and the extent to which these expressions reflect "real" attitudes rather than interviewing artifacts or random responses. These problems of public opinion's substantiality must be solved to interpret correctly shifts in mass orientations to public policy, electoral behavior, the responsiveness of government, and, in a more general purview, the empirical validity of theories of participatory democracy.[1] In this paper we present a conceptual and methodological formulation for analyzing individual beliefs and recent generalizations about the quality of American public opinion.

Introduction

Students of public opinion have reached conflicting conclusions about the salience of specific issues for the American electorate. Several major research efforts contend that for many Americans, issue opinions for the most part are either nonexistent, randomly articulated, or supported by little information. To be sure, no one characterizes the entire electorate as a collection of political buffoons and dolts. Still, for many writers not only beliefs in democratic norms but also the conditions for democratic actions are met by only a minority of the electorate.[2] An early study, *Voting*, concluded that the "ordinary voter . . . votes the way trusted people around him are voting."[3] The authors of *The American Voter* wrote that there exists a "widespead lack of familiarity with prominent issues of public policy, along with confusion on party position that remains even among individuals familiar with an issue."[4] Another study reported that only about 7 per cent of a 1958 national sample had "any discernible issue content" in their stated reason for voting Democratic or Republican in a congressional election.[5] Converse concluded that only about 20 per cent of the mass public has true opinions on a specific issue involving government and private enterprise.[6] In the same vein as Converse, the Butler and Stokes analysis of British data indicated that over a relatively short time span "only 37 per cent stuck to an identical and definite position" on the question of nationalization of industry.[7] Moreover, "only 50 per cent were consistent in either supporting or opposing further nationalization over the three interviews."[8] Thus, the American voter shares with his British cousin an inability to meet many of the requirements of some theories of the democratic citizen.

The authors of the above studies have been careful in refraining from condemning all members of the American citizenry. Nevertheless, this care does not prevent other scholars from generalizing to the "average voter," while excluding only a small opinion elite. One recent report contrasted some of its findings with the prevailing view that

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[1] Two decades ago, Bernard Berelson assessed the state of knowledge about public opinion and related it to the demands of democratic theory. See Bernard Berelson, "Democratic Theory and Public Opinion," *Public Opinion Quarterly,* 16 (Fall, 1952), 313–330.

[2] For related studies examining the degree to which Americans believe in democratic norms see Samuel A. Stouffer, *Communism, Conformity and Civil Liberties* (Garden City, New York: Doubleday, 1955); James W. Prothro and Charles M. Grigg, "Fundamental Principles of Democracy: Bases of Agreement and Disagreement," *Journal of Politics,* 22 (Spring, 1960), 276–294; and Herbert McClosky, "Consensus and Ideology in American Politics," *The American Political Science Review,* 58 (June, 1964), 361–382.

[3] Bernard Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954), p. 309.

[4] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley and Sons, 1960), p. 188.

[5] Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, 1966), p. 199.

[6] Philip E. Converse, "Attitudes and Non-Attitudes: Continuation of a Dialogue" (Survey Research Center, The University of Michigan, November, 1963), p. 15. See also Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent,* ed. David E. Apter (Glencoe: The Free Press, 1964), pp. 238–245.

[7] David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martins Press, 1969), p. 178.

[8] Butler and Stokes, p. 179.

> the average citizen appears lamentably ill-informed about politics, unacquainted with the most basic and simple ideological distinctions, unwilling or unable to organize his ideas in a logical and coherent fashion, and, in many cases, apt to alter his positions on important political issues on a random basis.[9]

Likewise, Bernard Hennessy recently reviewed the literature on attitudes and belief systems in the American electorate and reached the following conclusion:

> What does this mean for the study of political opinions, attitudes, and belief systems? I submit it means we are naive and inconsequential if we waste time with mass belief systems and mass attitudes. Beyond the concern for some outer cultural limits to what elites may do, we should not try to investigate that which isn't.[10]

Based on methodological objections (and perhaps, partly in an ideological reaction to the findings reviewed above), others exhibit more confidence in the substantive and behavioral importance of individuals' opinions about political issues. While not always interpreted within the context of a "rational man" model, the net impact of some recent research efforts is to "rehabilitate" the American voter. V. O. Key, for example, in his posthumously published work, *The Responsible Electorate,* asserts that Americans demonstrate a laudable amount of issue orientation.[11] Key believed that, with appropriate analysis, "the proposition that the voter is not so irrational a fellow after all may become credible."[12] Likewise, in rejecting both the model of the "philosophical citizen" and the model of the "manipulated citizen," Pomper writes that "voters do not have broad factual knowledge or an integrated ideology, but they do react to the parties in terms of the group benefits they provide and their records in office."[13] At the same time, others have indicated a relatively high association between opinions on some political issues, such as Medicare and federal aid to education, and one's choice of political parties.[14]

A methodological revisionism is the source of several other favorable conclusions about the voter. RePass observed that through the use of a different method, "We have shown that the public is in large measure concerned about specific issues, and that these cognitions have a considerable impact on electoral choice."[15] RePass also found that members of the electorate perceive a fairly large number of issues with which the government is concerned and that there is "considerable party-relatedness of vote based on specific issues."[16] Thus, he concludes that "voters seem to be acting more responsibly than had previously been thought."[17] Likewise, in a recent examination of "short-term fluctuations in U.S. voting behavior," Kramer claimed that

> one basic finding to emerge from this study is that election outcomes are in substantial part responsive to objective changes occurring under the incumbent party; they are not "irrational," or random, or solely the product of past loyalties and habits, or of campaign rhetoric and merchandising.[18]

In an even more emphatic challenge to earlier conclusions, Fishbein and Coombs employ what they label as an "explicit psychological theory," to "throw down the gauntlet before such an imposing mass of accumulated evidence."[19] Indeed, Fishbein and Coombs's conclusions present the voter in a much more laudatory light than do those of many previous works.

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[9] Paul M. Sniderman and Jack Citrin, "Psychological Sources of Political Belief: Self-Esteem and Isolationist Attitudes," *The American Political Science Review,* 65 (June, 1971), 415.

[10] Bernard Hennessy, "A Headnote on the Existence and Study of Political Attitudes," in *Political Attitudes and Public Opinion,* ed. Dan D. Nimmo and Charles M. Bonjean (New York: David McKay Company, 1972), p. 36.

[11] V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966).

[12] Key, p. 8.

[13] Gerald M. Pomper, *Elections in America* (New York: Dodd Mead & Company, 1968), p. 92.

[14] See, for example, William H. Flanigan, *Political Behavior of the American Electorate* (Boston: Allyn and Bacon, Inc., 1968), p. 62; and Frank J. Sorauf, *Party Politics in America* (Boston: Little, Brown and Company, 1968), p. 163, for general discussions. Also see the findings in Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *The American Political Science Review,* 63 (December, 1969), 1083–1105; Richard W. Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *The American Political Science Review,* 66 (June, 1972), 429–449; and Gerald M. Pomper, "From Confusion to Clarity: Issues and American Voters, 1956–1968," *The American Political Science Review,* 66 (June, 1972), 415–428.

[15] David E. RePass, "Issue Salience and Party Choice," *The American Political Science Review,* 65 (June, 1971), 400.

[16] *Ibid.,* 400.

[17] *Ibid.*

[18] Gerald H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964," *The American Political Science Review,* 65 (March, 1971), 140.

[19] Martin Fishbein and Fred S. Coombs, "Basis for Decision: An Attitudinal Approach Toward an Understanding of Voting Behavior," paper delivered at Sixty-Seventh Annual Meeting of the American Political Science Association (Chicago: September 7–11, 1971), p. 2.

This brief survey demonstrates that questions of the reasonableness of American voters, and the degree to which their orientations to politics include relevant and consistent beliefs, lack consensual answers. To be sure, it is a simplification to create two camps—one condemning and the other congratulating the "average voter." Yet, two diverging interpretations of the mass public *are* found in current scholarly work. While the most recent studies generally point to an upgrading or rehabilitation of the voter, such conclusions are based on data and methods distinct from those employed in the earlier studies they challenge. Moreover, several scholars, notably Rusk and Weisberg, have attempted to reconcile the two sets of studies by hypothesizing that the public itself has changed its issue-relevant behavior.[20] This argument is attractive on several grounds, for the late 'sixties indeed appear more issue-dominated than the late 'fifties, but it does not resolve disagreements about the extent of public opinion in the earlier period.

The resolution of that disagreement about public opinion is essential. The appropriate measurement of the extent of true attitudes in the population is a problem which must be solved *prior* to examination of the causes, consequences, and interrelationships among attitudes. If attitudes are found only occasionally in the population, analyses of the causes or behavioral consequences of the attitudes can be conducted only after a determination of which individuals have true attitudes and which do not. In this area, the widely cited work of Philip Converse is particularly important. As we noted above, Converse maintains that true attitudes are scarce—often held by a small minority of the public. In this paper, we reformulate the questions asked by Converse in his publication "The Nature of Belief Systems in Mass Publics."[21] Unlike most other reinterpretations of the voter, ours uses data identical to that employed by Converse—the results of the 1956–1958–1960 Survey Research Center panel study of the American electorate. We discuss several limitations in present conceptualizations of the problems of belief measurement and belief stability and the resulting methodological complications which they underpin. The panel data are then reevaluated in terms of the stability and randomness of beliefs through time.

Attitudes and Attitude Consistency

An attitude is a "predisposition to respond in a particular way toward a specified class of objects."[22] The attitude or underlying predisposition itself is difficult to measure directly, so we appreciate Rosenberg and Hovland's concept of three classes of measurable responses manifesting the attitude: the affective, the cognitive, and the behavioral.[23] Respectively, these responses refer to: the emotional orientation toward the object (pro-con); the perceptions and beliefs about the object; and the actual behavior toward the object.

The consistency of individual attitudes as indexed by affective responses is the subject of this study. Two types of consistency are considered: longitudinal and organizational. *Longitudinally*, attitude consistency refers to the stability through time of the predisposition's measured manifestation. A consistent attitude at any one point in time is one in which the affective, cognitive, and behavioral components are *well-organized* and stable, or in which separate attitudes toward closely related objects take the same direction. Attitudes which manifest an internal inconsistency among the three components are unstable and highly susceptible to internal reorganization and change.[24] We hypothesize, like many other writers, that the individual has an aversion to inconsistency; internal inconsistency creates cognitive tension which may move the individual to reorganization of one or more of his attitudinal elements.[25]

The individual's penchant for consistency is not without its limits. Several attitude theories and their derivative studies demonstrate that people can tolerate some inconsistency both within and between related attitudes.[26] The amount of internal inconsistency an individual will tolerate before one of the components must change depends on whether the level of inconsistency—the amount of cognitive tension it has produced—has gone beyond that individual's tolerance threshold. In turn, the tolerance threshold one possesses is determined by the interaction of social, psychological, and logical constraints under which he operates.

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[20] Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *The American Political Science Review*, 64 (December, 1970), 1167–1185.

[21] Converse, "The Nature of Belief Systems in Mass Publics."

[22] Milton J. Rosenberg and Carl I. Hovland, "Cognitive, Affective and Behavioral Components of Attitudes," in *Attitude Organization and Change*, ed. Carl I. Hovland and Milton J. Rosenberg (New Haven: Yale University Press, 1960), p. 1.

[23] Rosenberg and Hovland, p. 3.

[24] Milton J. Rosenberg, "An Analysis of Affective-Cognitive Consistency," in Hovland and Rosenberg, pp. 15–63.

[25] See the general review by Robert B. Zajonc, "The Concepts of Balance, Congruity, and Dissonance," in *Attitude Change: The Competing Views*, ed. Peter Suedfeld (Chicago/New York: Aldine-Atherton, 1971), p. 66. See also: Milton J. Rosenberg, Sidney Verba, and Philip E. Converse, *Vietnam and the Silent Majority: The Dove's Guide* (New York: Harper and Row, 1970), p. 86; and Leon Festinger, *A Theory of Cognitive Dissonance* (Stanford, California: Stanford University Press, 1957), p. 3.

Consequent to the above framework, apparent inconsistency in the form of longitudinal variations in one of the elements of the attitude (the behavioral, for example) may reflect temporal stimuli, but the resulting inconsistency may be insufficient to bridge the tolerance threshold and bring reorganization of the attitude. That is, through time the basic underlying attitudinal predisposition remains unchanged even though variations in one of its observable components have occurred. If party vote is considered the behavioral component of the partisan attitude, this conceptualization is akin to the attitudinal processes underlying Converse's notion of the "normal vote." In discussing the influence of short-term forces, Converse notes that "the hallmark of the short-term partisan force is, of course, that it induces defections across party lines, yet defections which are unaccompanied by any underlying revision of party loyalty."[27] In this case, partisan feelings remain stable while behavior varies. Likewise, as this paper demonstrates, variations in expressed affect through time therefore may not indicate inconsistent or random attitude expression, but may still be located logically and psychologically within the tolerance range of consistent attitudes. Therefore, considering the crudeness with which instruments used in mass surveys allow for the individual's ability to differentiate his own affect, plus the normal variations in affect, one must be extremely cautious in explaining opinion movement as real attitude change or as an underlying absence of attitudes. Fluctuations in responses (but not in attitudes) may be due to: (1) variations in the expression of affect reflecting temporal influences which (a) do not raise the level of inconsistency over the threshold and (b) continue to reflect the basic underlying predisposition; and, (2) instrument-related variations reflecting both the instrument's and the individual's inability to discriminate the individual's precise affect.

The above conceptualization is important when measuring and evaluating attitudes and attitude change with the focus on attitudinal affect. The affective component generally is assessed through the responses of an individual to a stimulus in the form of a questionnaire item. The measured affective response becomes a product of (1) the basic underlying attitudinal predisposition of which affect is but one of the three components; (2) the short-term, temporal influences, of which there are several types (psychological, social, physical) which may contribute to a dispersal of the response around the position of the real attitude; and (3) the ability of the measuring instrument to provide the individual with a means of differentiating his affect for the attitude object to a degree which is congruent with his internal ability to differentiate. The latter potential for congruity is particularly important because the differentiation allowed by the response stimulus (the interview item) remains constant for all individuals, while the ability and proclivity to differentiate varies across individuals.

In sum, the above conceptualization provides a basis for demonstrating that longitudinal variations of opinions need not indicate either random articulation of nonexistent attitudes or real attitude change. Instead, such movement is acceptable within the bounds of what is known about the structure of individual attitudes. The remainder of the paper contains: (1) a discussion of the methodology involved in opinion measurement; (2) a statistical and mathematical formulation for recovering the degree to which expressed opinions may deviate from the true attitude without indicating random attitudes or attitude instability; and (3) the application of the conceptual and statistical models to the SRC panel study to test the models' ability to interpret and estimate longitudinal opinion distributions.

Attitude Measurement

Differentiation and Variation. Whether members of the public give vague or precise responses to opinion questions depends partly on the questionnaire form. Forms which permit only dichotomous responses (as in agree, disagree) allow only minimal differentiation, while those allowing interval responses (as in an attitudinal thermometer) facilitate highly differentiated responses. In principle, at least, members of the public possess relatively specific beliefs; in attitude studies these specific beliefs become grouped together, and the extent of grouping depends on the responsiveness of the questionnaire item. The less responsive and differentiated the questionnaire item, the more the grouping together of distinct beliefs into a single category, increasing the measured consensus among the respondents.

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[26] Rosenberg, "An Analysis of Affective-Cognitive Consistency," p. 22; and, Festinger, *A Theory of Cognitive Dissonance*, p. 16.

[27] Philip E. Converse, "The Concept of the Normal Vote," in Campbell et al., *Elections and the Political Order*, p. 15.

An infinitely differentiated response scale could not, however, place accurately any members of the public. For every individual there are psychological and cognitive limits beyond which no differentiation occurs. The ability to differentiate varies from individual to individual, from time to time, and from topic to topic.[28] The amount of information possessed, the intensity with which the attitude is held, the immediacy of the attitude object to the present situation, and the degree to which this attitude is attached to other attitudes are among the forces constraining the degree of differentiation.[29] Thus, for an attitude scale with infinite gradations, no individual can be placed more precisely on that scale than his own level of differentiation, so every respondent's own attitudinal position necessarily is a *range* on that scale. The ranges on the infinitely gradated scale vary in width and location—depending on the individual and the attitude object—but each attitude would be a range and none would be a specific point on the scale.[30] Yet, in most conventional analyses individual opinions are treated as *discrete points* on a scale rather than as a belief range.[31] This treatment has important implications for interpreting long-term attitudinal consistency and change.

On most recurring, important public questions, individuals have more than one response (opinion) which they are able to give in an interview situation. The interviewer's age, sex, social class and personality all may influence the respondent's articulated opinion. While under certain circumstances these interview influences can be minimized, with a large and diverse sample they cannot be eliminated. In dealing with these effects we can standardize them or randomize the potential error, but we cannot erase the error. In addition to the interview environment, other social and psychological factors sway the response: recent public events creating issue saliency, influence of friends, emotional changes, and the family's economic condition. The analyst may have no direct control over these social-psychological influences on the expression of the individual's belief, although he can expect most of them to be random across his entire sample.

Any given articulated opinion may reflect a distorted image of the real and stable underlying attitude (the range on the infinitely graduated scale).[32] Our earlier conceptualization illustrated how these expressed variations are psychologically tolerated within a relatively well-organized attitude. The implication is that the response variation does not indicate random, unstable, or wavering attitudes, but rather normal variations in the expression of the underlying stable attitude, none of which need create internal inconsistency sufficient to initiate the reorganization or change of the attitude.

Every opinion sample in fact consists of two samples: a sample of a population (population sample) and a sample of potential responses on a topic within each individual (response sample).[33] The response sample, as the popu-

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[28] The problem of matching the "opinion surveyor's" response categories with the "individual's own categories for evaluation" is discussed in Carolyn W. Sherif, Muzafer Sherif, and Roger E. Nebergall, *Attitude and Attitude Change* (Philadelphia: W. B. Saunders and Company, 1965), chapter 4.

[29] *Ibid.*

[30] This conception of an attitude as a range on a dimension is akin but not identical to the latitudes of acceptance and rejection in Sherif, Sherif, and Nebergall, pp. 24–25.

[31] Technically, only people whose ranges cross the boundaries of question response categories are treated as having discrete point attitudes; because, however the categories themselves can be shifted with regard to referent attitudes, in effect all individuals are treated as having attitudes in the form of discrete points.

[32] The opinion response may not correspond to the attitude a researcher wishes to tap because the question and the respondent's attitudes are at different levels. The question may be more precise or more general than the attitudes; characteristically, such questions require the respondent to employ several attitudes in forming a response. For instance, a Goldwater conservative might have difficulty responding to the school integration question posed in SRC surveys, for it requires him to respond to federal government intervention (negative) and school integration (positive) in simple agree/disagree fashion. Over repeated trials, such questions which in effect create temporary cognitive dissonance may produce inconsistent responses, if the attitudes are of relatively equal importance to the subject. Asking pacifists if they would defend their wife or mother from a rapist is a popular form of this pastime. We expect that for such items, the question responses are not a good index of the attitude as it would be exemplified in the respondent's behavior if the hypothetical situation confronted him.

[33] Harry S. Upshaw, "Attitude Measurement," in *Methodology in Social Research,* ed. Hubert M. Blalock and Ann Blalock (New York: McGraw-Hill, 1968), pp. 80–81, notes that Thurstone, in his development of "The Law of Comparative Judgment,"

> assumed that each presentation of a stimulus arouses in the respondent some undesignated perceptual process which is, in principle, quantifiable. Furthermore, he assumed that the perception of a particular stimulus on any given presentation is distorted somewhat by random and independent factors associated

lation sample, varies in validity. Greater response differentiation increases validity—as does giving "each" of a respondent's potential answers an equal chance of being selected by using unbiased questions and interview procedures. Even so, many of the normal forces which influence response selection remain unmeasured and uncontrolled. Therefore, a single item on a single interview constitutes an unreliable sampling of an individual's opinion.[34]

Even if the sample of opinions within each individual is not reliable because of response error ('error' here merely indicates non-attitude aspects of the response), the population sample of opinions still may be reliable if the individual sampling error is distributed randomly across the population (that is, the marginal scores are accurate while the individual scores are not). An unbiased interview will distribute randomly across the population the causes of errant response selection.

SRC Panel Study: A Critique. Prior to the SRC panel study of 1956–1958–1960, longitudinal analysis of opinions could be conducted only by coarse methods—examination of net change in population marginals. The marginal change usually was small, and it was assumed that the gross change for individuals was not much larger. The internal patterns from the 1956–1958 panel responses contradicted this assumption: net marginal change was small only because while many individuals gave different responses in the two samples, those differences tended to cancel out across the population. Thus, for example, party identification, one of the most stable items, had a 12 per cent (marginal) change, but 39 per cent of the respondents had actually changed their responses from 1956 to 1958.[35]

The 1960 panel results were even more startling. Again, the change was widespread and multidirectional, and the change of individuals also was multidirectional. Through the three sets of interviews there was about as much agreement between the 1956 and 1960 responses on party identification (57 per cent the same) as between the 1958 and 1960 responses (62 per cent the same). It would be difficult to delineate a series of public events which would in any simple way account for the particular and peculiar pattern of change.

It is possible that the above findings indicate a great deal of rational attitude movement in accordance with important external factors. Such an interpretation, however, runs counter to other findings which indicate that most voters simply do not have the information or the interest to engage in that sophisticated a behavior pattern. Indeed, as we have noted, Converse pointed out extensive, formless response variation.[36] Converse suggested that the change stemmed largely from nonattitudes, not from attitude change. He demonstrated that response variation increased as question items moved away from the central, popular political topics—such as parties—toward more remote issues—such as the public ownership of utilities.[37] The items on which the public generally manifested sufficient information and interest to support an approximation of rational choice and rational change showed the least variation; consequently, the variation in all probability

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with the experimental procedures and with the respondents. Because the assumed distorting factors are random and independent, the perceptual processes aroused by the repeated presentations of a given stimulus would, if quantified, be distributed normally. If this hypothetical distribution were available as data, its central tendency would be taken as the best estimate of the true position. . . .

The same randomizing process may apply to the expression of the true attitude in response to that perception.

[34] An alternative means which has been employed in determining attitudes from a single interview is the correlation of separate items. For some of the problems in this method, see our discussion of attenuated correlations presented in the "implications" section of this paper.

[35] The source for these data is the authors' analysis of the SRC panel data. This computation includes only those interviewees who expressed an identification in both samples. For subsequent computations we omit nonsubstantive responses. The panel data were collected by the Survey Research Center at the University of Michigan and made available through the Inter-University Consortium for Political Research. Neither the SRC nor the ICPR are responsible for the analysis and interpretations presented in this paper. The proportion stable varies with the accounting procedure. If all codes are counted, 59 per cent of the total panel is unstable on party identification in 1956–1958. If noninterviewed respondents are dropped, then unstability is 45 per cent. When, additionally, the sample is reduced by omitting "don't know," minor parties, apoliticals, etc., the unstability remains 45 per cent. When the remaining respondents are limited to five categories by ignoring the leanings of the independents —and this is the calculation reported in the text— then 39 per cent are unstable. If the "strong-weak" distinction for partisans is ignored, then only 20 per cent are unstable, i.e., shift between the broad Democrat-Independent-Republican categories. If respondents who at either time answer "Independent" are dropped, the unstability (Democrat, Republican) is limited to 5 per cent. By this point, however, most of the sample and categories have been omitted. The party identification turnover tables are presented on the bottom of page 632.

[36] Converse, "The Nature of Belief Systems in Mass Publics," and Converse, "Attitudes and Non-Attitudes."

[37] Converse, "The Nature of Belief Systems in Mass Publics," p. 241.

was not real attitude change. According to Converse, lacking true attitudes, many individuals must have responded randomly in all three interviews, thus producing the low correlation of the responses through time.[38]

In testing his nonattitude hypothesis, Converse found a near perfect fit to what he called his "Black-and-White" model on only one issue. Nevertheless, several conclusions he proposes indicate his belief in the model's more general applicability, e.g.:

> The substantive conclusion imposed by these technical maneuvers is simply that large portions of an electorate do not have meaningful beliefs, even on issues that have formed the basis for intense political controversy among elites for substantial periods of time.[39]

The deviations from the pure model which occur on the other issues are taken to be the consequence of a "third force," i.e., those people who undergo meaningful attitude change. Yet "this third force is small. . . ."[40] Consequently, in the following discussions we present a general critique of that model and propose an alternative model which fits the data and generates conclusions contrary to those of the "Black-and-White" model.

Converse's arguments against accepting the multidirectional variations of 30 to 60 per cent of the population as true attitude change are valid. The interpretation of this variation as random response, however, is less acceptable on three counts: (a) the methodology is questionable, (b) the interpretation does not account for the observed results, and (c) an alternative hypothesis without equivalent deficiencies gives a more complete account of the responses.

Converse's methods are an insufficient basis for accepting his thesis because they do not lead to an explanation of responses, they do not exclude alternative accounts, and they do not seem to be replicable. Unless behavior itself is ultimately random, a random model cannot explain behavior and it can be consistent with many different explanations. For ex-

Party Identification Turnover Tables: 1956–1958, 1956–1960, and 1958–1960*

1956	1958 Strong Democrat	Weak Democrat	Independent	Weak Republican	Strong Republican	Total	*N*
Strong Democrat	16.7%	4.2	.3	.1	.3	21.6	231
Weak Democrat	7.9	12.5	2.1	.5	0	23.0	245
Independent	2.1	4.7	13.3	3.2	1.5	24.8	264
Weak Republican	.3	1.2	2.2	8.7	2.5	14.9	160
Strong Republican	.5	.4	1.0	4.6	9.4	15.9	170
Total	27.5%	23.0%	18.9%	17.1%	13.7%	100.2	
N	293	246	202	182	147		1070
1956				1960			
Strong Democrat	15.2%	5.7	1.0	.1	.2	22.2	257
Weak Democrat	7.0	11.8	3.2	.6	.3	22.9	265
Independent	2.2	4.5	13.5	3.3	.9	24.4	282
Weak Republican	.3	1.6	2.8	6.9	3.4	15.0	174
Strong Republican	.4	.3	1.6	3.4	10.0	15.7	182
Total	25.1%	23.9%	22.1%	14.3%	14.8%	100.2	
N	290	277	257	165	171		1160
1958				1960			
Strong Democrat	18.0%	7.2	1.2	.2	.2	26.8	353
Weak Democrat	5.5	13.4	3.6	.7	.2	23.4	308
Independent	1.2	3.3	12.2	2.1	.8	19.6	258
Weak Republican	.4	.5	3.0	9.1	4.3	17.3	227
Strong Republican	.2	.2	.9	2.8	8.9	13.0	170
Total	25.3%	24.6%	20.9%	14.9%	14.4%	100.1	
N	333	324	276	196	187		1316

* The entries in each cell are the percentages of the sample placed at the intersection of the identifications for the two years. Only individuals responding in both surveys with an identification are employed in the presentation.

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[38] If individuals' opinions reflect a response set, then the responses are not random, for they represent a predisposition to respond and one which is consistent across attitude objects. Also, see Appendix II for a discussion of response set as it applies to the panel study. Response set as it is discussed here is not an agreement bias, but rather a "strong" bias and hence will not influence Converse's results. The only impact such a conceptualization will have on the model we subsequently present will be to increase the potential for error, rather than minimizing it.

[39] Converse, "The Nature of Belief Systems in Mass Publics," p. 245.

[40] *Ibid.*, p. 244.

ample, the "Black-and-White" consistency test is used in exactly the same form by Philip Converse to demonstrate the *lack of attitudes* and by James Coleman to demonstrate the presence of *attitude and measurement error*.[41] In Converse's approach, the main variable—attitudes—is operationalized as a residual category (what is left after other variables have been accounted for), thus minimizing the number of responses counted as evidence of attitudes; alternative operationalizations of the same attitude concept with the same data can considerably increase the number of responses counted as attitudes. As Converse neither specifies his other variables nor justifies theoretically the treatment of attitudes as residues, alternative accounts of the data producing different conclusions are just as acceptable as Converse's work. The widespread acceptance of Converse's conclusions is particularly curious in view of the limited applicability of his methods. The attempts to replicate his test on other questions failed, and the operating criterion for the appropriateness of attempting replication appears to be: if the test shows nonattitudes, it should be used; if not, it is inappropriate. We therefore conclude that Converse's nonattitude hypothesis cannot be accepted on the basis of the methods he presents.

The second major drawback of Converse's method lies in the failure to fit the data in four basic ways. (1) The Black-and-White model does not account for the responses on any question in any single year. Converse estimated that 31 per cent of those taking sides on the public power issue have attitudes.[42] The remaining respondents answer randomly. We find, however, that they do not respond equally among the four available categories: less than 9 per cent of the sample respond "weakly agree" in 1960. Even if this entire group is answering randomly, an equal distribution of random responses would indicate that 64 per cent, not 31 per cent, of the sample taking sides has attitudes. The Black-and-White model does not present variables accounting for the scarcity of "weakly agree" responses. The inclusion of such variables, e.g., response set, would hamper the predictions in other respects (see our Appendix II).

(2) The response variation over times does not fit the Black-and-White model in two respects: variations are incremental rather than random, and they indicate an underlying response continuum rather than dichotomized (or four-fold) response groups. Table 1B, which gives the ratio (multiplied by 100) of observed to expected frequencies for responses in 1958 and 1960, shows an incremental pattern of variation, with the strongest associations along and near the main diagonal, e.g., "depends, depends" responses, falling gradually down to infrequent associations furthest from the diagonal, e.g., "strongly disagree, strongly agree." When, as in Converse's Black-and-White model, the "depends" responses are omitted and the "strong" and "weak" responses collapsed (see Table 1C), the incremental variations and underlying continuum are hidden. Converse's transformation and recoding of the responses have several unfortunate effects. While the collapsing of categories admirably acknowledges that attitudes are ranges and not points, it implies that the only ranges are within "agree" and "disagree" categories, which Table 1B demonstrates to be incorrect. Also, while grouping increases the number of responses counted as stable, in this case it increases even more the number of stable responses counted as random in the Black-and-White model.[43] Thus, collapsing leads to an

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[41] James S. Coleman, "The Mathematical Study of Change," in Blalock and Blalock, *Methodology in Social Research*, pp. 453–456.

[42] Converse, "Attitudes and Non-Attitudes: Continuation of a Dialogue," p. 15; and Converse, "The Nature of Belief Systems in Mass Publics," p. 245. In the Black-and-White model, the total sample of 1514 is divided as follows: no interview exists at one or more trials for about 420 respondents, so these are excluded, leaving a base of about 1100 respondents. Another 80 respondents always reply "no opinion" or "don't know" and are counted as not having attitudes; additionally, about 420 respondents who give both substantive and "no opinion" or "don't know" responses are counted as not having attitudes, leaving a group of about 600 respondents consistently giving substantive responses; of these, about 140 at some time say "depends" and are thus counted as nonattitude respondents. The remaining 460 or so respondents—those taking sides on the issues in all three trials—are analyzed by the Black-and-White model into a group of about 250 without attitudes and a group of somewhat more than 200 assumed to have attitudes. Thus, of the base group of about 1100, less than 20 percent are estimated by Converse to have attitudes. There are a number of other ways of dividing up the sample, and for each of these Converse's percentage estimates must be recast, though the basic calculations remain constant. Of the 1038 respondents who take sides in 1960, at most 275 have attitudes in the Black-and White model, yet these "attitudes" are insufficient to explain the vast marginal differences, so the surplus of "strongly agree" responses (over half) and the shortage of "weakly disagree" answers (9 per cent) remain unexplained.

[43] For instance, when the full five categories are used, as in Table 1B, stable responses are 141 per cent of the expected (marginally) stable responses, while with a two category collapsed table (1c), stable responses are only 127 per cent of marginally expected stable responses; one-third of the extra stability has been removed by collapsing categories. For random expectations not based on the marginals, grouping has the same effect of discounting stable responses. In the full

Table 1. Cross-Tabulations of Opinions on Federal Ownership of Electric Power in 1958 and 1960

A. Complete Cross-Tabulation and Raw Figures

1958 Response	1960 Response									
	Strongly Agree	Weakly Agree	Depends	Weakly Disagree	Strongly Disagree	Sub-Total	NI[a]	DK[a]	NO[a]	Total
Strongly Agree	247	39	23	22	52	383	28	0	43	454
Weakly Agree	77	33	16	11	17	154	6	1	23	184
Depends	35	13	13	4	12	77	6	2	12	97
Weakly Disagree	27	20	15	16	21	99	1	1	14	115
Strongly Disagree	46	12	16	22	74	170	12	0	21	203
Subtotal	432	117	83	75	176	883	53	4	113	1053
NI	31	16	1	5	18	71	18	0	22	111
DK	6	2	0	0	2	10	0	1	6	17
NO	71	41	10	13	33	168	8	8	149	333
Total	540	176	94	93	229	1132	79	13	290	1514

[a] NI is "no interview," DK is "don't know," and NO is "no opinion."

B. The Ratio of Observed to Expected Frequencies[a]

1958 Response	1960 Response[b]					
	Strongly Agree	Weakly Agree	Depends	Weakly Disagree	Strongly Disagree	*N*
Strongly Agree	132	77	65	67	68	(383)
Weakly Agree	102	161	111	84	55	(154)
Depends	93	127	180	61	79	(77)
Weakly Disagree	56	152	162	191	107	(99)
Strongly Disagree	56	54	100	152	219	(170)
N	(432)	(117)	(83)	(75)	(176)	(883)

[a] The entries are the ratio of observed to expected frequencies multiplied by 100. The expected frequencies are obtained from the marginals.

[b] Only respondents with opinions in both waves of the panel are included.

C. Ratio of Observed to Expected Frequencies Using Collapsed Agree and Disagree Categories

1958 Response	1960 Response		
	Agree	Disagree	*N*[a]
Agree	117	64	(498)
Disagree	65	175	(238)
N[a]	(501)	(235)	(736)

[a] The column and row totals include only individuals with agree or disagree responses in the two samples and, therefore, do not equal the sum of the entries in Table 1A.

underestimate of stable responses and the corresponding number of attitudes.

(3) The third difficulty of the nonattitude approach in accounting for observed results stems from the coherence of the nonattitudes." If they are random, the nonattitudes ought to be both unstable over time and unintegrated with responses on related topics; in fact, however, the nonattitudes are both stable and integrated.

In the consistency test, panelists providing

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five by five table, anywhere from 100 to 254 responses can be counted as random, depending on the admission of "special" explanations for unequal numbers of responses in the unstable cells; even with special explanations, there are more unexplained stable responses in the full table than in the collapsed two by two table, so the net effect of grouping responses is to count more stable responses as nonattitudes.

Table 2. Tau-Beta Associations of 1958–60 Responses for Respondents Unstable from 1956–58[a]

Issue	Tau-Beta	N
School Integration	.28*	(418)
FEPC	.26*	(382)
Job Guarantee	.22*	(459)
School Aid	.22*	(438)
Public Power	.17*	(348)

[a] The tau-betas are computed from the five-by-five cross tabulations of responses in 1958 and 1960 for those respondents answering differently in 1956 and 1958.

* Significantly different from zero at the .00005 significance level. We also find, with 71 per cent confidence, that stable and unstable responses to the public power question are independent samples of the same population of attitudes.

different responses in consecutive samplings are considered to have no attitude, while those who give the same responses encompass all the individuals with stable attitudes.[44] Thus, on a third sample, the unstable group should answer randomly. As Table 2 illustrates, many in the unstable group are not answering randomly.

The unstable respondents have coherent attitudes by a second index: the degree to which their responses on one topic are related to their responses on another topic. If individuals in the unstable group were answering randomly, their answers should have only a chance relationship with other variables; the stable group, if it possesses attitudes, would more probably integrate these attitudes with other attitudes. While all respondents do not integrate their attitudes along identical dimensions, some dimensions are shared widely enough to test for the predicted stable, unstable differences in response integration. Table 3 presents a few of the many possible tests.[45] The reader can see that the unstable group hardly is bereft of coherent sets of responses.[46] For the nonattitude approach, the stability criterion is the key means of distinguishing attitudes from nonattitudes. Yet this criterion does not separate the population into groups which differ on the prime characteristics of true attitudes, their persistence through time and their integration or constraint with other attitudes. The parity of coherence of the nonattitudes when compared to the attitudes remains unexplained.

(4) Fourth, the Black-and-White model states

Table 3. The Association Between Pairs of Responses in a Single Year: Taking into Account the Stability of Responses on One of the Variables in Two Consecutive Samplings[a]

Stability Variable	Associated Variables	Stable Respondents	Unstable Respondents	Probability of Difference
FEPC 1956–1958	FEPC 1958 Job Guarantee 1958	.26***	.22***	.27
FEPC 1956–1958	FEPC 1958 School Integration 1958	−.44***	−.28***	.69
School Integration 1956–1958	School Integration 1958 Job Guarantee 1958	−.10**	−.04*	.50
N's range from 412 to 457.				

[a] The cell entries are to be read as the tau beta association between two responses among individuals who had the same opinion on one of the issues in the two years and those who had a different response in the two years. Thus, the top row should be read as "a tau beta of .26 was found between 1958 FEPC and 1958 Job Guarantee responses among those with identical FEPC responses in 1956 and 1958, while a tau beta of .22 was found between 1958 FEPC and 1958 Job Guarantee responses among those who had different responses on FEPC in 1956 and 1958."

* Different from zero at the .32 level of significance.

** Different from zero at the .02 level of significance.

*** Different from zero at the .00005 level of significance.

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[44] By chance, some individuals will give the same response twice; thus some respondents without attitudes fall into the stable group and the size of this group is, according to Converse, larger than the size of the attitude-holding population. See Converse, "The Nature of Belief Systems in Mass Publics," p. 259 (footnote 39).

[45] The presented data are neither the most significant nor the least significant findings. They are a cluster of three variables for the same time period simply to indicate that they are not isolated cases.

[46] Converse, in "The Nature of Belief Systems in Mass Publics," p. 228, employs a similar correlational measure to compare attitudinal constraint among an elite sample and a mass sample.

that "attitude" and "nonattitude" respondents have completely different bases for their responses. This proposition can be tested by comparing the two sets of respondents across the five domestic policy questions. Dichotomizing responses into "agree" and "disagree" and estimating nonattitude and attitude groups on the basis of the number of respondents changing sides at one or more of the three trials for the five questions, we find that on at least one major characteristic—the proportion of respondents giving predominantly "agree" responses—the "attitude" and the "nonattitude" groups are from the same population. For all five questions, the Pearsonian product-moment correlation coefficients (r) between the predominance of "agree" responses among respondents taking different sides (nonattitudes) and (a) the total predominance of "agree" responses and (b) the predominance of "agree" responses among "attitude" respondents are .954 and .993. Contrary to the assumption of the nonattitude approach, there is not one chance in a thousand that the "nonattitude" responses are unrelated to the "attitude" responses. Both sets of respondents are part of the same population, replying to the same question, with the same basis for responding and with responses indicating similar attitudes.

To summarize, the nonattitude model leaves unexplained: (1) the non-random distribution of responses, (2) the relative stability of responses, (3) the coherence of attitudes among those assumed to be responding randomly, and (4) the shared patterns among consistent and discrepant sets of responses. The notion of stable firmly fixed *responses* fares no better. The hypothesis of widespread attitude change also seems implausible. What remains, then, are attitudes, relatively stable, related to other attitudes, infrequently changing in a systematic fashion, but with responses manifesting many variations. In two ways these response variations are systematically misconstrued in Converse. Variations of response within the broad "agree," "disagree" categories are presented as stable, which they are not; all other response variations are represented as random, which they are not. Instead, variations in responses can be attributed to the problems of congruent opinion discrimination on the part of the individual and the measuring instrument, and temporal influences; moreover, they are consistent within the individual's attitudinal organization as long as they remain within the amount of tolerance the individual's own threshold allows. That is, variations may be evidence of flexibility within a certain attitude range, rather than either real attitude change or merely random responses.[47]

In the following sections we present a model for the empirical testing of our conceptualization and then apply it to the data at hand. This presentation is designed to demonstrate the third general weakness of the nonattitude approach we noted earlier, namely, that an alternative hypothesis without equivalent deficiences gives a more complete accounting of the responses. If our alternative model explains the data better than does the nonattitude hypothesis and if it is free of methodological objections, the nonattitude hypothesis ought to be rejected in favor of the alternative presented here.

An Alternative Model

The problem of reconstructing the components of responses has given rise, from the subfield of social science methodology dealing with measurement error, to a family of analytical models and methods.[48] The approach presented here is a simple member of that family because almost all the SRC panel responses can be simply explained and because a fair comparison of results from our probability model and the Black-and-White model obliges us to severely restrict the variables affecting responses.

According to James Coleman, "the influences of the moment may lead a respondent to reply differently at different moments, though he has not changed his attitude in any way. . ."[49] Correspondingly, we assume that

(1) responses result from an attitude plus short-term forces. Where Z is the response, X is the attitude, and Y is the short-term force,

$$Z = X + Y.$$

For each respondent, the short-term forces in principle comprise a random variable, whose multiple causes make it normally distributed, independent of the attitude, with a mean of zero. Thus, over a panel study, the responses of individuals should basically reflect their attitudes as "each will be tending toward his own mean (attitude)."[50] Across the population of a polity at a given point in time, attitudes on items of moderate generality are likely to be normally distributed because of the diverse but delimitable sources of attitudes. We assume that in principle

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[47] For a discussion of the possibilities and complications of a revised "nonattitude" method see Appendix Two.

[48] For a basic discussion of methods of treating measurement error see Paul M. Siegel and Robert W. Hodge, "A Casual Approach to the Study of Measurement Error," in Blalock and Blalock, pp. 28–59; also see James S. Coleman, "The Mathematical Study of Change," in Blalock and Blalock; Paul Lazarsfeld and Neil Henry, *Latent Structure Analysis* (Boston: Houghton Mifflin, 1968); J. Johnston, *Econometric Methods*, 2nd ed. (New York: McGraw-Hill, 1972), chapters 7 and 9; F. M. Lord and M. R. Novick, *Statistical Theories of Mental Test Scores* (Reading, Mass.: Addison-Wesley, 1968); and, Herbert Costner, "Theory, Deduction and Rules of Correspondence," *American Journal of Sociology*, 75 (September, 1969), 245–263.

[49] Coleman, "The Mathematical Study of Change," in Blalock and Blalock, p. 472.

(2) attitudes and short-term forces are independent, random, normally distributed variables, and consequently responses are independent, random, normally distributed variables.[51]

In lieu of verification to the contrary, we assume that these principles can be directly applied to the domestic policy questions in the 1956–58–60 SRC panel study. That is,

(3) there is no bias in population sampling, sampling of individual attitudes, coding, and political events, and other potential sources of net error need not be considered.

These first three assumptions are the general elements of a probability model of responses. In addition, the operations here require three further assumptions. Because we wish to base our estimates on only two trials of a single questionnaire item, we assume that:

(4) all respondents have the same attitude range.[52]

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[50] *Ibid.*, p. 439.

[51] The conception of attitudes can be formalized in regression notation in the following manner: $Y_{ij} = X_i + e_{ij}$ where Y_{ij} is the response of the *i*th individual at the *j*th trial, X_i is the true attitude of the *i*th individual, and e_{ij} is the sum of the *i*th individual's nonattitude response influences at the *j*th trial. If the nonattitude response influences (e_{ij}) are random with regard to the true attitude, then $\overline{Y}_i = X_i$. That is, across sufficient trials the opinion responses will directly reflect the underlying attitude. Given an infinite number of trials, with e random with regard to the attitude, then an individual's attitudinal and nonattitudinal components or bases of response can be recovered for each trial.

Nonattitude components of response, while treated as error, are simply independent variables which we do not want to study precisely. Sometimes—if they are of sufficient magnitude, are non-random with regard to attitude, or are easily identifiable—they should be brought into the equation in the following manner: $\mathrm{Y}_{ij} = b_{i1}X_{i1} + b_{i2}X_{i2} + \ldots + b_{ik}X_{ik}$, where X_{ik} are variables (across k) which compose the *i*th individual's response at trial *j*. The b_{ik} are weights assigned by the *i*th individual to the variables going into the nonattitude response component. Response variation in this model arises from (1) variation across trials, individually and collectively, in the values of the variables, and (2) variation across individuals in the weights assigned to determining factors. When individuals' variable scores for several factors are constant across trials ($X_{i11} = X_{i1j}$, $X_{i2l} = X_{i2j}$, etc.), the total of these factors can be treated as an individual's constant (his attitude): $a_i = b_{i1j}X_{i1j} + b_{i2}X_{i1j}$. This fuller model can be solved only with sufficient and detailed information at the individual level about the values of variables.

The above model is limited in two serious ways: (1) it is static, and (2) it makes no provision for perceptual screening. This screening has the effect of limiting or changing the values for short-term forces, change which is based on the values of the longer term variables and weights. Thus, some variables are partly dependent on other variables, yet independent in their effects on the response. $X_{it} = B_{ih}Y_{iht} + \gamma_{ikt} Z_{ik}$ where X_{it} is the vector of the responses of the *i*th individual across t trials, Y_{iht} is the matrix of h short-term forces for the *i*th individual across t trials, B_{ih} is the vector of h short-term force weights for the *i*th individual, Z_{ik} is the matrix of k long-term forces for the *i*th individual, and γ_{ikt} is the vector of k weights for the long-term forces of individual i at t trials. Thus, the short-term forces depend on attitude, $Y_{ih} = \gamma_{ijt}Z_{ik}$, and predisposing events such as questionnaire bias. Short-term forces have impact, but only after they have been screened.

Attitude change results not only from the direct addition of a new predisposing factor, but also from the impact of some screened variables. In attitude change, the change factor often is originally treated as just another short-term force. If, after screening, the change factor appears to be of sufficient duration and magnitude, it modifies the treatment of the predetermined variables (attitude) $\gamma_{ijt} + 1 = \gamma_{ikt}Z_{ik} + B_{ig}Y_{igt}$. So, the importance attached to the long-term attitude components is a function of previous components and their importance plus short-term impacts.

[52] Variations in the stability of categories should, in our model, depend on the distance from the mean and the size of the category. The expected difference in stability between categories can be calculated. However, other differences in variation can occur. In these cases, variation in individual standard deviations is related to the responses given. The likely sources of the category variation-individual variation association all involve more than one attitude being tapped by the question. Generally, this second attitude has to do with a "strong-weak" dimension, while the dominant dimension remains agree-disagree on the particular issue involved. To illustrate this point, some individuals' attitudes are minimally differentiated—they simply agree with the particular issue position. No more complex response could be accurately elicited. When these individuals are forced into a strong-weak distinction, that distinction may be made on the basis of a second attitude. For many questions, the source of this second attitude appears to be found in a culturally determined preference for strength, as in "strongly agree." As a result, many give strong responses which may or may not be related to their substantive preferences on the issue. This is of particular concern for individuals who are ambivalent in their true attitude—who through the normal variation we already have noted may come down one time on the agree and another on the disagree. Some people who both are ambivalent in their preference of agree or disagree and have a very minimal amount of differentiation in their choice are coded as strong agreement on the first trial and a certain

As long as the precision of attitudes is not related to the location of attitudes, this convenient assumption will produce relatively small error. Following the assumptions, the equations for attitudes and short-term forces are

$$X' = Z'\sqrt{1 + b^2} - bY'$$

$$Y' = \frac{Z'\sqrt{1 + b^2} - X'}{b},$$

where

$$b = \frac{\sigma y}{\sigma x},$$

and Z', X', Y' are the standardized scores for responses, attitudes, and short-term forces. Once the response data are in hand, calculation of attitudes and short-term forces for the population requires only an estimate of b, the ratio of short-term to attitude components.

The investigation of attitudes over time requires consideration of the possibility of attitude change. Here, we assume:

(5) no attitude change occurs.

The error involved in making this assumption can be estimated by applying Heise's formulas to the responses.[53] Either dichotomizing the data (agree, disagree) or scoring responses −2, −1, 0, 1, 2, for "strongly agree" to "strongly disagree" provides the results presented in Table 4.

Table 4. Causal Path Analysis of Attitude Stability

Question	Stable Attitude Share of Attitude Variance[a] 1956–1958	Stable Attitude Share of Attitude Variance[a] 1958–1960
School Integration	61%	95%
Job Guarantee	81%	98%
School Aid	91%	90%
FEPC	94%	98%
Public Power	99%	99%

[a] Entries are the squared stability coefficients—the squared correlations between attitudes on a question in the two samplings.

While change definitely occurs in school integration and job guarantee attitudes between 1956 and 1958, presumably as the result of Little Rock and the 1957–58 recession, little or no change occurs on the other items or in the 1958–60 period. In this general absence of change, attitudes can be estimated from two trials by either our probability model or a consistency test of the Black-and-White model.[54] The second trial adds new information—some changed responses as a result of individual shifts in short-term forces—without adding any new unknowns. Because estimation of attitudes and short-term forces requires only two pieces of information, responses and the ratio (b) of attitude to short-term components, two trials provide all the information necessary. Either trial provides information on responses and the difference in individual responses defines the ratio b. Thus:

$$\text{Variance of } (Z_1' - Z_2') = \frac{2b^2}{1 + b^2},$$

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percentage of the time give a disagree response and are coded as strong disagreement because of the second attitude involved. Thus, the actual differences in response are magnified, and each time the variation from the true attitude is considerable. "Weak" responses, which include only the single attitude dimension (no second attitudinal preference for weakness), thus show more stability than the "strong" responses (relative to the stability predicted from the distribution). If this strength dimension occurs, then the true estimate of attitudes should be geared mainly toward accounting for the weak categories. For example, assume the attitude continuum on public power control as shown below:

Depends
Strongly Agree ________________ Strongly Disagree
10 9 8 7 6 5 4 3 2 1 0

An individual's true attitude may be found at the "depends" point. On trial one, the response may be at the 6 point on the dimension as the result of short-term forces, but because of the second attitude biased toward strength he may in the end be coded at the 9 or 10 position. At the second trial, his response may be true at the 4 position, again because of short-term forces (still basically within his tolerance for inconsistency and relatively stable with his earlier 6 response —both varying only one point from his true attitude at position 5), yet the bias for strength may place him at the 1 or 0 position of strong disagreement. In the recorded response, his inconsistency appears to be extremely high—the difference between position 9 and position 1, while his true *response* varied only from 6 to 4, and his true *attitude* remained completely stable. The impact of the potential intrusion of a second, and possibly even third attitudinal dimension into the one the research is attempting to measure will certainly vary with the topic. Thus, for party identification, the strong-weak distinction appears to tap a politically more substantive attitude, in which the strength may be more closely tied to the actual attitude toward a respondent's own party.

[53] David R. Heise, "Separating Reliability and Stability in Test-Retest Correlation," *American Sociological Review*, 34 (February, 1969), 93–101.

[54] When attitude change is not involved in response variation, the timing of surveys is not an essential item. Because individual variation around the mean is unsequenced, the sequence of surveys is irrelevant, and measures of change over time—such as Markov chains and Coleman's techniques—are inapplicable.

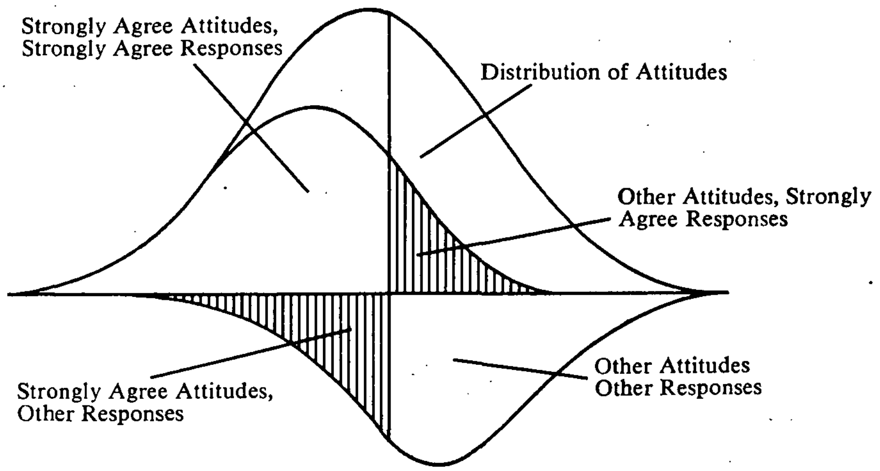
Strongly Agree Attitudes,
Strongly Agree Responses
Distribution of Attitudes
Other Attitudes, Strongly
Agree Responses
Strongly Agree Attitudes,
Other Responses
Other Attitudes
Other Responses

Figure 1. Relationship of Attitudes and Responses: FEPC 1958–60 As an Example.

Explanation: The vertical axis denotes the percentage of the population, the horizontal axis the strength of the attitude. The uppermost curve represents the true attitude distribution of the population; the unlined segments of the other two curves depict attitude and response congruence, the lined segments depict attitude and response discrepancy.

where Z_1' and Z_2' denote standardized response scores of an individual at the first and second trials, respectively. An illustration of the joint distribution of attitudes and responses for a single trial of a 55 per cent "strongly agree" item (FEPC in 1958–60) is presented in Figure 1.

While a complete accounting of the SRC panel data for even the single public power item would include analysis of noninterviews (28 per cent of the sample) and nonsubstantive responses (e.g., "no opinion" at one or more trials, another 33 per cent of the respondents), both the Black-and-White model and the probability model apply only to the data indicating substantive attitudes at two or more trials, usually about two-thirds of the total sample. For the five available substantive categories, "strongly agree" to "strongly disagree," we assume that:

(6) respondents pick the category most closely approximating their preferred response.

Our theory has presumed that attitudes, short-term forces, and responses are interval variables, albeit measured ordinally in the SRC surveys. Further, the type of inferences we (or Converse) wish to make about the relative roles of attitudes and other forces in producing responses requires pinning numbers on the categories. The final requirement for transforming the data is the ability to represent properties of the data which untransformed or otherwise transformed categories would miss.[55] From our second assumption, we presume that responses are normally distributed, with a mean of zero and a variance of 1 for each set of responses (each item, each trial). From the sixth assumption, the five categories can be conceived as covering exhaustive and exclusive ranges of the response continuum, with the boundaries between categories occurring at the points in a normal distribution which cut off the percentage of responses at least that extreme. For instance, if 40 per cent of the sample responds "strongly agree," the most extreme lower category, then the upper boundary of that category must be —.25 standard deviations from the mean (zero), because only that point cuts the normal distribution into forty and sixty per cent shares.

One of the empirical propositions which derive from our assumptions is that the stability of responses across trials should vary with the width of the category. To test briefly the representativeness of the normal transformation, we take the five domestic policy questions in 1956, give each response category the normal score of its median member's average distance to the category boundaries, and correlate these scores with the 1956–58 category

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[55] E. R. Tufte, "Improving Data Analysis in Political Science," in *Quantitative Analysis of Social Problems*, ed. E. R. Tufte (Reading, Mass.: Addison-Wesley, 1970), esp. pp. 440–441; and Thomas P. Wilson, "Critique of Ordinal Variables," in *Causal Models in the Social Sciences*, ed. Blalock, esp. p. 417 and notes 2 and 3.

Table 5. Correlation of Response Category Normal Score Functions with Response Stability, for Five Domestic Policy Questions: 1956–58[a]

Question	Correlation
Public Power	.98*
Job Guarantee	1.00*
School Aid	.97*
Fair Employment	.98*
School Integration	.99*

[a] Entries are Pearsonian product-moment correlations.

* Significantly different from zero at the .01 level.

stability (per cent of 1956 respondents giving the same response in 1958) for each question. As Table 5 shows, the transformation accurately reflects empirical properties of the data. Other transformations are less accurate guides: equal interval scores produce zero correlations, of course, and transformations approximating dichotomous treatment (for instance, −5, −4, 0, 4, 5 scores) which produce strongly negative correlations, $r = -.85$ to $r = -.90$, are significantly incorrect predictors of stability. There appear to be solid theoretical and empirical grounds for using a normal transformation to investigate response stability characteristics.

The final jump of moving from normal scores for categories to recovering normal scores for a full distribution of the sample is again justified empirically as well as theoretically. To anticipate slightly, when responses are given full distribution scores, correlations between attitudes and responses range from .71 to .82, an average increase in the attitude explanation of response variance of 12 per cent over the use of single category normal scores (correlations from .60 to .75 for the five questions). As expected, the attenuation of correlation between attitudes and responses is considerably reduced by using the full normal distribution.

While the probability model has a number of uses, we employ it here only to account for the aggregate over time distribution of responses to the five SRC items so that we can reliably estimate the relative impact of attitudes and short-term forces in producing the responses. With stable attitudes and no bias, the response distribution for any pair of trials results from three independent, normally distributed, random variables: attitudes, short-term forces at the first trial, and short-term forces at the second trial. In formal notation, the proportion of respondents giving "strongly agree" responses in both samples is a function of

$$\int_{-\infty}^{\infty} \Phi(X')dX' \int_{-\infty}^{(Zsa\sqrt{1+b^2}-X')/b} \Phi(Y')dY' \cdot \int_{-\infty}^{(Zsa\sqrt{1+b^2}-X)/b} \Phi(Y')dY'$$

where $\Phi(X')$, $\Phi(Y')$ are the normal functions of attitudes and short-term forces, and *Zsa* is the upper category boundary for "strongly agree" responses.

The notation indicates that the probability of a respondent with any particular attitude giving two "strongly agree" responses depends on his attitude and on the prevalence of short-term forces which can keep him (or push him) into the "strongly agree" category in both trials. If "strongly agree" responses are frequent and stable in the sample, then many people have "strongly agree" attitudes and the short-term forces are relatively weak.

To arrive at a good estimate of *b*—the ratio of short-term forces to attitude variation—requires only the overall distribution of responses and, in theory, the aggregate frequency for any of the twenty-five pairs of responses in two trials. In practice, possible minor variations of our assumptions led us to choose the best estimate of *b* by minimizing the deviations of actual from predicted frequencies for the respondents giving identical responses in both samples—the "stable" respondents. We found it most practical to arrive at the best estimate by experimenting, yielding precision to retain economy. Readers further interested in our methods are referred to Appendix 1.

Findings

Application of the probability model to the SRC panel data produces several precise inferences about the relation of attitudes and responses. Before moving to these relations, however, we consider several tests of the model's accuracy. The first test is a version of the standard goodness of fit criterion, while the remaining tests evaluate accuracy by extending predictions to new data.

Goodness of Fit and Error Sources. For each question the five substantive response categories give a 5 × 5 table of responses in each pair of years. Using the probability model, four of the average marginals and an estimate of the attitude/short term variance ratio, we estimate the proportion of respondents who should fall into each of the 25 cells. If the model and the estimate of the ratio (*b*) are correct, the fre-

Table 6. Goodness of Fit and Error Sources: Application of Probability Model to SRC Panel Responses

Question: Years	Respondents Correctly Placed	Error Source	
		Asymmetrical Responses	Unexplained
School Integration			
1956–1958	93%[a]	4%[b]	3%
1956–1960	93%	3%	4%
1958–1960	92%	2%	6%
Fair Employment			
1956–1958	94%	4%	2%
1956–1960	95%	4%	1%
1958–1960	94%	3%	3%
Federal School Aid			
1956–1958	94%	3%	3%
1956–1960	91%	8%	1%
1958–1960	90%	5%	5%
Job Guarantee			
1956–1958	92%	3%	5%
1956–1960	93%	5%	2%
1958–1960	94%	4%	2%
Public Power			
1956–1958	93%	4%	3%
1956–1960	91%	5%	4%
1958–1960	90%	5%	5%

[a] Entry is 100 per cent of respondents minus one half of the absolute deviations of actual from predicted cell frequencies.

[b] Excess frequencies for one ordering of pairs of responses ("strongly agree," "weakly agree") over opposite ordering ("weakly agree," "strongly agree"). Under the no change and no bias assumptions, these differences are outside the probability model.

quency of the various response patterns (cells) should match our predictions. The model is accurate when actual frequency matches the prediction for each cell and is inaccurate when actual frequencies deviate from estimates. The adequacy of the sample probability model is illustrated in Table 6. For the five items, accuracy varies from 90 to 95 per cent, as measured by "respondents correctly placed."[56] Considerations of imprecision, sampling error and asymmetrical response shifts led us to take 90 per cent accuracy as our operating criterion of adequate fit.[57]

Our assumptions of no attitude change and no sources of bias make it *a priori* impossible to account for asymmetrical patterns of response where there are real differences between trials.

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[56] The particular measure is the absolute deviation of observed from predicted responses, divided by two. The division is necessitated by all deviations from prediction being counted twice, once positively and once negatively, producing error which potentially runs from zero to two; division by two results in error potentially running from zero to one (an alternative computational method is to count only positive [negative] error). No squaring procedure is used with the error for several reasons. First, it involves weighting cases, which we have no grounds to do. Second, it confuses interpretation; the percentage of cases deviating from prediction is easily understood. Third, the squaring overemphasizes large errors, but these are precisely the errors most likely to result from response bias and thus the errors which provide no true test of the hypothesis.

[57] There is no simple test for the adequacy of the model, but our operating criterion of 90 per cent accuracy was chosen with the following considerations in mind. First, there is some imprecision in the percentage of respondents and the predicted frequencies. The sample averages 1000 or less respondents per table, so accuracy beyond the .1 per cent level is impossible; similarly, our 100 intervals of the normal distribution average a precision of about .2 per cent. From these imprecisions, we would expect a total of about 2 per cent inaccuracy across the 25 cells. Second, sampling theory would lead us to expect that if the predictions were correct for the entire population, half the time the deviation of the sample from the predictions (or population) would be more than 25–30 per cent across the 25 cells. Because our predictions are based on the actual sample frequencies, however, many of the deviations would have been taken account of in arriving at the predictions. Correspondingly, we note that if every cell were below its predicted error, total deviation would be 9 per cent—a more reasonable estimate. Third, actual change, response-set factors

Table 7. Accuracy of Probability Predictions When Extended to Three Trials

	Item				
	Power	School Aid	School Integration	Job	FEPC
Accuracy[a]	96%	97%	97%	98%	99%

[a] The entries are the variance explained in the cell entries, 125 cells for the 1956–58–60 panel, based on predictions from the probability model for two years; variance explained is measured by the square of the Pearsonian product-moment correlation (r^2).

These responses are outside both the Black-and-White model and the probability model as used here. As the reader can see from Table 6, such responses are a noticeable though not major (average: four per cent) component of the data; they are, however, the major source of inaccuracy for the probability model. Errors within the probability model average about three per cent of responses and respondents, a tolerable amount of inaccuracy.

Prediction Tests. Goodness of fit has in part been tested by the same data which give rise to our estimates. While this is a common statistical practice in political science, e.g., the use of correlation and regression coefficients from the same data base, an external standard of accuracy is more reliable. Because we have used only two trials in arriving at our probability model estimates, the third trial can be used as the basis for several tests.

We take as the best estimate for each question the *b* and marginals from the pair of years with the least asymmetrical bias. If the assumptions about attitudes and short-term forces for two trials are extended to the third trial, tests on several aspects of the predictions are possible. First, we test predictions of cell frequency. There are 125 cells (5 × 5 × 5) in the three trial turnover table, with the average cell containing about four respondents, but with considerable variation in the actual frequencies. Using four average marginals and one estimate of *b* from the two-trial procedure, we predicted the number of respondents in each of the 125 possible cells, rounded conveniently to the nearest whole respondent. As summarized in Table 7, the correlation of actual and predicted cell frequencies is quite high for all five questions, with 96 per cent to 99 per cent of the variance in cell frequencies explained by the model, despite the presence of asymmetry. In part, the correlations arise from the presence of a few highly populated cells; even with these cells omitted, however, the worst prediction still accounts for 87½ per cent of the remaining variance. The absolute accuracy is equally striking and well within the SRC's normal sampling error: the cells predicted to have below average frequencies of 0 to 3 cases, taking the FEPC question for example, are never off by more than 3 cases, 97 per cent are 2 or fewer cases off, and 75 per cent deviate from prediction by one case or less. The probability model estimates adequately account for the new data from a third trial.

A second type of test using the third trial aims directly at the asymmetry, bias, change and instability of responses problems. To isolate such data, we take only those respondents who have switched sides by answering some form of "agree" at one trial and some form of "disagree" at another trial. For two trials, the aggregate of such respondents necessarily gives equal frequencies of "agree" and "disagree" responses, as every respondent produces one of each; thus, there is no data from these responses themselves indicating a prevalence of "agree" or "disagree" attitudes. Using the rest of the sample, though, the probability model necessarily predicts from two trials what the relative prevalence of "agree" or "disagree" responses will be for unstable respondents in three trials. With only two categories—"agree" and "disagree"—and three trials, the respondents will either give two "agrees" and one "disagree" (responses are .67 "agree") or one "agree" and two "disagrees" (responses are .33 "agree"), and the aggregate of such respondents can show a predominance of either "agree" or "disagree" responses. The probability model predictions of "agree" predominance and the actual predominance for three trials on each question are reported in Table 8. Aside from a slight tendency to overestimate "agree" responses, the probability model predicts change

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limited to one year, etc., cannot be accounted for by the prediction if they are unidirectional in net effect across the sample (otherwise they simply get counted as "nonattitude forces"—the fate of most School Integration and Job Guarantee change in 1956–1958). These range from 2 through 7 per cent for any pair of consecutive samples.

Table 8. Predicted and Actual Predominance of "Agree" Responses Among Respondents Taking Different Sides on an Issue in Three Trials[a]

Source	Issue				
	School Aid	FEPC	Power	Job	School Integration
Probability Prediction	.61	.64	.60	.57	.49
Actual Results	.62	.61	.59	.56	.45

[a] Entries are the proportion of respondents with predominately "agree" responses among the total respondents answering "agree" and "disagree" in different years.

"bias" quite well ($r = .97$, predicted and actual "agree" predominance, across the five items). Prediction test results confirm the reliability of probability model estimates.

Attitudes and Responses in the SRC Panel Study. In the probability model, responses reflect two influences: the distribution of attitudes and short-term forces producing variation within the attitude ranges. While all responses reflect attitudes, they also vary with short-term forces. For an infinite number of trials, these short-term forces would be of equal variance and average direction (zero) for all respondents; with only two trials, respondents will vary in the magnitude and direction of short-term forces, so that responses will not have exactly the same relation to attitudes for all individuals; in the aggregate, however, the relation of attitudes and short-term forces to responses is fixed, so we can estimate the attitude share of responses for the sample as a whole.

For each question, we take the attitude variance as a normalized constant set equal to one; the response variance is then a function of the normalized short-term variance, as

$$\begin{aligned}\text{Variance }(Z) &= \text{Variance }(X) + \text{Variance }(Y)\\ &= 1.0 + b^2,\end{aligned}$$

where Z is responses, X is attitudes, Y is short-term forces and b is the ratio of attitude to short-term forces standard deviations. The attitude share of the response variance is then

$$\frac{\text{Variance }(X)}{\text{Variance }(Y)} = \frac{1.0}{1.0 + b^2}.$$

Note that attitudes are here being recorded as points (the mean of the attitude range) to measure their impact.

The attitude share of response variance for the five items is presented in Table 9. If responses were truly random around any measured mean, the attitude share would be measured as zero, as differences between individuals would be accounted as resulting from the use of only two trials; if responses were exactly stable, the attitude share of the variance would be estimated at or near 100 per cent, as variation would entirely occur between individuals.

Because response variance reflects only differences, there are several reasons why the attitude share of the variance could be large or small. Most obviously, the magnitude of short-term forces can vary. Issues associated with large public controversy, e.g., an election, may receive sizable injections of short-term forces in the form of local events, personalities or temporarily associated issues. Perhaps less obviously, consensual issues will show less attitude dominance of responses than will polarized issues. As differences between individuals increase (polarization), short-term forces appear as less important because their impact relative to attitude differences diminishes. School integration, the most conflictual of the five issues, thus has the highest attitude share of the variance, though the short-term forces involved would appear historically to have been as great as or greater than those for the other issues. Additionally, there are a number of potential technical causes of variation in the attitude share of the variance, e.g., the use of questions which tap two or more attitudes. Despite these potential variations, the actual attitude share of the variance for the five issues stays within a fairly narrow range, an attitude-response cor-

Table 9. Attitude Share of Response Variance on Five Domestic Items[a]

Item	Attitude Share of Variance	r^a	Chance r Expectation
School Integration	67%	.82	.0
Job Guarantee	61%	.78	.0
Fair Employment	61%	.78	.0
Federal School Aid	60%	.77	.0
Public Power	51%	.71	.0

[a] r is the Pearsonian product-moment correlation of attitudes with responses.

Table 10. Relation of Responses to Attitudes: FEPC 1958, Simplified Example

Responses	Attitudes			Total
	Agree	Depends	Disagreee	
Agree	66%	4	3	73%
Depends	4	1	2	7%
Disagree	7	3	10	20%
Total	77%	8%	15%	100%

relation of .71 to .82, suggesting that while attitudes almost always dominate responses, the short-term forces have a reliably sizable impact.

In practice, the short-term forces disturb inferences about individual attitudes but have slight effect on inferences about attitudes in the population. While, according to the probability model, all individuals do have attitudes, for some individuals these attitudes are not at all clear from the responses, and for any single individual we can only make probabilistic statements about his attitude (see Table 10). When discussing the population or sample, however, inferences are more precise. The only effect of short-term forces on net responses is to increase the variance, making attitudes appear more polarized than they in fact are (see Table 11). Thus, short-term forces have little effect on our estimates of the distribution of attitudes in the population, and the responses provide a good guide to population attitudes.

Comparison of Results from Two Models. Both the probability model and the Black-and-White model aim to account for response variation in the absence of attitude change, and both hypothesize a mix of stable attitudes and random force as the cause of the over time response patterns. The models differ in allocating attitudes and random forces among respondents. The Black-and-White model posits two exclusive groups of respondents: those with attitudes, subject to no apparent random forces, and those without substantive attitudes, influenced solely by random factors. The probability model assumes all respondents have attitudes and all are subject to short-term random forces, which are normally distributed across respondents for any trial. Response patterns are used for different purposes in the two models: in the Black-and-White model, responses are a lever to segregate respondents and estimate the proportions of "nonattitude" respondents; in the probability model, responses are the clue to estimating attitude and short-term forces in the responses themselves and they indicate the relative impact of random forces across the sample.

Converse assumes that respondents are unreliable, so when one response discrepancy occurs he removes more than one respondent from view, while we assume that responses are unreliable and remove a portion of the response from view when a discrepancy occurs. Thus, different aspects of the same data are used to arrive at estimates of stable attitudes. Precise direct comparisons of results are difficult because of the contrasting treatments: Converse's figures apply to respondents, while ours apply to response variance; we use two trials, Converse uses three; an interval dimension based on five categories underlies our calculations, while Converse distinguishes ordinal groups from two or four response categories; and, finally, the slight estimating errors in the models cannot be translated into modifications of the basic estimates. Nevertheless, the results of the two models are so different that even imprecise summaries will show the contrast.

For the respondents consistently giving substantive responses to the public power question, the two models sharply disagree on the distribution of attitudes among respondents, on the relation of attitudes to responses, and on the polarization among the attitudes which do exist. The Black-and-White model assumes that only about one-third of this group of interviewees has attitudes, while the probability model indicates that almost all of them have attitudes. For Converse, attitudes are directly reflected in responses, though the overall pattern of responses misrepresents the pattern of

Table 11. Marginal Distribution of Responses and Attitudes for Federal Aid to Education, 1956–1958

	Category					
	Strongly Agree	Weakly Agree	Depends	Weakly Disagree	Strongly Disagree	Total
Responses	51%	20	9	7	13	100%
Attitudes	49%	28	10	6	7	100%

attitudes; for us, attitudes are not exactly mirrored in responses, but across the sample, responses give an adequate fix on the distribution of attitudes. The Black-and-White model leads to an estimate that there are two polarized sets of attitudes: a majority with extreme agree positions and a sizable minority with extreme disagree views; for the probability model, attitudes are even more consensual than responses, with moderate agreement the norm and few respondents strongly disagreeing. It is useful to decide which model better reflects reality.

In our critique above, we pointed out that the Black-and-White model does not account for four main aspects of the data: the distribution of responses among substantive categories, the cross-trial variance in responses, the stability and coherence of "changers" over time and across questions, and the common response patterns of "changers" and stable respondents. The probability model, however, not only accounts for the marginals but provides a full reconstruction of the attitudes and short-term forces producing the marginals. With a mild coda applying to attitude change, the probability model correctly estimates the over time response patterns for the whole set of respondents giving substantive opinions, stable and "changed" responses alike. In the probability model, the respondents are all subject to the same type of forces and hence respond according to a general pattern. Because attitudes are the primary basis for responses, the coherence of responses across questions is no mystery. There does not appear to be any extension of the nonattitude model which can account for all these aspects of the data (see Appendix II).

The probability model possesses more general advantages. It is applicable, with or without modification, to a wide range of questions, as indicated here by the results for the five domestic policy questions. The results across questions on the relative dominance of attitudes in producing responses (Table 9) accord with empirical indicators and theoretical expectations. Though we have made clear here only the attitude portion of responses, the probability model could lead to a more general explanation of responses by the addition of more dimensions for secondary attitudes, response set or attitude change. The model itself does not seem compatible with competing nonattitude explanations; a relabeling of attitudes as response set, for instance, would run afoul of some of the changes across questions. Finally, the model builds on attitude theory which does exist: attitudes are predispositions to respond with ranges of acceptance, the causes and support for attitudes largely overlap among the population producing major commonality of attitudes, responses are additionally affected by a host of other factors independent of attitudes and unequally distributed among individuals, and attitudes are widely stable except for the intervention of major public events. We believe the probability model is desirable on empirical, methodological and theoretical grounds and should prove useful in analyzing survey responses.

Implications

Correlations among Responses. The use of measures of association with opinion response data is open to pitfalls. With few, loose response categories and moderate attitude ranges, it is highly unlikely that the researcher will discover high correlations, even in the case of an identity relationship. This is particularly applicable where a large amount of consensus exists or where the data are grouped into "low" categories with many respondents and "high" categories with few respondents, most of whom will not truly belong there.

Correlations measure covariance against a maximum covariance. Normally, this maximum is calculated from the marginals, assuming that some 1:1 correspondence between variables is possible. This maximum is inappropriate for at least the opinion items included in the panel study. The actual maximum is best approximated by the correlation of responses at two trials, which is in theory a 1:1 correspondence within the limits of measurement. Correlations between opinion items should thus be measured against the correlation of the items with themselves. For example, the tau beta between the items on fair employment and job guarantee is nominally .24 in 1960; however, the tau betas of the two items with themselves in 1958-1960 are both .41, which indicates the actual maximum true correlation of any item with these questions. Correspondingly, the actual correlation between fair employment and job guarantee in 1960 is .24/.41 = .58 of the maximum possible, resulting in a substantial change in what we would conclude about the coherence of mass political beliefs on these two issues.[58]

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[58] Short-term forces operating on both responses in one interview may *increase* the association between attitudes, however, so some care is required in falsely correcting correlations. For specific guidelines on dealing with these problems see Costner, "Theory, Deduction and Rules of Correspondence"; Heise, "Separating Reliability and Stability in Test-Retest Correlation"; and Coleman, "The Mathematical Study of Change."

Democratic Elite Theory. The application of the probability model of attitude measurement results in the substantive conclusion that many more Americans have true attitudes on issues of public policy than previously had been concluded. These data lend support to the contention that in at least one regard the voter has been miscast and that we need not rely only on an "opinion elite" to provide substantive support to democracy in the form of attitudes about issues of public policy.

To be sure, many more demands are placed on the common man in democratic theory than that he take positions on issues of public policy. He also is asked to possess politically relevant information, to have some minimal degree of psychological involvement in politics—and in elections in particular—to monitor the activities of government for their personal impact, to support democratic principles and procedures, and to participate in the electoral choice of the system's political leaders. The failure of most Americans uniformly to measure up to the *complete set* of expectations has been the implicit, if not explicit, interpretation of a broad range of empirical studies. Also, this picture of the "common man" has given support to various theories of democratic elitism in the form of a recasting of democracy that emphasizes

> the stable, constitutional, and liberal nature of the system of elite pluralism; the competitiveness of political elites, their accountability to the electorate at periodic elections; and the open, multiple points of access to elite power. . . . To be sure, the ordinary man still plays a role in the system since he has the freedom to vote, to bring pressure upon political elites, and to attempt himself to rise to an elite position. But by and large he does, and is expected to, remain relatively passive—in fact, the health of the system depends on it.[59]

The question of the nature of the electorate's opinion holding is quite central to the arguments over democratic theory. Without true attitudes, or with randomly expressed ones, how can information about politics be made individually relevant to the role of issues in electoral choice,[60] on what basis are governmental policy decisions to be monitored, and how are political leaders to respond to apparent expressions of the constituency opinions?[61] Of course, the findings in this paper do not directly confront all of the questions raised in democratic elitism. On the other hand, these results suggest that one of the empirical foundations of the democratic elitist view of the common man—that his opinions on issues of public policy are expressed randomly—needs reformulation.[62]

Appendix I

Those interested in using the approach presented in this paper may wish to consider the following simplified example. A continuum from negative to positive infinity is divided into ten groups, each containing one-tenth of all cases distributed normally on the continuum. The means, in standard units, of these groups are −1.76, −1.04, −.68, −.39, −.13, +.13 . . . +1.76. These groups, and their means, apply to all normal distributions. For a particular case, responses to the FEPC item in 1958 and 1960 dichotomized into two groups, "strongly agree," and "weakly agree to strongly disagree," contain respectively 54½ per cent and 45½ per cent of the responses. The dividing line between the two groups (*Zsa*) is therefore +.1256 standard units of *Z* from the mean of *Z*. The ratio of short-term forces standard units to attitude standard units (*b*) is, from the text, about .9. What we first want to know is which short-term forces will push any given attitude into a response group; this is given in the text as all short-term forces lying between negative infinity and

$$\frac{Zsa\sqrt{1+b^2}-X'}{b}$$

which in this case equals

$$\frac{(.1256)(1.345)-X'}{.9} = .188 - 1.11X'$$

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[59] Peter Bachrach, *The Theory of Democratic Elitism: A Critique* (Boston: Little, Brown and Company, 1967), p. 8.

[60] Weisberg and Rusk, in "Dimensions of Candidate Evaluation," p. 1185, state that "party and issues thus provide two basic mechanisms of candidate evaluation."

[61] As Charles F. Cnudde and Deane E. Neubauer point out, "One of the most important relationships supporting democratic stability involves citizens' preferences, leadership awareness of those preferences, and policy outcomes"; see "New Trends in Democratic Theory" in *Empirical Democratic Theory,* ed. Cnudde and Neubauer (Chicago: Markham, 1969), p. 529. This relationship, of course, is central to the work of Warren E. Miller and Donald E. Stokes, in "Constituency Influence in Congress," *The American Political Science Review*, 57 (March, 1963), 45–56, in which leadership responsiveness is shown to vary across issue domains.

[62] Lewis Lipsitz, in "Forgotten Roots," in *Frontiers of Democratic Theory*, ed. Henry S. Kariel (New York: Random House, 1970), pp. 402–403, discusses the impact of empirical findings such as those we discussed early in the paper and concludes that in relationship to thinking about the attitudinal foundations of democracy, "these are empirical questions that have not been asked clearly enough."

That is, for any attitude, the short-term forces less than .188 minus 1.11 times the attitude will put the respondent into the "strongly agree" response group. Short-term forces more positive than this will put the respondent into the "weakly agree to strongly disagree" response group. Responses depend on the relative magnitudes of the attitude and short-term forces, each (at this point) measured in terms of their own standard units. Both attitudes and short-term forces are distributed into the ten groups mentioned above, in their respective standard units around their means.

X' (Attitude in Attitude Standard Units)	$1.11X'$ (Attitude in Response Standard Units)	$.188\text{–}1.11X'$ (Criterion Value)
−1.76	−1.96	2.14
−1.04	−1.16	1.35
− .68	− .75	.94
− .39	− .43	.62
− .13	− .14	.33
+ .13	+ .14	.05
+ .39	+ .43	− .24
+ .68	+ .75	− .56
+1.04	+1.16	− .97
+1.76	+1.96	−1.77

The above figures, rounded off after a more precise calculation, indicate for each of the ten groups of attitudes what level of short-term forces divides responses into "strongly agree" and the other category. For instance, that tenth of attitudes which is −1.76 units below the mean requires short-term forces 2.14 units above the mean to produce anything but a "strongly agree" response.

The imprecision of our ten groups may cause some problems. The point $Zsa\sqrt{1 + b^2}$ defines which attitudes are truly "strongly agree," namely 56¾ per cent of the attitudes. With only ten groups, we are limited to guessing either 50 per cent or 60 per cent, so for convenience let us further divide this group in half, changing nothing else, which will allow us to trace the 55 per cent (approximately) of the respondents whose attitudes are truly "strongly agree." Also, because the criterion value for one of attitude groups (−1.77) virtually coincides with the mean value of one of our short-term groups (−1.76), let us divide the attitude group in half, so that half has a criterion value above −1.76 and half below −1.76. The effect of these changes is to produce 12 attitude groups, 8 with 10 per cent of the attitudes apiece, 4 with 5 per cent of the attitudes apiece. The short-term forces (Y') remain divided into ten groups with values the same as for the original ten X' groups, except for the standard units. We wish to know, for each set of attitudes, which will be counted as "strongly agree," and this depends on what proportion of short-term forces falls below the criterion value.

All of the most extreme group of "agree" attitudes responds "strongly agree" because there are no short-term forces strong enough to produce any other response. Similarly, the 5 per

Per Cent Attitudes	Criterion	Short-Term Forces Below Criterion	Attitudes Below Criterion
Strongly Agree			
10%	2.14	10/10 = 100%	10% × 100% = 10%
10	1.95	9/10 = 90%	9
10	.94	8/10 = 80%	8
10	.62	7/10 = 70%	7
10	.33	6/10 = 60%	6
5	.05	5/10 = 50%	2½
			42½
Other			
5	.05	5/10 = 50%	2½
10	− .24	4/10 = 40%	4
10	− .56	3/10 = 30%	3
10	− .97	2/10 = 20%	2
5	−1.48	1/10 = 10%	½
5	−2.16	0/10 = 0%	0
			12%
100%			54½%

cent of attitudes which are most extremely "disagree" produce no "strongly agree" responses, again because short-term forces are insufficient to push them past the response borderline. In between, attitudes can be counted in either category, with the attitudes closest to the dividing line most likely to be split evenly between response categories. For a sample of 1000 persons, this pattern would produce a distribution of responses which largely match attitudes for a single sample.

Attitudes	Responses		
	Strongly Agree	Other	Total
Strongly Agree	425	125	550
Other	120	330	450
Total	545	455	1000

In this case, 75½ per cent of the sample respond in accordance with their attitudes.

If this set of respondents were sampled a second time, the second distribution of responses should exactly resemble the first in its overall dimensions. Individuals, however, would change some of their responses if short-term forces changed for them. For any attitude, the chance of responding "strongly agree" twice equals the chance of responding "strongly agree" the first time times the chance of responding "strongly agree" the second time; for the most extreme "agree" tenth of the attitudes this is 10/10 × 10/10 = 100/100. The similar figures for other responses and other groups are given below.

With three trials, 48.6 per cent of the respond- forces remains constant but that impact affects the response categories of more respondents. The complete match-up of attitudes and responses for the two trials is limited to 59.45 per cent of the sample. Most of these "correct" cases have extreme opinions: about 80 per cent of the most extreme half of the sample are correctly matched, while only about 40 per cent of the middle half show a perfect correspondence of attitudes and responses. The same pattern occurs for incorrect responses; for 8.45 per cent of the responses, both responses disagree with the attitude, but the misrepresentation is less than 3 per cent for the extreme half, while it is over 14 per cent for the middle half. The difference between the two groups in the adequacy of the sampling depends largely on the categories used: if the question were more strongly worded, extreme attitudes would show less correspondence with responses.

A third trial follows the pattern already established: the chances of giving any three responses in a row are equal to the product of the chances of giving each response at a single sample, which depends on the extremity of the

Respondents by Attitude	Responses*				
	SA–SA	SA–O	O–SA	O–O	Total
Strongly Agree					
100	100	0	0	0	100
100	81	9	9	1	100
100	64	16	16	4	100
100	49	21	21	9	100
100	36	24	24	16	100
50	12½	12½	12½	12½	50
Subtotal	342½	82½	82½	42½	550
Other (WA to SD)					
50	12½	12½	12½	12½	50
100	16	24	24	36	100
100	9	21	21	49	100
100	4	16	16	64	100
50	½	4½	4½	40½	50
50	0	0	0	50	50
Subtotal	42	78	78	252	450
Total	384½	160½	160½	294½	1000

* The response abbreviations are: SA–SA = strongly agree in both responses; SA–O = strongly agree in th first response, other in the second; O–SA = other response in the first trial, strongly agree in the second O–O = other response in both trials.

individual's actual attitude. Thus, for three trials:

Respondents Attitude	Number of Strongly Agree Responses				
	Three	Two	One	None	Total
Strongly Agree	286	169	79	16	550
Other	16	77	157	200	450
Total	302	246	236	216	1000

With three trials, 48.6 per cent of the respondents have responses which always match their attitudes, while 3.2 per cent of the respondents have responses which never match their attitudes. Of the remainder whose responses disagree, more than two-thirds have attitudes which match the majority of their responses.

Appendix II

Extending the Nonattitude Model to Include Response Set. Several readers suggested response set as the key explanation of difficulties in the nonattitude model. In fact, revising the nonattitude model to include explicit response set variables subtracts from the accuracy and cogence of Converse's account of the data.

Response set derives from attitudes—predispositions to respond—which are not the attitudes the questions are designed to measure; therefore they do not have a single appropriate response category and should lead to response instability over repeated samplings. In principle, then, stable response set attitudes can produce response variance if there are slight random forces affecting the response. We have extended the nonattitude model by adding predispositions to: *respond, agree, disagree,* and *demonstrate strength.* As in the original model, attitudes on the question asked are a residual category, and "depends" responses are omitted from analysis. Individuals are allowed any combination of response set attitudes which does not include both *agree* and *disagree* predispositions; attitudes on the questions are assumed to determine responses despite any presence of response set.

While by definition the extended nonattitude model accounts better than the original model or the marginals, certain over time patterns of instability, some of the coherence and stabilty, of nonattitudes, and some of the commonlity of stable and unstable responses, in pracice it does not fit the data as well. For the five uestions, the extended version accounts for only 40 to 75 per cent of the variance in preicted cell frequencies over the three trials. Any attempt to estimate a response set factor common across the items fares even more poorly.

In addition to being inaccurate, the response set uses questionable assumptions. The form of the five questions is standard, so response sets might be expected to be standard, and differences between patterns of response should be traced to the prevalence and direction of attitudes on the specific questions. In fact, it is the generalized predispositions to *agree* and *demonstrate strength* that vary most across questions. If this variance in turn is explained as the result of a zero-sum relationship between substantive attitudes and response set—so that attitudes on the question are again the source of variance—the data again disconfirm it. Across items, the *agree* response set and "strongly agree" attitudes are significantly correlated ($r = .63$), as are "strongly disagree" attitudes with *disagree, demonstrate strength,* and *respond* response sets ($r = .83, .87, .74$). The differences among the five questions indicate the spuriousness of the model: as responses in a particular category increase, the estimates for both the corresponding attitudes and the corresponding response set increase, showing the artificiality of the distinction. Further, though the question form and presumed response set factors remain constant, across items there is a zero-sum relationship among the estimates of the various types of nonattitudes. If response set varies with the question topic rather than the question form, what sense does it make to label it "response set"?

While the extended nonattitude model can be discarded on methodological grounds and further versions proposed and similarly rejected, the reliance on methodology indicates a serious problem: we do not have an operational theory which can reject models whose substantive conclusions make no sense. Converse's conclusions that 20 per cent of the American public had attitudes on public power questions were easily accepted. We suspect that the extended nonattitude model estimate that about 15 per cent of respondents had attitudes on school integration would have been equally palatable, though to use such an estimate is wildly implausible: the cognitive and behavioral components of school integration attitudes at the time of Little Rock and the jailing of Martin Luther King far exceed 15 per cent; theoretically, racial and regional issues should generate some of the strongest evaluative and affective responses possible. Yet the nonattitude estimates are surprisingly immune to the theoretical constraints because theory has not been formulated in a way that determines methods and checks conclusions.

Comment: The Status of Nonattitudes

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This is the fourth set of extended comments I have been asked to make on this evolving manuscript, my third effort having been a 27-page rejoinder prepared for publication. The original version of the Pierce-Rose piece contained, in addition to several interesting and fresh ideas, a lengthy section impugning my methods, common sense, and good faith in reporting the work I did years ago with what I called the "black-and-white model." This polemic rested, as it has turned out, on a labyrinth of factual misconceptions as to what I had actually done or why, underpinned by some embarrassing confusions on the authors' part with respect to elementary distinctions in the philosophy of science, the nature of attitude measurement, types of error structure and the like.

The majority of these flaws, including the *ad hominem* elements and the most egregious factual errors, have been progressively weeded out over three distinct versions of the manuscript. But in each new version new inaccuracies or misleading statements have relentlessly appeared, requiring another round of commentary on my side. As of this writing I have spent well over one hundred hours in these tutorial communications, a matter which may help to explain why my patience has worn thin.

A small fraction of this time has, as it seems to me, been well spent. One of the dozen or more issues covered below—that regarding the effect of response set in these estimates—was left unexplained in my original article because of space pressures, and I am pleased to have an opportunity to discuss it here. The other nine-tenths of the time and space has been spent, however, either on direct misreadings of things made clear in my original text that seem to have caused no confusion among astute readers in the profession, or on matters usually sifted clear in elementary statistics courses. I have doubts as to whether any of this has been worth anybody's time, or surely this much space in the *Review*. However, even in its current version the Pierce-Rose piece seems likely to stir up a maze of unnecessary confusion, and hence requires this fourth rejoinder.

I

It is still not clear to me whether the authors quite grasp the purpose of the black-and-white model, as used in the original context. Therefore I would like to begin with some background.

The observations that ultimately led me to write those pieces had originated years before in my own interviewing experience. In that period I had been struck by two facts in particular. One was that very many respondents could not understand that a battery of pure opinion items had no objective "right-wrong" scoring, or that don't know responses were not a confession of the most abject ignorance, to be avoided at all cost. The other was the frequency with which respondents chose a response alternative dutifully but accompanied the choice with side cues (shoulder-shrugging, eye-rolling, giggles, and even *sotto voce* comments) indicating that they were very much out of their element and would pick any alternative haphazardly by way of helping me out. I should make very clear that such events were rare to nonexistent when questions dealt with the immediate terms of the respondent's life, such as whether the respondent was satisfied with his housing or how he would feel about blacks moving in next door. But they began to mount significantly in questions concerning politics, and hit something of a maximum on the national issue items of the structured kind being discussed here. I should also be clear that even these episodes involved nothing approaching a majority of respondents: they remained a limited minority. In addition to those who betrayed themselves in such relaxed fashion, however, there were substantial numbers of further respondents of harassed mien who were obviously struggling to pick an answer and who, if there were the embarrassment of follow-up probes, could muster no self-starting comments whatever on the issue, suggesting that their failure to give a high sign as to the absurdity of the situation was mainly an effort to save face. In short, I came to feel that the tiny proportion of cases —usually one or two per cent—in polls and surveys of the period registered as explicit "don't knows" on items of this type was an underestimate of heroic proportions.

I should add that this is not some idiosyncratic experience of my own. Most professional interviewers are painfully aware of the same phenomenon. Moreover, one of the most persistent themes of disenchantment with survey research that arises among our graduate students who do actual interviewing in connection with the Detroit Area Study practicum is the spectacle of older scholars lacking interviewing experience, who treat tabular data on attitudes as though it were all real and meaningful in the Pierce-Rose mode, whereas the students themselves have become sensitized in the interaction of real interviews to the fact that some fractions of it are simpy not meaningful, as they or I would define the term.

Such observations lay behind one major innovation that we introduced in the 1956 study with respect to the structured issue items Pierce and Rose discuss. Instead of browbeating our respondents into giving opinions they did not feel they possessed, as most interviewers were instructed to do in that period in order to minimize the analyst's "missing data," we explicitly invited respondents who had no opinions on a particular issue to report that fact directly, instead of laboring to concoct some kind of meaningless data point for us.

The result of this supplementary operation handsomely confirmed our expectations. The proportion of self-confessed nonattitudes increased by an order of magnitude over the conventionally-trivial residue of "don't knows" left after strong interviewer pressure to force a response. Even on the two most familiar items of the set of issues, the "no opinions" almost reached 10 per cent. On the issue most remote to the average respondent—the power and housing item central to this debate—the proportion passed to 27 per cent, with a further handful of "don't knows" bringing the total to 29 per cent. There was in addition another 7 per cent who said they were not sure how they stood. Such a high tide of null responses was in fact one of several reasons that I had for selecting this item for more concentrated attention as a limiting case of the nonattitude phenomenon. Some 29 to 36 per cent of the adult electorate of 1956 appeared to have nonattitudes by self-confession on this ssue, despite its centrality in elite debate of he preceding thirty years, and this much was rue without any elaborate chain of statistical nference or filigree of mathematical assumpions whatever.

The other reasons why I became engrossed with this item as a limiting case came into iew once we had cross-time data over three waves from our panel. Basically, these other easons involved a perplexing tension between wo empirical statistics that could be derived from the cross-time behavior of the power and housing *content* responses (the high tide of no opinions set aside). One of these two statistics is handsomely represented in Table 4 of the Pierce-Rose text. There we see that the power and housing issue is also a limiting case in terms of what the authors call the "stable attitude share of attitude variance," and one which is used as justification for the fifth assumption desired for the Pierce-Rose model, to wit: "(v) no attitude change occurs." The labelling of this table is quite misleading, and if we were not thoroughly familiar with the data being discussed, we would be highly perplexed at the results it shows.[1] But we had been using the algorithm the authors call "causal path analysis" long before Heise popularized the relevant formulae, and indeed our interpretation of the Heise-form data squares almost exactly with the meaning attributed by Pierce and Rose to the relevant statistics presented in Figure 4: where the limiting case of the power and housing item is concerned (although not for the other issue items), the cross-time data structure implies that "no attitude change occurs." However, we say "almost exactly," rather than "exactly," because what lies between the estimates extracted by the Heise route and the vastly different entries one would make in Table 4 if its labelling meant what it appeared to (see footnote 1) is the amendment to our consensual interpretation "no (*true*) attitude change occurs."

The second datum about the power and housing issue which seems striking from cross-time observation is that the correlation of *content* responses (keeping aside the 29 to 36 per cent of self-confessed null responses) is as miserably low as it is (about .3). This means that massive change is occurring in these responses from administration to administration. Yet we have just finished agreeing with the Pierce-Rose interpretation of the Heise-form statistics which seem to say that "no (true) attitude change occurs" in the limiting case of the power and housing item. Hopefully the reader will have no trouble in sensing the tension here: there is no change in (true) attitudes, yet massive rates of change in attitude response are registered. We can return to the placid simplicity of the ancient syllogism to resolve this tension. We must conclude that the massive change in responses must arise from something other than "true attitudes." This "something other" in turn must, as the simplest logical matter, be something out in the *terra incognita* of the set of "non-true attitudes," or manifest responses in one way or another disengaged from the realm of true attitudes. This brings us perilously close to the notion of nonattitudes.

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[1] I assume, for example, that most readers would take the footnote phrase of Table 4 referring to "correlations between attitudes on a question in the two samplings" to mean a correlation formed on the set of apparent content responses (ranging from strong-agree to strong-disagree) at two successive administrations. The reader should be advised, however, that the square of the latter type of correlation for the public power item is not 99%, but somewhere in the vicinity of 10%. A number of the other items, like school integration, run higher, although probably none exceeds 25% and few exceed 20%. In other words, the quanta presented in Table 4 are not what most readers would believe, and frankly I recognize them only from my own lengthy work on the problem. Ironically, in using these estimates, the authors are much closer to accepting the logic of the black-and-white model than they appear to understand.

It does not bring us all the way, because I felt there should be at least one more demonstrable property of such non-true attitudes to validate my suspicion that there was an integral connection between the haphazard, meaningless response alternatives I felt I had seen respondents adopt in some cases, and the more general realms of "non-true attitudes." What I really wanted to know was whether people whose responses to the power and housing item did change sides from interview to interview despite the evidence that no true attitude change was occurring *also* showed a random time path in their responses, which is to say, a correlation between responses at two time points which approximated .00.

It was this focussed analytic question that the black-and-white model was invented to solve. It appeared to solve it very nicely. Given the evidence of the Heise type that on the power and housing issue no true attitude change occurred, the set of people who changed from side to side in successive trials would have to represent a pure group of the phenomenon of "non-true attitudes."[2] Such a pure group could be defined on two waves of the panel, saving the behavior in the third wave as an independent test of their cross-time behavior. The crucial stability correlation arising from this test turned out to be essentially zero, as I had come to suspect it would. The remaining group of people who did not change sides on the issue, had to be an impure group of some "true" attitudes and people who by chance took the same side twice. There is no way of splitting this group into two pure types, but the rest of the logic makes it easy to calculate the proportions of the mix in this group, and hence the stability correlation between the second and third wave that should pertain *if* such a mixture of stable true attitudes and persons showing only a random time path in their responses, in known proportions, were present. The test showed almost exactly this correlation. Hence I was willing to conclude that in addition to the 29 to 36 per cent of the sample with *self-confessed* nonattitudes on the power and housing issue, the remaining majority of content responses were themselves strongly adulterated by a contingent responding "as if" randomly across time, and hence who might reasonably be coded as "nonattitudes" as well. It was when all of these were added in to the self-confessed nonattitudes that my estimates of the prevalence of nonattitudes in this limiting case rose as high as they did.

These results were corroborated by another test of somewhat different form suggested by Wiggins. The Wiggins test also is appropriate only if there is no true change in attitude. It takes the configuration of data from three waves at the same time and estimates the response probability between "agree" and "disagree" of those members of the sample who do not have "true" attitudes leading to probabilities of .00 or 1.00 of choosing the agree response. The probability estimated from the power and housing issue was .52, or very close to the expected 50-50 toss-up. The Wiggins test has one advantage over the black-and-white test, inasmuch as its estimates are invariant whatever the actual temporal sequence of the waves is taken to be. However, like the black-and-white test, the assumption of no true change in attitude means that the Wiggins tes is not applicable beyond the single issue o power and housing.

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[2] Earlier versions of the Pierce-Rose manuscript were extremely exercised over my decision to work with this dichotomization of the sample. The authors felt that once into a partitioning game any change of position, even from "strong" to "weak" on the agree side, must be counted as a nonattitude. They also insinuated that by using a dichotomy in the critical correlational test, I was trying to hide a non-fit that would have shown through because of an edge of stability in the weak-strong choice within a side of the issue. The latter allegation was easy to dispose of, since in fact the correlational test was *not* performed dichotomously, but rather on the four-by-four table using the strength differentiation. This left the authors complaining about the original dichotomy, and in my third review I laid out my several reasons for deciding this was the wisest procedure. However, this current version of the Pierce-Rose manuscript, revised with those reasons in hand, seems to have largely if not completely subsided o this issue. Therefore to save space, I will not reiterat those reasons here, but I would be glad to mail anybody concerned about the matter a copy of my thir review.

Two further observations about the test with the black-and-white model deserve stress. This stress is not new: the same emphases were given in the original "Belief Systems" article. But one of the reasons why this private tutorial has been as long and costly as it has is because Pierce and Rose have insisted on ignoring these points. In the original presentation I spent two or more paragraphs trying to make as clear as I knew how that the use of the black-and-white model hinged on (1) side evidence that no change in true attitudes occurred; and (2) goodness of fit of the data when given an independent test with the third wave. Now it is clear from the Pierce-Rose Table 4 that the evidence for the first condition becomes shaky once beyond the power and housing issue. I pointed out that this was so, and counselled against blind application to other items. I did myself experiment with a couple of the further issue items that most nearly approximated condition (1), and found that they looked correspondingly shaky on condition (2); hence I dropped the matter with the extended counsel against stupidly overapplying the model to other items. The problem is a straightforward one: the black-and-white model is extremely simple, and once there is reason to believe that some observed change in responses is true attitude change, the situation is underspecified and blind cranking with the model is likely to produce nonsense estimates.

Pierce and Rose set out originally with this broadcast application of the model, even to panel items beyond the issue set, and took great glee at the nonsense results generated. They attributed these nonsense results to failures in my own thought and methods, and used them to cast doubt on the meaningfulness of the results even in the case of the power and housing issue. Most of these nonsense results have been deleted in shortening the article to its current version, but a few remain and constitute some of what might appear to the casual reader as the more "telling" points the authors muster.

The second observation to be stressed is closely related and involves the power and housing issue as the limiting case of the nonattiude phenomenon. We may recall that it was not only the limiting case for stability of true attitudes (Table 4), but also the limiting case for prevalence of *self-confessed* nonattitudes, across this issue set. Further work with the black-and-white model implied that it was probably also a limiting case of *concealed* nonattitudes as well. I treated it very clearly as such a limiting or extreme case. I pointed out, and still firmly believe, that the demonstration of substantial numbers of concealed nonattitudes in the power and housing case very likely meant that there were also substantial numbers of concealed nonattitudes on the other issues as well, although I should make clear here that logically they must be fewer. I pointed out that with the underspecification problem the black-and-white model could not provide us with estimates of these lesser numbers, so that the argument had to remain intuitive. However, the possibility that all instances of concealed nonattitudes occurred on the power and housing item and none whatever occurred on any of the other similar items in similar format seemed wildly implausible.

I am a bit sorry that I failed to rough out some estimates as to nonattitude prevalence on the other issues simply by way of making clear my own sense that of data available, the power and housing issue was in fact a limiting or extreme case. However, it is obvious that it was so. For example, some of the more familiar issues had self-confessed nonattitude rates 20 per cent below that for the power and housing issue, so that even if everything else were equal, the estimate of nonattitude prevalence for these items must necessarily be cut back from the extreme in the neighborhood of 70 per cent to something closer to 50 per cent. But everything else was not equal: some of these issues had higher stability correlations across time than the power and housing issue, and hence fewer concealed nonattitudes in addition to the fewer self-confessed ones; also there was for these issues greater evidence of true attitude change, reducing by a still further margin the potential pool of concealed nonattitudes.

While I did not feel empowered to make such estimates beyond the limiting case of power and housing, I did think that I made the limiting status of that item clear by spending large conceptual sections of my article attempting to formulate what accounted for the range of variation in prevalence of nonattitudes as one moved from one type of issue to another. This was the whole motivation for the introduction of explanatory constructs such as the varying centrality of content to the individual, variations in information, group stereotype, and the like. Perhaps I do not write clearly, but it does not seem to me I would have spent so much effort trying to explain the range of variation in nonattitude prevalence below the limiting case set by the power and housing issue if I believed that this range was absent or, for that matter, even fairly small. Yet this is another point that has consistently eluded the current authors, despite several rounds of my pointing it out, and the obviousness of the original text.

We have not begun to cover all of the topics on which Pierce and Rose seem reluctant to get beyond the kind of "maximum possible misunderstanding" they originally developed. However, we can deal with further of these if we move to details of their current text.

II

The abstract of the Pierce-Rose article summarizes the main finding of the piece: "The conclusions reject the thesis that only 20 to 30 per cent of the American public has true attitudes (on issues of public policy). . . ." One must read into the text to find out whose thesis this preposterous statement is supposed to be, but it is alleged to be mine. I have tried in the few preceding paragraphs to make clear why such a sweeping statement departs widely from any general thesis I have developed. At first I thought this attribution was an honest product of sloppy scholarship. I now rue the time I have spent trying to explain through this tutorial why it is inaccurate, since it has become clear that the authors are not very interested. I think I have explained my own position well enough above, and shall not belabor the point, although it seems more basic to the debate than it should be.

It will be useful further background for many later points if we review the outlines of the Pierce-Rose conception of attitudes. The key supposition is that a "true and stable attitude" is not a point, but a range on an underlying attitude continuum. In such a conception, different manifest responses to the same item by the same individual are no sign of any attitude unreliability or instability on his part: if anything, they help to show that true attitudes are in fact ranges, not points.

The conception of such a range at a latent level is closely akin to what James S. Coleman has used in his analysis models for panel data, although he has elaborated it further and geared its assumptions more appropriately to survey data than the Pierce-Rose proposal. It is a conception I have always found attractive, within limits, although it figured only indirectly in the black-and-white model. Perhaps its most attractive feature is that it can in principle address the problem of differential crystallization of attitudes. In speaking of nonattitudes, I had in mind a degree of crystallization which would stand at or surely close to zero. At the other extreme, one can imagine persons with attitudes so sharply crystallized on a particular topic that if the truth were known, their attitude range would be shrunken if not all the way to a single point, at least close enough to that state that the coarse response categories of survey instruments could find no range at all. But these are hypothetical extremes, and it would be reasonable to expect most people most of the time to lie somewhere between these poles on issues they know about, and truly to have a meaningful attitude *range* in exactly the Pierce-Rose sense.

When I say I find this conception attractive within limits, one limit comes when one asks whether there is any range so large (from one end of an attitude continuum to its polar opposite, for example) that it may be rather misleading to say the individual has a "true and stable attitude" toward the competing alternatives. Surely the original ascendancy of the attitude construct in social psychology hinged on the assumption that many important attitudes were crystallized to reasonably narrow ranges and were stable within those ranges, so that they could be used as tools to predict other choice behaviors. If any range on a position issue, be it ever so large, is expressive of the same true attitude, then the predictive power which originally justified the construct has evaporated, outside of situations where, empirically, attitudes are in fact of short range. Moreover, the whole rationale for studies of attitude stability is definitely snipped away by such a view, although only in a rather shallow semantic sense. In other words, if one attitude shows high stability within a set of individuals over time and another shows very low stability with the same index, it is not that stability differs; presumably all ranges have the same stability, and it is the *length* of the ranges that is hypothesized to be varying. Perhaps that change of label gains something, although it is not entirely clear to me what it is. Or again, in a practical political context, perhaps if a person with a wide-range attitude on a public referendum eagerly votes pro on Sunday and eagerly con on Tuesday, it is desirable to say that both votes are expressive of the same true and stable attitude. This usage, however, would seem to have obvious drawbacks.

Most of the charm of the range concept, which to our mind lies in variations from person to person in attitude crystallization or attitude range, is however promptly removed in the Pierce-Rose model by the assumption that "(iv) all respondents have the same attitude range." This assumption is quite unsettling exactly because of the evidence on the power issue from the black-and-white test, which made the point that the response-giving part of the population was almost maximally *heterogeneous* in terms of attitude crystallization. One subset was sharply enough crystallized in its opinions not to stray at all from "agree" or "disagree," while the other subset was so uncrystallized as to turn up on any side in repetitive trials, in a pattern of statistical independence over time. Yet the superstructure calculations of the Pierce-Rose model seem to rest on a foundation assumption of perfect population *homogeneity* in these regards, an assumption already known to be thoroughly untrue. Moreover, the authors propose to reexamine the heterogeneity results of my tests with this new model which denies by assumption in advance that such heterogeneity exists. I am open to persuasion that such an exercise has some internal logic, but it continues to escape me.

With the general conception of attitudes involved in the Pierce-Rose model reviewed, we can turn to some related issues that have helped to perpetuate this debate over many months. We have had a dreadful time, for example, with the concept of randomness. My demonstration of a "random" time path for the responses of some respondents led the authors in an earlier version to charge that by imagining these responses were "uncaused" behaviors, I was undermining the deterministic foundations of social science. So I was obliged to explain the obvious: all sorts of processes leave random tracks, including the outcomes of coin flips, without in any ultimate sense being "uncaused." Indeed, the short-term force component figuring so prominently in the Pierce-Rose model has some random properties, as does more conventional measurement error. Yet every error has some proximate cause; the important point is that the list of such proximate causes is enormous and unrelated to the object of measurement itself—just as the Pierce-Rose "short-term forces" are by explicit assumption independent of the attitude itself—so that random properties can often be assumed. The response choices made to survey items by people to whom the content means little or nothing may show random properties across time as well, but I never imagined that they were uncaused in any ultimate sense. The random pattern arises precisely because all sorts of causes irrelevant to the intended content of the item are shaping the response.

When we put together these observations with the earlier discussion of large attitude ranges, we can begin to see the sense in which much of this debate seems chiefly semantic. I would scarcely feel that I was a traitor to my original argument if I were to rephrase it as follows, trying to adopt the Pierce-Rose vocabulary preferences: "The proportion of Americans whose attitude ranges are incredibly large on this set of issues, and whose manifest responses are strongly influenced by short-term forces with random properties independent of attitude content, is vastly greater than most political scientists have supposed, given the customary tiny per cent of Don't Know answers." Such a change of phrase does nothing, of course, to change the real-world evidence.

No sooner were we over this first small hurdle involving the nature of randomness than I discovered the authors were laboring under the delusion that random time paths (statistical independence, or a correlation of zero between trials) presupposed the equiprobability of states or choices at any given time point. In fact, they do not, as is readily apparent if one contemplates successive flips of a biased coin. Ironically, I had gone to some pains to *avoid* a model which would be tied to an assumption of equiprobability, because I was well aware that other known properties —most especially response set—stood in direct violation of such an assumption. The black-and-white model was useful in this regard, although Pierce and Rose in their several manuscripts have had much fun showing how badly the data meet the equiprobability assumption I succeeded in avoiding, and persist in implying that such demonstrations invalidate the test rather than underscore their own state of confusion.

The problem of response set is a serious one in the 1956 data, as discerning scholars have discovered. Moreover, it creates mischief for both models. I wrestled with the problem for some time before organizing the black-and-white model so that it could at least co-exist with the known presence of these tendencies. The Pierce-Rose model does not recognize the problem at all, and more than one of its key assumptions are thereby compromised.

One demonstration of the seriousness of the problem comes from our most direct experiments with it in 1956. At Richard Christie's suggestion, we included F-scale items of the standard kind in which an "agree" response typically means an "authoritarian" one. We also included some item reversals, such that an "agree" instead meant the nonauthoritarian response. Scores based on the two batteries of items not only failed to show any positive correlation in content terms, but actually showed a significant *negative* one: people who ranked as relatively authoritarian on one scale ranked as relatively nonauthoritarian on the reversed version. Of course the tendency to agree or disagree to such items regardless of the direction in which the content was keyed thereby was significantly positive: response set actually outweighed the evaluation of content! With such data as these, it would be interesting to know what levels of "true and stable content-related attitudes" the Pierce-Rose model would demonstrate for each authoritarian item in the two batteries, had we repeated them over time. My guess would be that such estimates would be amusingly high, and directly belied by the other facts known as to internal inconsistency.

Space forces us to a very abbreviated discussion of the response set problem. There are in fact several kinds of response set, including a tendency to choose the "strong" alternative rather than less vehement ones. However, the dominant effect is a tendency toward acquiescence or agreement with "agree-disagree" items. One reason why such a response set thrusts itself forcefully on this debate is because it is probable that extraneous factors like question form intrude most sharply on responses where attitudes are least crystallized (or, in Pierce-Rose terms, have the widest true range, even if their model fails to permit such variation). This means in turn that the Pierce-Rose hope that "precision of attitudes is not related to the location of attitudes," necessary for the "convenient" assumption (iv), is probably quite wrong. Moreover, the Pierce-Rose assumptions (ii) and (iii) hinge on the proposition that error terms unrelated to meaningful attitude content are mutually compensating (mean of zero), which of course is exactly what is not true of data vulnerable to response set.

I did not attempt to represent my own thinking on the response set problem in the few brief pages I originally gave to the black-and-white test, so I have no desire to fault Pierce and Rose for their thorough-going misunderstanding of the subject, at least up to the point where I reviewed that thinking for them. I was saved from making obviously inappropriate assumptions by the fact that a model does exist which preserves the independence of successive trials without the assumption of equiprobability at any given trial. This model, in the familiar coin-flipping metaphor, is as I have suggested the one which admits coins of varying bias (p), with the unbiased coin (p of .50) as but one special case. Thus an item subject to particular response set tendencies favoring an "agree" response, as many of our items show, could be seen as behaving for people without true attitudes like a coin biased toward the "agree" side by some degree (e.g., p of .60 or .70), but still showing a random path of responses over time.

I did not get into these depths in my article chiefly because of an editor already impatient at the article length. But there was another simpler reason. The power and housing issue changers whom I was considering as a successful isolation of respondents with random cross-time responses had initially split about 53–47 in their initial (or, equally, second-wave) choices between agree and disagree. For the N involved, this division could not be reliably distinguished from 50–50, or the simpler unbiased coin model. Therefore it seemed less than imperative to introduce the niceties of the broader model. If the ultimate testing which introduced the third-wave data had obliged me to commit myself as to just how biased the coin-flip was in the specific instance (i.e., how many more nonattitudes were hidden in the stable-agree cell than in the stable-disagree cell), I would have been in trouble. Happily, however, the correlational format of the central test, along with a focus on the crucial set of changers, relieved me of that problem. Given all of these circumstances, I decided that the space cost of introducing response set problems and the biased coin model outweighed the advantages. But contrary to Pierce and Rose, the equiprobability assumption is not any intrinsic assumption of the model.[3]

III

I mentioned at the outset that while the current third-round version of the Pierce-Rose test has purged itself of a number of the most glaring errors that plagued the first version submitted for publication, it remains rather dense in statements which are misleading or even more purely inaccurate. I think I have some responsibility to document this charge, so let me start with the paragraph on page 632 and proceed, as succinctly as possible, sentence by sentence.

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[3] The authors' response to criticism of the vulnerability of their model to response set has been mainly made up of the addition of Appendix II, in which they set out to show that the black-and-white model does not perform well with items where sharper response sets than the power and housing item encounters are involved. The Appendix is largely irrelevant, since these other items are the ones where the black-and-white model should not be used in any event, not because of response set intrusion, but because of failure of these items to meet the "no change in true attitude" assumption necessary for the model. Moreover, a crucial proposition in that Appendix—to the effect that the standard form for the five issue items should mean a standard degree of response set—is well-known not to be true among scholars who have looked into these matters at all closely. In general, this Appendix is one of several reasons why I have had trouble building confidence that the authors really have much understanding either of the black-and-white tests or of the several deeper measurement issues involved.

"Converse's methods are an insufficient basis for accepting his thesis because they do not lead to an explanation of responses, they do not exclude alternative accounts, and they do not seem to be replicable."

> *Comment:* Parts of the statement are true but misleading. My treatment did not pretend to "explain" responses in any rich sense of the verb, nor did it purport to exclude alternative accounts. The statement is misleading in context, however, as the authors have promised a superior model and the strong implication is that these are invidious comparisons with their own model. Yet where explanation is concerned the texture of their model is exactly the same as mine: a mapping of observed data which leaves a strong fraction of determinants to "short-term forces" with random properties. Where the exclusion of alternative accounts is concerned, no model can claim this under ordinary circumstances, and the Pierce-Rose version surely does not. Where replicability is concerned, the charge seems serious since the authors have already pointed out they are working the same data I used, but later in the paragraph we learn that they do not mean "replicability" in a strict sense, but rather than when the test is extended to other items with properties that do not match its assumptions, it gives nonsense results, as I said it would.

"For example, the 'Black-and-White' consistency test is used in exactly the same form by Philip Converse to demonstrate the *lack of attitudes* and by James Coleman to demonstrate the presence of *attitude* and *measurement error*."

> *Comment:* This observation makes me despair, because it was I in an earlier review who urged the authors to relate their model to the ground already well-ploughed in this area, mainly by Lee Wiggins and James Coleman, although more recently by Leo Goodman. I am now sorry I asked, because it seems clear that the authors have skimmed Coleman without much understanding. The first problem in the statement is a factual error: the two approaches are in no sense "exactly the same form." The spirit is similar, but the form is dramatically different. Quite apart from this difficulty, the common spirit is a partitioning of data into true attitudes and "noise", and there is no reason to believe that the two approaches would arrive at substantially different conclusions concerning the power and housing item, save that my model is better suited to smoke out population heterogeneity in these regards than is the Coleman model as commonly used.

"In Converse's approach, the main variable—attitudes—is operationalized as a residual category (what is left after other variables have been accounted for) . . . As Converse neither specifies his other variables nor justifies theoretically the treatment of attitudes as residues, alternative accounts . . . are just as acceptable. . ."

> *Comment:* The simplest reason why I fail to specify "other variables" is that no other variables are involved, any more than they are in any given "running" of the Pierce-Rose model. The two models are entirely equivalent in these terms. Hence the "residue" argument is mainly mystifying.

"The attempts to replicate his test on other questions failed, and the operating criterion for the appropriateness of attempting replication appears to be: if the test shows nonattitudes, it should be used; if not, it is inappropriate."

> *Comment:* The criterion, which the authors have never understood, has gemlike clarity. The model is to be used where its assumptions fit other facts known about the data configuration, and should be avoided where such fit is absent. One assumption is "no change in true attitudes." This is true to all intents and purposes for the power and housing issue, and clearly untrue for the other four issues (see Table 4). Where assumptions have changed from appropriate to inappropriate, the charge of "failure to replicate" borders on the absurd.

Proceeding sentence by sentence is too costly a business, so let us continue more efficiently, point by point. The authors go on to develop "four basic ways" in which my "method" fails to fit the data. The first point elaborated in this bill of particulars hinges on the ascription to me of an equiprobability assumption, and is mainly a throwback to the authors' earlier confusions as to what I was and was not assuming.

The second point is perplexing. The authors show a two-wave cross-tabulation of the power and housing item for the total sample, and claim that it violates my model in its "incremental" variations and evidence of an underlying response continuum. All of Table 1, including the incremental variation, was summarized in my original report as a positive correlation of about .3 for that issue in the total sample, and was one of several pieces of evidence which, when laid side by side, prompted further thought and ultimately the critical test itself. The evidence in Table 1B seems to be another throwback to the time when the authors assumed I had collapsed "strong" and "weak" categories in making my critical test, an assumption which is simply wrong.

The third point is ingenious, but irrelevant in an important sense, as I have pointed out in earlier reviews. The authors complain that I overestimated the proportion of people with nonatttitudes, or that I arrived at my estimates by some vague method of residues which assumed that every response which could possibly be labelled a nonattitude was so considered. Yet my set of identifiable potential nonattitudes numbered 144 cases on the power and housing issue, whereas the authors' purported efforts to examine the same cases for stability and relationship with other issues involves some 348 cases, or a multiplication of nonattitudes by a factor approaching 2½! It is not hard to see what is happening here. The authors draw in a large set of people whom I did not consider as nonattitudes, and then use the traces of stability which these additional people produce (up to a correlation of .17, rather than my original near-zero), by way of showing that my diagnosis fails to fit the data (Table 2). Presumably the same abuses are occurring with respect to Table 3, although in this instance they do not even show the data for the one item appropriate to the black-and-white model, so the display is irrelevant to the discussion in a double sense.

The fourth point suffers many of the same difficulties. Again we are dealing with five issue items, four of which are irrelevant, so that "deductions" and "expectations" purportedly based on my reasoning are necessarily going to prove wrong. In addition, we are dealing with a set of "nonattitude respondents" larger by a factor of two or three even on the single relevant item than any I ever employed, and I presume the same misdefinition is ardently pursued into the four irrelevant questions. Therefore we have already adulterated the effects with which I was working by a factor of about ten (2½ times 4) to one. And, as though this were not enough, we shall make the crucial statistics depend on the "predominance of 'agree' responses," a tactic which enables the "agree" response set on the four irrelevant items to lend a further compound of negative evidence. At the end, we can talk of "not one chance in a thousand" that an assumption which I cannot recognize in the form stated, is correct. This is surely damning.

I should make clear that the oddities which persist in other parts of the text are not as densely packed as in the few pages I have just discussed. But quite a number remain and we cannot hope to discuss them all. One other matter involving the assumptions of the Pierce-Rose model is the need to attach a scoring system to the response categories in order for the model to operate. The prime demonstrations justifying the particular scoring adopted surround Table 5 and seem enormously impressive. However, the high correlations reported in that table are mainly due to the fact that these issue responses tend to be "lumpy"—certain categories drawing large proportions of responses, and others, small proportions—and these lumps reproduce themselves faithfully from wave to wave. In general, turnover tables will show "category stability" as the authors measure it to be very high for popular categories and much lower for unpopular ones. The standard way of removing this artifact in order to ask the kind of question the authors want to pose is to measure category stability in terms of departures from expectations of the chi-square type. The authors leave the size artifacts in their category stability measure, however, and since their scoring system is mainly a function of category size, seemingly impressive correlations result, although they approach the tautological rather than being any serious test.

The operation of scoring the categories is not as unsettlingly inappropriate to the data as some of the other key model assumptions, and I find such experiments of interest. The scoring does rest on the assumption that underlying true attitudes are always normally distributed, an assumption that flies in the face of many observations of attitude dimensions that polarize in response to relevant social crises. However, such polarizations are not obvious in the data from these five issue items in the 1956–1960 period, and hence I would rate the scoring used as no more than a small strain on other things known about these data.

What is puzzling is that the authors have insisted even through the current version of the manuscript that my black-and-white test requires the same operation of "pinning numbers on categories." Of course it does not, and since the difference has been explained and the authors persist in their contention, I am obliged to conclude that their grasp of "levels of measurement" distinctions is also remarkably weak. In the critical test I used ordinal correlation measures which fit very nicely the uncontested ordinal properties of the attitude categories involved. And the only other part of the operation involved the initial dichotomization, which requires no manipulation of numbers whatever. The authors will find that my results are invariant whatever cardinal numbers they might wish to assign to the two groups, be they zero and one, one and zero, 347 and −40, or any other conceivable pair of real numbers. Once again, every step of my test was carefully geared to all of the known properties and deficiencies of the data being employed.

IV

Near the close of their discussion, Pierce and Rose draw the following summary comparison between my black-and-white test and their "probability model."

> "The probability model possesses more general advantages. It is applicable, with or without modification, to a wide range of questions, as indicated here by the results for the five domestic policy questions. . ."

We should also like to close with a few comments on this proclamation of broad applicability, which we find quite surprising.

At base, I do not understand what "applicability" means to the authors. It is obvious that most models of the type under discussion can accept a batch of empirical data as input and, with a turning of the crank, give some kind of numerical results back. Surely "applicability" means more than this. Some few models—the Coleman is a case in point—give handy warning signals as to their inapplicability in special cases by generating output numbers that are obvious nonsense, such as probabilities that are negative or greater than one. Other models, like the two being compared here, are unlikely to make their own nonsense so obvious, yet both if misapplied can provide results that are nonsense nonetheless. Indeed, it is this kind of hidden nonsense that led me to stress in the original article that the black-and-white model was inappropriate to all but the power and housing item.

To my way of thought, a model becomes "applicable" to a given set of data once it can be shown that (1) its assumptions are tailored to known properties of the data; and (2) there is additional evidence of "goodness of fit" in predictions to further relevant bodies of data. Unfortunately, this is not a situation where half a loaf is better than none: both types of fit must co-occur if a claim of "applicability" is to be certified in any special case.

I have thus far said nothing about the sec-ond criterion, or the Pierce-Rose demonstrations of "goodness of fit" for their model. The authors are pleased with these data (Tables 6, 7 and 8), and it is safe to guess that most casual readers will find them quite dazzling. In Table 6, for example, about the poorest prediction is the one associated with the 1958–1960 transition for the power and housing issue. Yet even here we see a value of 90 per cent of "respondents correctly placed," and that seems to leave remarkably few respondents *mis*placed.

I do not find these demonstrations at all impressive, and it is important to convey why. What most readers will fail to understand about a datum like the 90 per cent is that (a) it involves a type of aggregative prediction that does not purport to assign individual respondents to any particular time path of responses; and (b) these items are remarkably inert in their aggregative contours over time. When these two facts are combined, it means that seemingly "bad" predictions would be almost impossible to generate in this context.

Where the former is concerned, the authors are merely trying to predict the aggregate shape of responses for a certain time period, given knowledge of their shape at earlier periods. This is perfectly legitimate in itself, although readers not close to these problems would be astonished to know how numerically inflated such aggregate predictions will necessarily be, relative to their individual-level counterparts. Although the Pierce-Rose text is very clear as to the aggregate nature of these demonstrations, the Table 6 heading "respondents correctly placed" (a phrase usually reserved for exercises like discriminant function analysis where individual assignments are actually made), serves to blur the issue.

The second point, which involves the difficulty of getting poorer absolute results from this kind of operation with data as aggregatively inert as these turn out to be, may be made vivid with a simple experiment. I have taken the 1956–1958 cross-tabulations on the power and housing issue in the same five-by-five form erected by the authors for the purposes of Table 6, and used these twenty-five percentages to "predict" to the aggregate contours of the parallel 1958–1960 version of the table for the same issue. Instead of the 90 per cent predictive success cited for the 1958–1960 test of power and housing in Table 6 when the full Pierce-Rose model is brought to bear, I find 91½ per cent of "respondents correctly placed" using, as it were, no model at all!!

This is a far cry from the closeness of predictive fit achieved by the black-and-white test for a much more surprising prediction. Hence we conclude that it is charitable to say that the Pierce-Rose demonstrations of "goodness of fit" are indifferent or mediocre.[4] However, as always with such tests, there is a zone of ambiguity as to how "good" any goodness of fit must be to legitimate a claim of applicability. Therefore let us return to the first criterion—the appropriateness of initial assumptions—where there seems to be no ambiguity at all.

It has been almost impossible to convey to the authors that the black-and-white model was not suggested as a general analytic routine for panel data in the sense the authors claim for their own model. The current text continues to misuse it beyond its proper limits for purposes of derision, all the while developing the new theme that it must be inferior because it is not more broadly applicable. It was not suggested as a method, but rather was developed as a finely-honed tool to answer a very specific question that could be asked of a particular cross- time data structure displaying certain peculiar properties. Since I anticipated intuitively surprising results, I labored very hard to stay within a set of assumptions that violated no known properties of the issue item involved. Only one decision necessary for the operational implementation of the model (*not* a model assumption as such) was even particularly arbitrary, and that involved an *a priori* commitment that changes of strength of agreement or disagreement on the power and housing issue did not have the same significance, at least for people with true attitudes on that polarized elite issue, as a full change from *pro* to *con*. All other assumptions and procedures were unquestionably well tailored to the known properties of the data.

This cannot be said of the Pierce-Rose model. We have seen that in numerous ways the assumptions of their model strain the data, and at a few palpable points stand in the most direct violation of known properties of the data, including, where the power issue is concerned, the heterogeneity property demonstrated by the careful black-and-white test. Actually, in such component facts as respect for the ordinal character of the data, the black-and-white model is *better* suited to the texture of *all* of these issue items than is the Pierce-Rose model. The only reason why I took pains to limit its use to the power and housing item alone was because the model assumed no true attitude change, an assumption clearly violated by the other items. What is perplexing is that the Pierce-Rose model makes exactly the same assumption, yet this does not deter the authors from proclaiming their model to be more broadly applicable! Clearly the authors and I are operating on dramatically different wave lengths.

None of the above should be taken to mean that I consider the Pierce-Rose model as useless. I think it is worth experimenting with more powerful measurement assumptions, and I suspect that some of the differential statistics that the Pierce-Rose model may generate from item to item are worth heuristic attention. On the other hand, where its results appear to collide in significant degree with the conclusions from the black-and-white test on the power and housing isue, I do not have much trouble knowing which outcomes I would prefer to trust.

The broader point of the Pierce-Rose essay is to "rehabilitate the voter." If the authors truly believed me to be saying that only 20 or 30 per cent of the American populace has real attitudes on any political issue, as opposed to the limiting case of power and housing in 1956, then the voter would indeed be in direst need of such rehabilitation. But this was not the point even for the 1956 materials, and a broad range of data from the later 1960's and early 1970's seem to imply that attention to politics and the crystallization of attitudes have advanced markedly in recent years. Thus it seems likely that the power and housing item was not only a limiting case in the 1956–1960 period, but probably holds some kind of record over a broader period as well. This is not to say that my original interpretation of the 1956–1960 issue responses was in any way defective. Nor is it to say that survey research analysts are not well advised, even in the current period, to keep in mind the likelihood that some fraction of apparent content responses are not very meaningful. However, the range and central tendency of nonattitude prevalence on such items has probably receded significantly. In short, the voter has been rehabilitating himself, and this is much the better way.

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[4] Given what we have said about the "lumpiness" and cross-time inertia of these items, the reader should have no trouble seeing why the other tests of "goodness of fit" (Tables 7 and 8) are no more impressive than Table 6.

Rejoinder to "Comment" by Philip E. Converse

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The seven or so ideas discussed below will, we believe, aid readers in evaluating our article and Professor Converse's comments.

Interval Scoring

We assume that attitudes and responses occur along an interval dimension. If this is a reasonable assumption, the effects of Converse's collapsing of categories should be fairly predictable: the new groups should be a weighted average of the original categories. If the dimension is merely ordinal, however, the change should have no effect except within the new groups, for the rest of the ordering remains the same. One way of testing which assumption is appropriate is to compare the stability of persons who at some trial respond "strongly agree" but never respond "weakly agree" with the stability of persons replying "weakly agree" but not "strongly agree." As both groups have the same ordering for "agree, disagree" responses but different interval distances from "disagree" preferences, the presence of short-term random forces should produce the same proportion of respondents switching sides in both groups if categories are merely ordered; it should produce more switches from the "weakly agree" group if the dimension is interval. Since fifty-one per cent of the "strongly agree" group stays on the same side while only 13 per cent of the "weakly agree" group remains on the "agree" side, the interval assumption is confirmed. For equivalent groups of "disagree" respondents, results are similar, twenty-three and three per cent stability respectively. The "depends" category fits neatly along this interval, as suggested in our article, with stability equal to that of the "weakly" groups when it is counted as an "agree" or "disagree" response. The interval nature of responses can easily lead to errors if only the ordering of responses is considered.

While instability in a merely ordinal scale is simply instability, as no valid distinctions can be made among degrees of instability, instability along an interval scale is multivalued, depending on the distances between categories for the values. Prior to assuming that the scale was interval, our article reviews response stability in a strict ordinal framework, which Converse quite rightly points out is inappropriate. We agree: all instability is not the same, and various degrees of instability should not be lumped together—because the scale is interval. Respondents remaining exactly stable for any pair of trials average correlations (tau beta) of .48 with the third trial responses; if persons merely remain on the same side, correlations average .33. If, as in the black-and-white model, these two sets of people are lumped together and averaged, the result is a frequency-weighted average of the two groups' correlations, .43. This is not characteristic of all respondents but is an artifact of the grouping procedure. Grouping treats distinct sets of responses as though they were equivalent, which in mathematical language means giving them the same value and more colloquially translates as pinning the same number on different categories. The grouping shapes the results even if the analyst separates the procedures into steps: groups, waits ten minutes, then uses the grouped data in a further calculation.

We have reported ordinal correlations rather than interval correlations whenever ordinality is an issue. The ordinal correlations only capture part of the associations because they only include part of the interval relation between responses. Thus, on the public power item, the contrast in 1958–1960 response "correlations" of less than .3 (tau beta) and over .7 (Pearson product-moment) traces mainly to the omission of interval information about the responses.

Before employing the interval correlations, responses must be scored on an interval dimension. Converse validly indicates that size is related to our scoring system, but he implies our correctly predicted correlations between scores and response stability are tautologically determined by random marginal expected stability. If the randomly expected stability is removed entirely, the category widths correlate with the stability of the remaining responses at $r = .98$ (Pearson product-moment), as we expected. While the procedure makes no theoretical sense, it does indicate that the scores are validly related to the data because they reflect the interplay of random forces and attitude distances in producing responses.

Attitude Ranges

We believe attitudes are ranges on the interval dimension. These ranges need not be specified until attitude change is introduced, attitudes of different generality are compared, or adjacent and overlapping ranges are collapsed into a broad group. The operational form of the probability model in our article is faced with none of these problems, so range is left unspecified.

By assumption, the ranges match or exceed the short-term forces, which are themselves measured only in terms of the population diversity of mean attitudes, a primary and unmeasured quantity. Empirically, the short-term forces and thus the measured aspects of the ranges are less than the population attitude variance on all five domestic policy questions. In terms of response categories, the largely consensual opinions on fair employment, public power, job guarantee and school aid items will make small ranges appear relatively large, because range is at most measured against the consensus, while the less consensual questions such as school integration will make the same ranges seem to be small measured against large diversity.

Converse's discussion of how "large" or "narrow" attitude ranges are is not based on any measurement of ranges or any supportable inferences about their size. As the black-and-white model does specify ranges, incorrectly, we note in passing that Converse allows wider ranges on the public power item than any we hypothesize for the probability model.

For clarification, we have measured short-term forces (and, by inference, ranges) by taking response categories as absolute standards, a procedure with technical problems but at least producing actual estimates. Converse implies that the length of ranges will match "no opinion" response frequencies, response stability, and attitude consensus in the population; none of these suggestions is correct. While our estimates are not firm enough to be conclusive, they will stand until better estimates are made.

Converse appears to believe that we accept his original black-and-white model results as conclusive proof that there are two sets of respondents with completely different-sized attitude ranges. No. We reject the black-and-white results and indicate that the data best fit the assumption of one, not two, set of respondents with the same, not different, size attitude ranges.

Random Short-Term Forces

In both models, random short-term forces move respondents who switch sides from one trial to another. The test for random changers used by Converse is essentially a test of whether the sequence in which responses are given is important. Without attitude change, sequence should not be important, so respondents proceeding from "agree" to "disagree" responses in two trials should be equivalent to persons moving from "disagree" to "agree" responses. If sequence is not important, the third trial responses for the two groups will show an equal distribution of responses. Both models agree that attitude change does not occur on the public power item, hence both predict that sequencing is not important for dichotomized responses.

When the test is extended from dichotomized responses to ordered responses, as in the black-and-white version, the models give slightly different predictions. While Converse continues to expect that the groups will not differ, and thus that there will be no measured association between 1958 and 1960 responses, we would expect some association to occur as a result of strong-weak differences, though this association will be largely buried in the overall figures. Specifically, the probability model predicts an association of .04 (tau beta) between a changed response and a third trial. The average result is in fact .04, rather than .00 as predicted by the black-and-white model.

The dynamics of association for side-changers depends on whether responses are anchored by attitudes or not. If responses are unanchored, strong-weak distinctions are randomly arrived at and hence not important for prediction; if responses are based on attitudes, then strong-weak distinctions in responses do index probable attitude differences. To directly test whether it is the anchoring aspect which causes the association, we can isolate strong-weak distinctions (while controlling for possible response set and the amount of random forces) by comparing persons who respond "weakly agree" and "strongly disagree" with those who respond "strongly agree" and "weakly disagree" in two trials. The probability model indicates that on the third trial the first group should tend to have more "disagree" responses than the second group. In every pairing of trials, the prediction is correct, with an average association (tau beta) of .22 between having the more "disagree" responses on the first trials and having more "disagree" responses on the third trial. The side-changers are not random, they do not fit the nonattitude hypothesis and they differ from randomness in an amount and in subgroups predicted by the probability model.

The two models also differ on how many respondents are subject to random influences; we say all of the sample consistently giving substantive responses is subject to such forces, while the black-and-white model exempts about 40% of such public power item respondents from random influences. As the random influences account for all response changes, the predicted amounts of response change across trials will depend on how large such forces are and on how many individuals are subject to these influences; two trials suffice to estimate in either model random influences. If 40 per cent of the sample were really immune to short-term forces, however, the third trial predictions would be vastly incorrect for the probability model, for the immune respondents would not change responses as predicted. As Table 7 indicates, the immunity hypothesis is incorrect. The hypothesis remains incorrect when the lumps made by large frequency categories are removed, as shown in our text. Inert marginals have little to do with the question, which rests on extrapolation of change rates, most basically moving from products of two rates to products of three rates and involves predictions of cell frequencies, not marginals. The same type of predictions in the black-and-white model leave a third of the variance in cell frequencies unexplained, though only eight cells are being predicted—those stable in 1956–1958 but predicted to be unstable in 1960—and very specific allowances for response set (a special response set for each possible combination of responses) are included. Even under these most generous possible terms, the black-and-white model misestimates, while the probabilility model's assumption that random forces apply to the entire set of respondents is confirmed.

Bias in Short-Term Forces

At base, *random* means equiprobable. As there are a considerable variety of referents for this description, common usage of the term follows the form "random (equiprobable) with regard to . . ." and specifies a particular reference. After stating our assumptions that short-term forces are random with regard to every other referent used, we employ the shorthand term *random*. The same shorthand term does not have the same meaning in the black-and-white model. Short-term forces are not random with regard to respondents, though it can never be clear just which respondents depart from the equiprobability. Short-term forces are not random with regard to trials, as there are measured and employed differences between trials in short-term forces. Short-term forces are not random with regard to responses, as there are again measured and employed differences between response frequencies counted as short-term forces in the black-and-white model. Short-term forces, however biased among respondents, trials and responses, are not random with regard to each other, as there are clear departures from equiprobability when everything else is held constant. It is not apparent to us that there is any respect in which black-and-white model short-term forces are random.

The absence of randomness of short-term forces is, in fact, an integral ingredient of the black-and-white model's results on the power issue; equally important, however, the presence of non-random bias is left unexplained. As indicated in Appendix II, no testable hypothesis explaining the bias on any question, on the set of questions, or at any level of generality has survived. No explanation holds water. Having followed up on the assorted suggestions about bias from Converse and other readers, we have collected a fat folder of disproofs, which we shall spare the reader. Consider instead the summary impact of the treatment of bias on Converse's results. Predicting only half as many cells as we do, the black-and-white model correctly places 92½ per cent of the predicted respondents for the 1958–1960 public power responses, a level of accuracy Converse regards as indifferent or mediocre when it describes our own results. His accuracy derives from a highly elastic treatment of response set. If, following our own practice, Converse does not include unpredicted bias in his estimates and treats it as error because it is unexplained, then his accuracy drops to 69 per cent of the respondents correctly placed. If he sticks with a consistent bias for random forces (a biassed coin approach to response set), the accuracy is not much better—75 per cent. Response set should either be specified as a variable or excluded as an explanation of responses. If, as his comments indicate, Converse believes response set to be so complex and unpredictable that no estimates and tests can be performed, he should not use methods which assume that response set is clearly identifiable and has simple effects.

Testing the Probability Model

The level of prediction in both the probability and black-and-white models is aggregate, as individuals' responses are not predicted. The lowest level of prediction in either model is the proportion of the respondents providing each possible combination of responses. The test of predictions we use treats each such combination as a separate prediction and measures prediction error for each of 25 or 125 cells and thus measures error at the lowest possible level of aggregation. The "respondents correctly placed" test will not normally give scores around 0, but high scores are more difficult to arrive at than Converse implies. For example, much of the work in voting behavior assumes that party identification attitudes are stable, in 1958 and 1960 as an instance, and cause the survey responses to the party identification item; yet, at best, less than 62 per cent of the respondents giving substantive responses are correctly placed under the assumptions. If the reader has doubts about the measure, we suggest that he or she try it out on some data thought to be completely understood and adequately explained. The ordinal correlation measure used to test the black-and-white model predictions is an easier test because the level of aggregation is higher and because only one aspect—ordering—of the prediction is being tested.

The predictive accuracy of the probability model depends only partly on the items debated between Converse and ourselves. Additionally, some noncontroversial assumptions must be correct if the predictions are to be accurate. No one has suggested that the forces causing responses in 1956 differ from those in 1958 or in 1960, and therefore no one hypothesizes that the pattern of 1956–1958 responses differs from the pattern of 1958–1960 responses. Indeed, we start from the assumption of identical patterns resulting from identical causes, use the probability model to specify the causes, and correctly place 93 and 90 per cent of the respondents. Actually, the assumption of identical patterns is only partly correct, as Converse has shown in reporting a 91½ per cent "respondents correctly placed" figure for the assumption of identical patterns. The misplacements reflect the sampling error affecting both patterns. Without specifying sampling error location for each trial, a rather difficult task, neither the probability model nor any other model can correctly place substantially more than the 91½ per cent of the respondents, so our model would appear to be almost maximally efficient.

Residuals showing prediction error are particularly useful for the probability model. In the nature of the model, these residuals are not random, so analysis of the residuals is well-adapted to tests of the model's operating assumptions. The second half of these assumptions—all individuals have the same attitude range, no attitudes change, and no choice bias occurs—are stated as modifiable, operating assumptions. They are not universally applicable and are always worth checking by analyzing prediction error. The minor violations of assumptions reported in the article led us to a variety of subsequent, unreported modifications of the model. Almost none of these changes helped account for responses on the domestic policy items. The major exception occurs with school integration responses: adjustment for attitude change gives a better account of responses, a more stable estimate of random influences, and a more complete account of the unchanged attitudes. Because the probability model generates predictions for every cell, it results in a great amount of information which can be used to understand the data. When applied to party identification, for instance, the predictions are generally less accurate than for the domestic policy questions and the residuals indicate that almost every assumption of the model is incorrect for party identification. In addition to the residuals, several other tests reported in our article would discourage the use of the probability model on the party identification item. Finding out when the model is appropriate and when it is not, and testing for likely modifications is relatively easy and straightforward.

Black-and-White Model Causal Inference

The nonattitude hypothesis in Converse's hands consists of three major propositions: some respondents have attitudes, other respondents lack attitudes and provide unrelated responses, and no respondents change attitudes. Only the third proposition is tested in the black-and-white model. Without attitude change, the sequence in which responses are given is not important, as Converse's predicted correlations show. The probability model also predicts the cross-time correlations with considerable accuracy for it also assumes that sequence is irrelevant. The other two propositions, about attitudes and nonattitudes, are not tested in the black-and-white model. The second proposition, that some respondents provide unrelated responses, can be tested by isolating those respondents changing sides (or simply changing responses). If the responses are unrelated, response-patterns over three trials should fit random expectations. As we have indicated before, such a test would lead to the rejection of the nonattitude proposition.

Although Converse does not directly test for the presence of attitudes, the black-and-white procedures can be seen as a null model test for just that. In standard statistical practice, the black-and-white model would be used only to try to disconfirm the existence of attitudes. Obviously, the model shows that attitudes cannot be dismissed. Following the standard procedure, the next step would be to estimate the impact of attitudes on responses by using a model in which attitudes are the prime explanatory variable rather than a residual category as in the black-and-white model. This switch in procedures as we move from hypothesis testing to causal estimation stages is essential to correct inference. In testing the most difficult barriers are posed for a potential explanatory variable—other variables and random expectations are given complete freedom, while the tested variable must show it can survive on the leavings—which is what the black-and-white model does. Attitudes on the public power item easily pass this test and in standard practice should no longer be discriminated against but should be given equal status with other variables in the estimating stage; correspondingly, the explanatory power of variables almost always increases from the testing to the estimation stage. With only a single variable, as in the black-and-white model, the contrast can be extreme, as random expectations are allowed highly improbable power during testing but are explicitly minimized during estimation. For Converse's treatment, however, no increase in explanatory power occurs for attitudes because the testing procedures, which maximize the role of random expectations and minimize the role of attitudes, are used for causal estimation of the impact of attitudes. The importance of attitudes in determining responses is correspondingly denigrated.

The use of random patterns to represent causal variables is not new, of course, and the probability model also uses them. If such forces are large, no causal interpretation can be taken as established. For this reason, we have followed up on all suggestions of specifiable causes for our random forces; though none of the alternatives have turned out acceptably so far, the random forces are too important to be left unexplained This temporary use of random patterns to highlight some specifiable causes, attitudes in this case, en route to a more complete explanation is standard and correct practice. This is quite different from Converse's position that random forces, however important, should not be explained and that random patterns are preferable to specifying causes.

Application of the Black-and-White Model

We believe it shown on logical and empirical grounds that the black-and-white model should never be applied. On this we are in almost complete agreement with Professor Converse who wishes to exclude application to all but one item. The exception is important, for though Converse believes the prevalence of nonattitudes to be extensive across both the population and across political questions, confirmation of his belief at best rests on a single item. If, as we do, he extends the model to other items, the model is clearly biased toward vastly underestimating the number of attitudes in the population; if, as we do, he concludes the model is never applicable, the nonattitude hypothesis loses even its appearance of empirical support.

Converse's justification for not applying the black-and-white model to other items is the presence of at least slight attitude change on the items. The test for change is the test for the whole model, so whenever the test fails in application, the culprit is always change not the model itself. Without change, the model "fits." Party identification does not change demonstrably during the panel trials, and one published article takes the good fit of a black-and-white model to the party identification responses as evidence of widespread nonattitudes on this item equivalent to the nonattitudes Converse found on the public power item.[1] As we indicate, similar estimates of the prevalence of nonattitudes can be generated for the other domestic policy items because the model itself grinds out misestimates regularly. (Indeed, staple responses are necessary to prove nonattitudes exist.) These estimates have little to do with attitude change. Change is a minor factor except in considering the 1956 responses on two of the items. As the third trial is needed in the black-and-white model only to test for change, nonattitude estimates can be derived quite readily from the 1958–1960 responses for all five domestic policy items, though the estimates remain nonsensical. Even this restriction is not particularly necessary, as the effect of change is merely to increase the number of estimated nonattitudes by about one-third of the amount of attitude change, a minor error compared to those built into the black-and-white model.

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[1] Edward C. Dreyer, "Change and Stability in Party Identification," *Journal of Politics*, 35 (August, 1973), 712–722.

Converse's remaining scholarly statements fall into two categories. The first sort have obvious replies, and all the information necessary to dispose of them has already been presented. For example, Converse repeatedly equates the two model's requirements, while it is clear from displays like Table 4 that the probability model's assumption of no attitude change in 1958–1960, say, is much easier to meet than the black-and-white model's requirement of no change over three trials. Additionally, a number of Converse's arguments are based on simple misunderstandings and misrepresentations of our text. For instance, the reader will be relieved to find that we neither say nor attribute to Converse the notion "that only 20 to 30 per cent of the American populace has real attitudes on any political issue. . . ." We hope such difficulties will not distract from the basic issues involved.

Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group*

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Introduction

Institutional arrangements devised for collective decision making often result in biases for certain interests at the cost of others. In legislative settings, the representation of constituent interests is fundamentally affected by the processes that intervene between inputs and policy decisions. For example, reliance on committees for agenda setting and the structuring of policy alternatives results in different legislators having unequal access to important areas of decision making. Reaction to a perceived conservative institutional bias in the U.S. House of Representatives brought a group of liberal Democrats to organize the Democratic Study Group (DSG) over a decade ago in order to counteract those factors that minimized the effect of their numbers. Factors contributing to this conservative bias and an assessment of DSG success in mobilizing its forces to counteract this bias are the subjects of this paper.

It is not difficult to picture the roadblocks in the legislative process faced in the late 1950s by the founders of the Democratic Study Group. A recitation of legislative victories of the conservative coalition during the previous two decades would go a long way toward explaining the sense of frustration felt by many liberal Democrats. The sheer number and ideological consistency of Republicans and southern Democrats posed formidable problems for liberal efforts to legislate domestic policy. Yet what was especially grating was the failure of liberals to marshal the potential forces that they did have. Many decisions simply did not reflect the numbers they perceived to be sympathetic.

Certain institutional characteristics of the legislative process in the House provide a fruitful explanation of the liberals' dilemma. By institutional factors we mean that set of formal and informal procedures which govern everyday life within the legislative chamber. One aspect of the legislative process which emerges as relevant to the perception of a conservative bias in the House is the process of individual decision making on roll-call votes.

There is no argument among observers of Congress that specialization is the defining characteristic of the manner in which the House conducts its business. Nor is there any disagreement over the attendant ability of individual members to exercise considerable influence within narrow policy domains. What is somewhat less settled is an understanding of how committee decisions come to be ratified by the whole House. That question is itself enormously complex and not answerable by a simple generalization appropriate for the whole range of committees in the House. What can be said, however, is that in the late 1950s the conjunction of committee leadership seniority and integration on the one hand, and the process of individual decision making on the other, generally led to collective decisions somewhat more conservative than might otherwise have been expected.

Why is this the case? Several recent research efforts focusing on the information problems of representatives provide an interesting perspective from which to address this question.[1] An individual representative armed with a set of policy predispositions and informed by constituent interests often is poorly prepared to cast rational votes on choices to accept, modify or reject committee decisions. The sheer number of such decisions confronting each Con-

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* We wish to thank the Inter-University Consortium for Political Research for providing the roll-call data used in the analysis.

[1] Arthur G. Stevens, Jr., "Informal Groups and Decision-Making in the U.S. House of Representatives," (Ph.D. dissertation, University of Michigan, 1970); Donald R. Matthews and James A. Stimson, "The Decision-Making Approach to the Study of Legislative Behavior," (paper delivered at the 65th Annual Meeting of the American Political Science Association, 1969); John S. Saloma, III, *Congress and the New Politics* (Boston: Little, Brown and Co., 1969), pp. 214–218.

gress, together with their specificity and nonideological cast, render predisposition an inadequate guide to decision making. Information on the substantive content of the bill, its political implications, and its relationship to the representative's relevant behavior at an earlier period of time must enter into the calculus for a rational decision to be made.

The insight gained from recent research is that certain institutional forms and practices have developed in the House that provide the congressman with "short-cuts" toward gaining the information he needs. In large part, congressmen tend to rely for information on members of those committees that have jurisdiction over the legislation under consideration. As relationships with trusted sources develop, the norm is often one of cue taking, in which only the "correct" vote direction need be communicated. As one member described the process,

> on those bills on which I have doubts, I depend on friends whose views I can trust. Sometimes when I don't know the content of a bill or an amendment, I look to see how others vote. There is an old joke about one congressman who voted "absent" because he heard another vote "present."[2]

If congressmen choose as information sources those committee members with constituencies or attitudes similar to their own, where is the slippage that might result in a bias? Part of the answer is that the member chosen as a source may not share the predispositions of the congressman seeking advice or information. As Matthews and Stimson point out,

> cue-givers in most policy areas are a biased sample of the House membership; in almost all policy areas they tend to be more senior than the average member, they tend also to be from congressional districts particularly affected by the policies in which the cue-givers specialize. Service on at least the better integrated House committees tends to socialize the specialist into an overall committee point of view which may diverge from the views of the House as a whole. . . . The policies that result from cue-taking are thus more conservative and more sensitive to the wants and needs of the immediate parties-at-interest than would be the case with fully independent decision-making.[3]

That is, the number and strategic location of conservative-minded members make a perfect ideological matching of cue-takers and cue-givers unlikely. Of course, the likelihood of mismatch varies across committees, depending upon the extent to which the committee membership is representative of the views of the House.

Certain procedural practices in the House further complicate this information-seeking process. The dominant committee view tends to have a virtual monopoly of the most visible communication channels—committee reports and floor debate. Committee reports are frequently filed just prior to floor consideration of the legislation making it very difficult for members to obtain relevant information, even if they had the time to do so.[4] Similarly, floor debate, managed typically by the committee or subcommittee chairman and subject to the five-minute rule on amendments, for the most part conveys information loaded in favor of the committee majority. The unanimity of cues emerging from floor debate is particularly striking when the leaders of the partisan majority and minority are in basic agreement.

Certainly these findings regarding the information problems of individual congressmen are not surprising, given our knowledge of information control in social organizations. Hierarchy and specialization generally lead to information blockage, distortion, and misrepresentation.[5]

Two other institutional factors lend credence to the liberal perception during the late 1950s of a conservative increment of strength resulting from internal processes. One was the procedure for nonrecord votes on amendments in the Committee of the Whole prior to the 92nd Congress. While overall attendance was low on votes in the Committee of the Whole (less than a third of all members generally participated in nonrecord votes on amendments compared to 85–90 per cent attendance on roll calls and perfunctory quorum calls),[6] nonsouthern Democrats were notorious for their absence. They tended to represent competitive districts; hence instead of spending their time on the floor, they often were engaged in performance of constituent services or in public relations activities. Thus conservative forces had an advantage simply as a result of the number of people on the floor.

The other disadvantage confronting liberal

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[2] Stevens, pp. 58–59.

[3] Matthews and Stimson, pp. 18–19.

[4] The Legislative Reorganization Act of 1970 contained a provision that requires all committee reports to be printed and available three days (excluding Saturdays, Sundays, and holidays) prior to floor consideration. Furthermore, it also stipulates that Appropriations Committee hearing transcripts be similarly available.

[5] See Saloma, *Congress and the New Politics*, pp. 210–211 and the sources cited therein.

[6] Democratic Study Group, Special Report, July 7, 1970.

Democrats in the late 1950s stemmed from their bargaining position within the party. The seniority system had placed a number of relatively conservative southern Democrats in positions of power. Committee chairmen were free to use the powers of their official positions to implement their personal policy views regardless of whether those views were shared by a majority of their Democratic colleagues. The elected party leadership was accustomed to dealing with the southern wing of the party. And the party leadership was a deterrent to the effectiveness of liberals, especially liberals representing marginal districts, since the leadership tended to overrepresent areas of greatest Democratic strength.[7] These factors combined to make it difficult for liberals to enact their policy views.

The Democratic Study Group

Although small groups of liberals had been meeting on an ad hoc basis with increased frequency since 1953, the formal organization of the Democratic Study Group (DSG) in September 1959 marked the first sustained effort to counter the conservative coalition.[8] The organizational structure and operating strategies developed in the first few months, aimed as they were at the problems of information, attendance and strategic bargaining, remained remarkably constant over the subsequent decade. Concerned with increasing the likelihood of liberal policy outputs, the DSG engaged in activities at every stage of the legislative process. But its major focus continued to be providing information necessary to bring some coherence to the liberal Democratic bloc.

Organization. The Democratic Study Group is a voluntary group of House Democrats with a membership roster, elected leaders, permanent staff, and a set of regularized procedures for meeting and arriving at various decisions. Membership in the DSG has ranged between 115 and 170 over the last six Congresses. It is hard to determine membership precisely because the dues-paying membership list does not necessarily mirror the whip list.[9] Occasionally an active DSG member is remiss in paying his dues, though he continues to be carried on the whip list. Similarly, it is possible for a representative who has no ideological commitment to the implicit goals of the DSG to appear as a member merely because he pays dues so as to obtain DSG information sheets.

The DSG is headed by an elected chairman and executive committee who meet regularly to exchange information and plan strategy on important legislative matters. As in other leadership groups, the size of the executive committee (eight in 1959 and fifteen in 1970) often precludes full attendance and leads to the formation of small, informal groups who work flexibly under the supervision of the chairman. Policy decisions are the responsibility of full membership meetings. On substantive issues, either the DSG position is "obvious" (e.g., civil rights), necessitating no formal decision before action is begun, or the issue is sensitive enough to divide DSG opinion (e.g., funding of the supersonic transport aircraft). Hence, generally no formal DSG positions on legislation are taken.

In practice, virtually all of DSG's activities are generated and executed by the staff, which has assumed an independent leadership role in recent years. The DSG staff ordinarily consists of approximately twelve people, four of whom engage in legislative research. The institutionalization of the DSG has left the staff director a good deal of discretion in initiating and overseeing DSG operations.

The DSG is further organized into task forces in order to complement the work of the Democratic majority on regular House committees. Their number has varied greatly during the years the DSG has been in existence, ranging from none in the 87th Congress to eleven in the 91st Congress. Task forces were intended to provide a focus of leadership and coordination for the major legislative thrusts articulated in DSG's formative statement of purpose. Since it early proved difficult to get substantial attendance at regular DSG meetings on issues, the task force provided a manageable group that widened participation and harnessed a chairman who typically was already an activist in the area and a member of the relevant committee. The Civil Rights and Area Redevelopment task forces were especially successful in organizing efforts that forced legislation out of committees and prevented crippling amendments on the floor. Nonetheless, activity and effectiveness have varied greatly across task forces.

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[7] Barbara Hinckley, *Stability and Change in Congress*, (New York: Harper and Row, 1971), p. 80.

[8] The two best sources on the formation and early development of the DSG are Kenneth Kofmehl, "The Institutionalization of a Voting Bloc," *Western Political Quarterly*, 17 (June, 1964), 256–272; and Mark F. Ferber, "The Democratic Study Group: A Study of Intra-Party Organization in the House" (Ph.D. dissertation, University of California, Los Angeles, 1964). A more recent discussion and analysis of the effectiveness of the DSG is an excellent unpublished paper by Anne Henderson: "The Democratic Study Group: An Appraisal" (Rutgers University, 1969).

[9] In subsequent analysis we utilize the whip lists to identify DSG members. Special thanks are due Richard Conlon, Staff Director of the DSG, for providing these lists as well as numerous other bits of information and insight.

While a number of task forces have been viewed as successes by their members and by DSG leaders, others have not been so viewed. Several factors have worked against success: (1) staff resources apart from the task force chairman's personal staff are limited; (2) primarily because of lack of time, participation by task force members other than the chairman tends to be low; (3) divisions develop within task forces when the substance is outside traditional social and economic concerns; and (4) some task forces exist in areas in which legislative interest is dormant.

Activities. Within this organizational structure, the DSG engages in a series of activities designed to maximize liberal outputs throughout the legislative process. In doing so it addresses the three major difficulties identified above—information, attendance, and strategic bargaining.

A substantial portion of DSG resources are invested in meeting the information problem. Several of the staff concentrate on obtaining as much information as possible about legislation about to be reported from committees. Their sources include friendly congressmen serving on the committees, interest groups, the Administration (especially when Democratic) and DSG task force reports. They provide this information, primarily through fact sheets and weekly legislative reports, to DSG members. The intention is to help the members avoid a mismatch between predisposition and vote by providing essential information.

A typical fact sheet is approximately five to ten pages long. It contains the legislative history of a bill, the background of the substantive problem, an outline of the major provisions of the bill, the views of the Administration, probable amendments, and arguments for and against. The dual arguments, an innovation of the last several years, reflect the desire of congressmen for complete and objective information. While in earlier years DSG leaders did not feel obliged to explicate opposing views, they always subscribed to the principle of providing members with factual information.

The DSG also distributes a weekly legislative report (originally conceived as a supplement to the weekly Democratic whip notice), briefly stating the basic provisions of bills, the position of the Administration, interest groups and affected agencies, committee action and views, anticipated amendments, and proposed terms of floor consideration. These reports, begun in 1967, obviously reflect the problem of insufficient information prevalent in the House and, more particularly, the failure of the regular party leadership to keep its followers adequately informed.

In addition to providing members with information about legislation, DSG in recent years has furnished services which involve information of different kinds. One such service is a weekly bulletin intended to aid staff of DSG members by making available such items as sample letters to constituents on current topics and bibliographies of studies done on such topics.

The DSG also provides a variety of campaign services to members and Democratic candidates for Republican seats. In addition to making financial contributions, DSG attempts to aid nonincumbent Democrats by sending them the same research reports available to members plus reports on the legislative record of their incumbent opponents and by holding seminars to give advice on campaign techniques. At the beginning of each Congress, DSG holds meetings for freshman Democrats at which they are given advice on such matters as staffing, obtaining a desirable committee assignment, and using Library of Congress resources.

All of the above services, most of which involve providing some kind of information, are designed to increase the personal effectiveness of nonincumbent Democratic candidates and actual DSG members. Interviews indicate that such services are appreciated and in many cases provide the incentive for joining the DSG.

The DSG also has invested much effort in trying to increase the vote turnout of its members, primarily in the Committee of the Whole. One of the first actions taken by the informal group of liberals that pre-dated the organization of the DSG was the establishment of a rudimentary whip operation. This priority persisted in the early discussions of the Democratic Study Group and led to the establishment of a whip system. The telephone whip system in which brief messages are sent to members through calls by secretaries in each DSG member's office is the simplest operation. More elaborate ad hoc whip systems are formed for specific issues by utilizing as whips those members who are already active in the particular legislative area and by including non-DSG members and excluding unsympathetic DSG members when it is advantageous to do so.

As the system evolved, special whip arrangements were developed for highly important pieces of legislation. Their most elaborate forms were those in which certain members were assigned to be responsible for others, reporting absences or "wrong" votes on tellers to contacts just off the House floor, who in turn notified lobbyists important in their districts. The 1964 Civil Rights Bill is a fine example of how the DSG whip system, in cooperation with important interest group representatives, can get members to the floor and keep them there.

Yet with the exception of a handful of important pieces of legislation, the task of increasing liberal participation on nonrecord votes in the Committee of the Whole was a frustrating one. Attendance remained relatively low, and many important votes on amendments were lost by close margins. Whatever the effect of the whip system on nonrecord votes, it was marginal compared to the increased participation arising from the rules change that allowed for record votes in the Committee of the Whole beginning in the 92nd Congress. (In the 92nd Congress, attendance on recorded teller votes averaged 375 compared to the 150 mean attendance on teller votes in previous years.) The DSG's awareness of the limitations of its whip system was evidenced by its leadership role in the adoption of the recorded teller.[10]

Other DSG activities fall into four classes: agenda setting, parliamentary strategy, coordination of outside groups, and institutional change.

In the early days of the DSG, the difficulty that loomed above all others was getting legislation past the Rules Committee to the House floor. Judge Smith's chamber had proved to be the burial ground of numerous pieces of domestic legislation favored by the northern liberals. In their first tactical encounters with the agenda-setting problem, the DSG was successful.[11] DSG task forces effectively organized the discharge petition on the civil rights bill and the Calendar Wednesday procedure for the area redevelopment legislation, forcing both around the Rules Committee to the House floor. Subsequently, the DSG played a major role in enlarging the Rules Committee, primarily by keeping sufficient pressure on the Speaker.

Closely related are DSG's efforts at developing optimal parliamentary strategies for legislation reported for floor consideration. The reputation of liberals for being outmaneuvered by southerners on fine points of the rules of the House was accurate. The organization of the DSG provided a focal point for harnessing what few liberal parliamentary experts existed and developing, in advance, coherent strategies for the floor.

At the same time DSG has served in an important capacity as in-house coordinator of outside lobbying groups. The trade-off in resources has been impressive: DSG has an effective information gathering and disseminating system but little political clout; lobbies sometimes can provide the latter but are often in need of a targeted audience and general overall strategy. The marriage has worked particularly well with civil rights legislation and efforts to secure full funding of education programs.

Finally, in an effort to improve its bargaining position within the House Democratic Party, the DSG has sought institutional change. It has worked within the Democratic Caucus and, where necessary, the House, to modify those rules and practices largely responsible for the unresponsiveness of party leaders to the views held by a majority of their Democratic colleagues.[12] These activities are quite important to the future role of DSG within the party, and merit independent consideration.

Our theoretical discussion and description of the operations of the Democratic Study Group suggest that the formation of the DSG has led to increased turnout, especially in the Committee of the Whole, and to increased cohesion among DSG members. A concern about these dimensions of turnout and group cohesion has guided our selection of data, methodology, and analysis efforts.

Methods

Data. Several sources of data were used in this study. DSG records containing information about the organization, membership, and activities of the group were crucial. Other data sources were interviews with congressmen and records of roll-call votes. The interviews were collected at three different points in time. The first series of congressional interviews was obtained by Warren Miller and Donald Stokes in 1958 and provided initial insight into the theoretical focus of this study.[13] The second and third sets of interviews were conducted in 1969 and 1971.[14] The roll-call analysis depends upon a longitudinal set of 800 roll calls composed of a sample of 100 nonunanimous votes from each of the eight Congresses between 1955 and 1970.[15] Earlier analysis utilizing the outputs of several congressional committees across five Congresses and reported elsewhere provided incentive for this paper as well as support for our findings.[16]

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[10] John F. Bibby and Roger A. Davidson, *On Capitol Hill: Studies in the Legislative Process,* (Hinsdale, Ill.: Dryden Press, 2nd ed., 1972), chap. 8.

[11] Ferber, chap. 4.

[12] Roger H. Davidson, David M. Kovenock and Michael K. O'Leary, *Congress in Crisis: Politics and Congressional Reform* (Belmont, Calif.: Wadsworth Publishing Co., 1966); and Bibby and Davidson, chaps. 4 and 8.

[13] The interviews were obtained by Warren E. Miller and Donald E. Stokes as part of their study of the

Analysis techniques. Numerous methods have been developed for the study of intragroup cohesion and intergroup differentiation. The analysis methods found to be most appropriate to the concerns of this paper are based on the work of Duncan MacRae, Jr., and of Herbert Weisberg.[17] Both have been interested in the application of such dimensional techniques as factor analysis in the investigation of legislative roll-call behavior. In this framework, an inter-item correlation matrix[18] for the 100 nonunanimous roll calls from each of the Congresses between 1955 and 1970 was computed and factor analyzed.[19] The results of the factor analysis, after an orthogonal rotation, are presented in Table 1. The importance of particular factors can easily be gauged, since the total contributions for a perfect solution are equal to the number of items analyzed—in this case, 100. The amount added to the factor contributions by the fourth and succeeding factors was generally not greater than 2 per cent.[20] Thus only the first three factors were used in subsequent analysis and in the computation of factor scores, with the exception of the 90th Congress, for which only the first two factors were used.

Factor scores for all of the House members were then computed from the results of Table 1, with each congressman being assigned a factor score for each of the most important factors.[21] These scores were used to measure the degree to which congressmen voted together on roll calls defining an issue dimension. Basically, the more similarly two congressmen voted on a series of roll calls from the same issue domain, the smaller the difference in their respective scores for that factor. Thus we can use the variance of factor scores to talk about cohesion among a number of congressmen who form a voting coalition. The more similar the legislative voting behavior of the several members of the group, the smaller the variance of the factor scores assigned to those members. The variance in scores for a given factor provides a measure of cohesion on the roll calls which load strongly on that factor.

Factor scores also can be used to compare voting behavior of members across a number of issue domains. This comparison is most easily accomplished by analytically treating the members as points in an *n*-dimensional space,

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linkages between public opinion and congressional roll-call behavior. See Miller and Stokes, "Constituency Influence in Congress," *American Political Science Review,* 57 (March, 1963), 45–56.

[14] The 1969 interviews were conducted by Arthur G. Stevens, Jr., as part of his research for the dissertation cited above. The 1971 interviews with numerous congressional staff members, DSG members and leadership, as well as some Republicans, were gathered purposely for this paper.

[15] Nonunanimous roll calls were defined as those roll calls on which at least 10 per cent of the House members voted in opposition to the majority. The sample was drawn so that the number of roll calls selected is proportional to the output of the congressional committees.

[16] See Stevens, and see also Arthur H. Miller, "The Impact of Committees on the Structure of Issues and Voting Coalitions: The U.S. House of Representatives, 1955–1962" (Ph.D. dissertation, University of Michigan, 1971).

[17] Duncan MacRae, Jr., *Issues and Parties in Legislative Voting* (New York: Harper and Row, 1970); and Herbert F. Weisberg, "Dimensional Analysis of Legislative Roll Calls" (Ph.D. dissertation, University of Michigan, 1968).

[18] The correlation coefficient used is the tetrachoric *r*, a measure of association that indicates the degree to which variables share a common dimension. For an excellent discussion of tetrachoric *r* and its applicability to dimensional analysis, see Weisberg, chap. IV, and MacRae, chap. 3.

[19] The factor analysis was done with unities as the estimate of the commonalities, and an orthogonal (varimax) rotation was used to obtain the matrix of factor loadings. The solution from the orthogonal rotation was selected as optimal, since the factors were found to be uncorrelated in the oblique solution. Furthermore, the varimax solution gave the clearest pattern of loadings, i.e., an item has one large loading with much smaller remaining loadings.

[20] Variance terminology is not strictly appropriate for the factor analysis of dimensioning coefficients since such coefficients are not based on the statistical concept of covariance. The factor contributions, however, which are equal to the sum of the squared loadings for each factor, can be used as a rough measure of the relative importance of the factors.

[21] The matrix of factor loadings resulting from the factor analysis of the tetrachoric *r* matrix is used to compute the factor scores. The actual computation of the scores is based on the estimation procedure suggested by Harry H. Harmon, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1967), pp. 348–350. When factor analyzing tetrachoric *r*, a factor loading of unity means that the roll call falls exactly on the dimension indicated by the factor. A loading of minus one means that the item needs to be reflected (i.e., the scoring reversed) to be positively correlated with the other items on the dimension. The size of the loadings does not indicate order of the items on the dimension, only that they share a common dimension. The overwhelming majority of loadings obtained in the analysis were .8 or larger in magnitude.

Table 1. Factor Contributions from the Varimax Solution

Congress	Factor 1[a]	Factor 2[b]	Factor 3[c]	Total
84th	41.16	19.26	7.14	67.56
85th	34.84	28.15	13.91	76.90
86th	44.02	28.66	13.35	86.03
87th	41.49	19.87	22.45	83.81
88th	46.34	33.05	2.64	82.03
89th	44.29	40.21	5.43	88.93
90th	47.75	29.51	—	77.26
91st	32.52	28.45	14.89	75.86

[a] These votes deal with the scope of activities performed by the federal government. Topics include regulation of waterways and power generation stations, the federal role in construction and control of public facilities, regulation of increased industrialization, farm price supports, school milk programs, and the food stamp plan.

[b] These votes seem to represent attempts to extend individual freedom and human rights. The issues include civil rights, immigration and nationality laws, criminal procedure, voting rights, subversive activities laws, minimum wages, educational loans for students, and various poverty programs.

[c] These votes deal with foreign affairs and, during the 91st Congress, limits and priorities on presidential spending.

where *n* equals the number of factors and each factor acts as an axis in a Cartesian coordinate system. Interpoint distances between the members, or between the members and the central value for the group, can then be computed from the Euclidean distance formula by using the factor scores as axis projections.[22] The mean and variance of the distances derived from these computations can be used to measure cohesion among members on a series of roll calls that come from more than one issue dimension.

Predispositions. Our central hypothesis is that the DSG communications network has had an impact on the roll-call voting of DSG members. But since the members would be expected to vote alike even in the absence of an organization,[23] how can we ascertain the impact of the group? First, longitudinal roll-call data are used, extending from the 91st Congress back through two Congresses before the DSG was formed. This allows us to look at changes in DSG membership behavior across time. In so doing, treating the members *as a group* is in itself a control for predisposition; changes across time in behavior can be observed as changes on the part of a group of similarly predisposed persons. In addition, the extension backward of the file to include the 84th and 85th Congresses allows us to examine the behavior patterns of initial DSG members before and after the group was organized and to note any change.

Second, throughout the analysis the behavior of DSG members is compared with that of members of other "groups"—Republicans, southern Democrats, and nonsouthern, non-DSG Democrats. Study of the behavior of DSG members over time relative to that of nonsouthern, non-DSG Democrats enables us to compare two sets of congressmen who can be expected to share somewhat liberal predispositions. Evidence that DSG members have behaved increasingly differently from nonsouthern, non-DSG Democrats should be strongly suggestive, therefore, of "group" impact on the voting behavior of DSG members.

The behavior of Republicans and southern Democrats—the traditional adversaries of DSG—must also be traced over time. For instance, our expectation that the setting up of continuous lines of communication among liberals would lead to an improved vote performance cannot be confirmed merely by demonstrating that liberals have become more cohesive since formation of the DSG. Nor could this expectation be denied by the discovery that liberal cohesiveness has decreased. If the trend in cohesiveness among DSG members, regardless of its direction, is more favorable than that of the southern Democrats and of the Republicans—the groups the DSG was organized to counter—some support will be lent to our expectation.[24]

Findings

If the DSG communications network has

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[22] The computational formula for the squared distance between any two points in an *n* dimensional space is:

$$d_{xy2} = \sum_{i=1}^{n} (x_i - y_i)^2,$$

where *n* is the number of dimensions (of factors), and x and y are the two different points. When mean factor scores are used to compute the distance between groups, the mean values can be readily substituted in the equation for x and y.

[23] One alternative hypothesis would attribute cohesion to constituency similarity. Unfortunately, attempting to control explicitly for constituency factors would increase the complexity of the analysis far beyond the possible scope of this report.

[24] For a more detailed explication of the measurement problems surrounding predispositions and some approaches to a solution, see Stevens, "Informal Groups and Decision-Making in the U.S. House of Representatives," pp. 42–46.

Table 2. Turnout of Key Votes 88th–90th Congress by DSG Effort[25]

Congress and Session	DSG (%)	Nonsouthern Non-DSG Democrats (%)	Southern Democrats (%)	Republicans (%)
		Intense Effort		
88–1	97.9	91.2	92.1	94.5
88–2	97.0	93.1	94.9	96.3
89–1	97.0	95.3	94.2	94.5
89–2	87.6	85.0	76.4	86.4
90–1	94.6	92.9	92.3	93.1
90–2	95.4	92.0	90.5	94.2
Mean	94.9	91.6	90.1	93.2
		No Effort		
88–1	93.9	92.1	92.4	95.2
88–2	94.1	92.7	89.5	96.6
89–1	91.4	95.3	87.9	89.5
89–2	83.6	82.9	78.1	82.0
90–1	90.4	89.6	91.2	91.7
90–2	92.6	92.0	86.3	96.0
Mean	91.0	90.8	87.6	91.8

had an impact on the roll-call voting behavior of DSG members, we should find traces of the following voting patterns:

(1) There will have been some increase in DSG membership turnout on roll-call votes in those cases when there was a group effort to "get out the vote." This increase will have been fairly small, however, because congressmen almost always try to vote on important legislation when the votes are recorded.

(2) The voting behavior of DSG members will have become increasingly differentiated from that of nonsouthern, non-DSG Democrats.

(3) DSG vote cohesion will have increased over time or at least will have exhibited a more positive trend than has the cohesion of Republicans or southern Democrats or both, and this trend will not have been due to recruitment of new members or to loss of old ones.

Each of these hypotheses will be investigated in turn.

Turnout. A central concern of DSG leaders and staff has been to find means of increasing the voting participation of its members. As discussed above, an elaborate whip system was developed to notify members of important votes on the floor. In most cases, the effort has been directed toward the Committee of the Whole, where the use of nonrecord votes traditionally has resulted in low attendance, particularly among liberals. Occasionally the DSG whip system has been employed on roll-call votes, and it should be possible to determine the effectiveness of such efforts. (It should be noted, however, that in roll-call situations the whip system was used more to gain information about the voting intentions of congressmen than to get them to the floor.)

A measure of the DSG's impact on voting participation should differentiate votes according to whether the DSG put its whip system into operation. While this information is not available across the set of roll calls in this study, the figures in Table 2, collected by Anne Henderson, provide some illustrative data. The roll calls included are those identified by *The Congressional Quarterly* as key votes. We have chosen to compare situations in which DSG mounted an intense effort with those in which it mounted none. The observed pattern of participation is consistent with our hypothesis. On intense whip efforts, DSG members

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[25] These data are taken from Anne Henderson, "The Democratic Study Group: An Appraisal," Appendix F. The author's unique position on the Hill allowed her to discern the level of DSG involvement on a set of roll call votes in the 88th–90th Congresses. She used a quasi-experimental design to study cohesion as well as turnout, and her findings on the former are generally in accord with those reported in this paper.

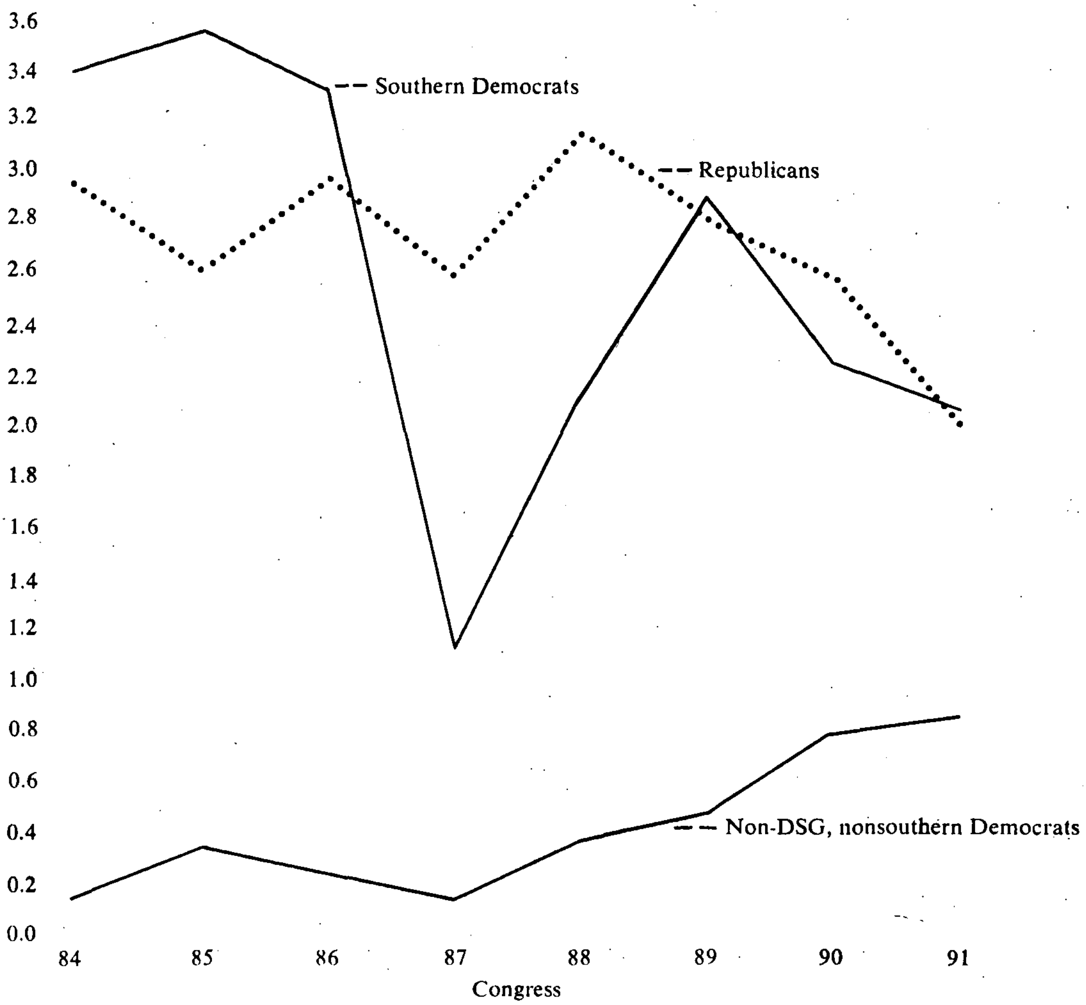

Figure 1—Mean interpoint distance of groups from the DSG

participated at a higher rate than they did on roll calls with no whip efforts and at a higher rate than did members of other groups. These differences are not very impressive upon first view, but it should be noted that when turnout is approximately 91 per cent without a whip effort there is little room for improvement.

DSG and Nonsouthern Non-DSG Democrats. One could legitimately question whether DSG nembers are different in their roll-call behavior rom other Democrats who are neither southrners nor DSG members. Mean scores on ach factor for each Congress were, therefore, omputed for DSG members and for non-DSG nonsouthern Democrats. These factor cores were used to calculate the mean interpoint distance between these groups in the multidimensional space.[26] The resulting distances between the DSG and non-DSG nonsouthern Democrats are displayed in Figure 1.

In the 84th Congress, non-DSG nonsouthern Democrats were very close to those who were to become the future DSG members,[27] par-

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[26] The computation of the mean interpoint distance was accomplished by using the group mean on each factor as the axis location for the group; from these axis values the squared Euclidean distance between each group was obtained. (See note 22 for the formula used to compute the distance.) As the similarity in the voting behavior of the two groups increases, the distance increases. If the members of the two groups vote exactly the same on all the bills, the distance between the groups would be zero. As the two groups grow increasingly opposed to each other, the distance value also increases.

[27] Future DSG members are identified as those Dem-

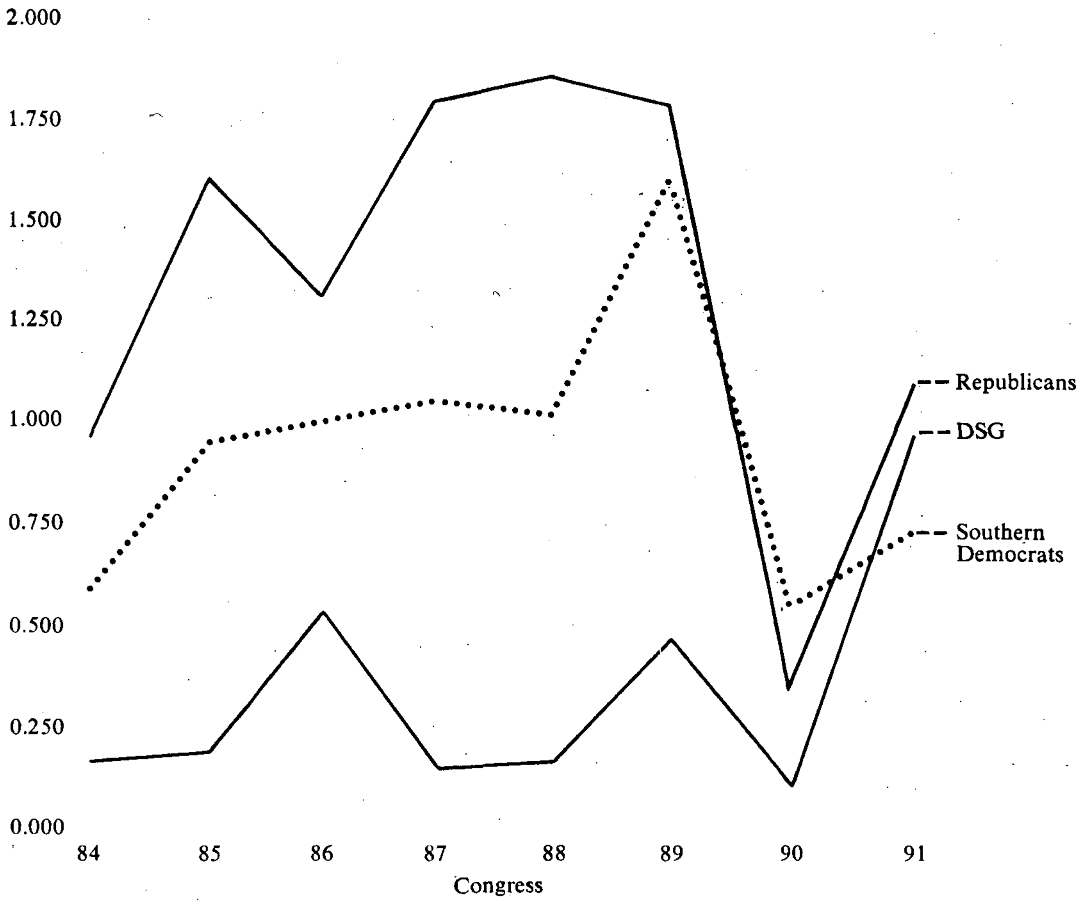

Figure 2—Distance of congressmen from the centroid of their group.

ticularly when compared with southern Democrats. In the 85th Congress the distance between the DSG and the non-DSG nonsouthern Democrats grew by a multiple of 1.9, only to recede slightly in the 86th and practically to vanish in the 87th. The shrinkage of the distance in the 87th is placed in perspective by examining the comparable shrinkage of the DSG-southern Democrat distance. This was the Congress just subsequent to the 1960 election, and the extremely strong tendency for *all* Democrats to close ranks in the first flush of victory (in part because of the willingness of the President to make the necessary compromises in committee) can be noted clearly. From the next Congress on, however, the distinction between the voting behavior of DSG members and other nonsouthern Democrats grew more pronounced. By the 91st Congress, the distance had increased 4.9 times over what it had been in the 84th Congress. This data array supports an argument for treating DSG members and other nonsouthern Democrats separately in roll-call analysis. It also suggests that the DSG may have had a substantial impact on its members, as the distance between them and their nonsouthern party colleagues, with whom they shared many predispositions, had widened so considerably since efforts were first made to form the DSG. Part of this distance may be due to the recruitment by the DSG of new members differing in some pre-dispositional characteristics from the origina members. A comparison of new DSG member with those already in the group will be take up below.

Cohesion. Now we shall observe whether th data support our expectation that DSG vot cohesion has increased over time, or if it ha at least exhibited a more positive trend tha

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ocratic congressmen who signed the Liberal Manifesto. For a description of that document, see Kofmehl, pp. 258–260.

Table 3. Proximity on Roll-Call Votes of DSG Members to the Centroid of DSG[a]

Standard Deviation[b]	Congress 84	85	86	87	88	89	90	91
1.00	57	63	110	94	106	155	124	70
0.75	50	56	97	90	102	148	115	66
0.50	41	47	74	83	93	126	99	52
0.25	24	31	45	63	71	92	65	27

[a] Each number represents the number of DSG members within the indicated distance, as measured in multiples of one standard deviation, from the DSG centroid for the indicated Congress.

[b] To facilitate comprehension, the cutoff points were translated into a statement of agreement levels by indicating the average number of roll calls on which members at the various cutoff levels differed. For example, if the core members of the DSG are defined as those lying within 0.25 standard deviations from the centroid, then we can determine, on the average, the number of roll calls on which the members lying within the various cutoff points voted differently from the core members. Results indicate that members who were beyond the core, but lying within 0.50 standard deviations, differed from the core on 5.5 per cent of the roll calls; members within 0.75 standard deviations differed on 8.2 per cent; and those within 1.00 standard deviations differed on 11.2 per cent of the roll calls. Thus in the 90th Congress, for example, 99 members of the DSG voted the same way on at least 94.5 per cent of the roll calls.

has that of Republicans and/or southern Democrats. The centroid of the DSG membership in terms of DSG voting behavior on the major dimensions in each Congress and the distance of each member from the group center were computed.[28] The same calculations were performed separately for southern Democrats and for Republicans. Members can now be identified in terms of their distance from the centroid of their group in the multidimensional space. The mean distance of the membership from the group centroid is presented in Figure 2 for all three groups across the eight Congresses studied.

The amount of cohesion within the DSG on roll-call votes appears somewhat more stable than that displayed by Republicans and southern Democrats, with the exceptions of the 86th and 91st Congresses. (An explanation of the decreased cohesion shown in the 91st Congress will be offered later.) These aberrations aside, there is no clear trend in DSG cohesion across Congresses. Figure 2 does show, however, that DSG members have been much more cohesive in each Congress than Republicans or southern Democrats (with the exception of southern Democrats in the 91st Congress). Indeed, the cohesion of DSG liberals has increased over time relative to that of the southern Democrats and the Republicans (although the latter comparison is less clear than the former).

Interviews with DSG members indicate that DSG membership can be viewed in terms of a set of concentric circles. At the center is a hard core of members who almost always vote and who are very cohesive and active in

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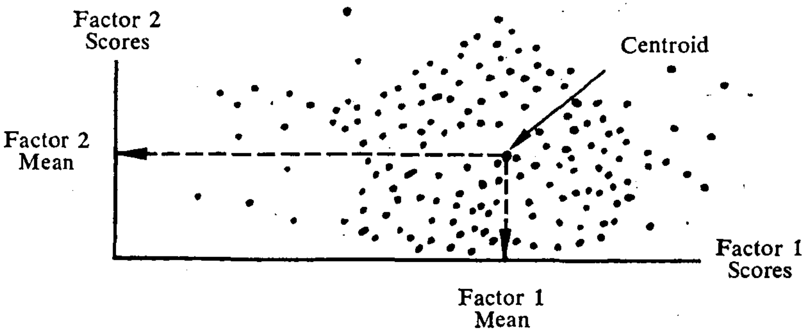
[28] The centroid is simply the center of the group within the hypothesized geometrical space defined by the several factors. It is found by using the mean factor scores for the group as axis projections. For example, if two factors are used, we may represent the centroid as follows:



The factor scores for each individual member (represented as a point in the two-dimensional space) are used as axis projections in computing the squared Euclidean distance of the individual from the group centroid. The standard deviation of the distribution of distances from the centroid is used to define the different levels of group involvement.

Table 4. Proximity on Roll-Call Votes of DSG Members to the Centroid of DSG[a]

Standard Deviation[b]	Congress 84	 85	 86	 87	 88	 89	 90	 91
1.00	.850	.787	.839	.817	.854	.871	.885	.500
0.75	.746	.700	.740	.782	.822	.822	.821	.471
0.50	.611	.587	.564	.721	.750	.700	.707	.371
0.25	.358	.387	.343	.547	.572	.505	.464	.192

[a] Each number represents the proportion of DSG members within the indicated distance, as measured in multiples of one standard deviation, from the DSG centroid for the indicated Congress.

[b] See Table 3, note b.

DSG affairs. Most members appear to be much less involved in the DSG, with some taking very little interest in its affairs beyond receiving DSG information sheets. It is possible to identify the members of these different layers of involvement to the extent that this is reflected in roll-call voting. Table 3 presents the number of members falling within the different levels of cohesion as defined by arbitrarily selected cutoff points measured in multiples of one standard deviation of the distance from the DSG centroid.

Three aspects of Table 3 are particularly striking. First, there is a surprisingly large number of hard-core cohesive members. A bloc of 126 members who fall within 0.50 standard deviations of the DSG centroid when measured across several dimensions on 100 roll calls (as was the case in the 89th Congress) could have considerable influence in a bargaining situation. Second, the number of hard-core voters increased dramatically and fairly steadily from the 84th to the 90th Congress, even in the 87th Congress when the total number of members declined. This suggests that the potential impact of the DSG has soared from the early days. It demonstrates that the core of the DSG has not only multiplied in numbers but also become more cohesive. Third, once again the figures for the 91st Congress constitute a striking reversal of a trend.

As the above figures tend to vary with the size of the DSG, Table 4 presents the proportions of the membership falling within each cutoff range. It now seems apparent that the reversal of the trend toward an increasingly large cohesive core of members appearing in the 90th Congress is due primarily to electoral forces in the 1966 campaign; the DSG lost approximately 40 members in that election. Until the 91st Congress the proportion of the membership which held together solidly increased fairly steadily. Over most of the Congresses, this phenomenon occurred at the same time that membership was increasing. Since a large proportion of new members tend to be freshman congressmen, this fact testifies to the impact of the DSG on the socialization of freshmen.

We can easily identify the members who frequently break ranks with the DSG majority. Throughout the history of the DSG these members have represented primarily rural districts —unlike the majority of their colleagues—and the few southern DSG members have tended to be among their number. This is not to say that no large city Democrats were among the group that deviated most from the core DSG; indeed, several can be so labeled.

In Figure 2 we detected a weak tendency for the DSG to become more cohesive over time relative to southern Democrats and to the Republicans when cohesion was measured in terms of the average distance from the group centroid. In Tables 3 and 4 we noted that the proportion of DSG members relatively close to the group centroid clearly has increased over the years. One might question whether this increase in cohesion could be due to recruitment of new DSG members whose voting behavior placed them closer to the center of the group than those members present in the previous Congress. If this were so, then the favorable trend in cohesion demonstrated by the DSG could not be attributed to the impact of the DSG communication network on members already there. In order to test this alternative hypothesis, the average distance from the DSG centroid for new members and for returning members was calculated for each Congress. These figures are presented in Figure 3.

As Figure 3 demonstrates, in every Congress after the formation of the DSG, the new members were farther away, on the average, from the center of the group than were returning members. This is a definite reversal of the behavior of the individuals whom we labeled new members in the 85th Congress: those signers of the Liberal Manifesto in the 85th Congress who were serving their first terms in Congress. It appears obvious that the maintenance or increase of DSG cohesion across any two Congresses has occurred *in spite of* the recruitment of new members. Examination of the behavior of members who dropped out of the DSG indicates that on the whole, they were no farther away from the centroid of the group than were those who remained. Any increases in DSG cohesion could not, therefore, have been due to loss of membership. The overwhelming majority of cases of membership loss has been due either to defeat at the polls or to retirement. The few members motivated to withdraw voluntarily were farther from the group center, in their last Congress of DSG membership, than was the average DSG member.

As established from Figure 3, DSG members tend to detract from the cohesion of the group during their first term of membership. Since the number of new members has varied substantially across Congresses, mainly owing to the shifts in electoral tides, another time series of cohesion figures for the DSG, Republicans, and southern Democrats should be examined. In this instance, we have labeled as members of each group only those who had also been members in the preceding Congress. (It should be noted that new southern Democrats and new Republicans, unlike new DSG members, show no tendency to be "off the reservation" in their first Congress of membership; indeed, more often than not, they are closer to the center of their respective groups during this period than is the average returning member.)

In Figure 4 an emerging trend toward increased cohesiveness on the part of the DSG can be observed between the 85th and the 90th Congresses even without comparison to southern Democrats and Republicans. These figures are more in accord with the message of Tables 3 and 4 than were those of Figure 2 in which new members were treated similarly in compilation to those longer exposed to communications within their respective groups.

We have presented evidence relating to each of the voting patterns to be expected if the setting up of continuous lines of communication among liberal Democrats did indeed lead to improved liberal performance on roll-call votes. In each case the expected pattern has been found—usually rather clearly. We cannot safely conclude that the organization of the DSG alone is responsible for this improved liberal performance without collecting and analyzing interview data on the subject, but this does seem to be an entirely reasonable inference.

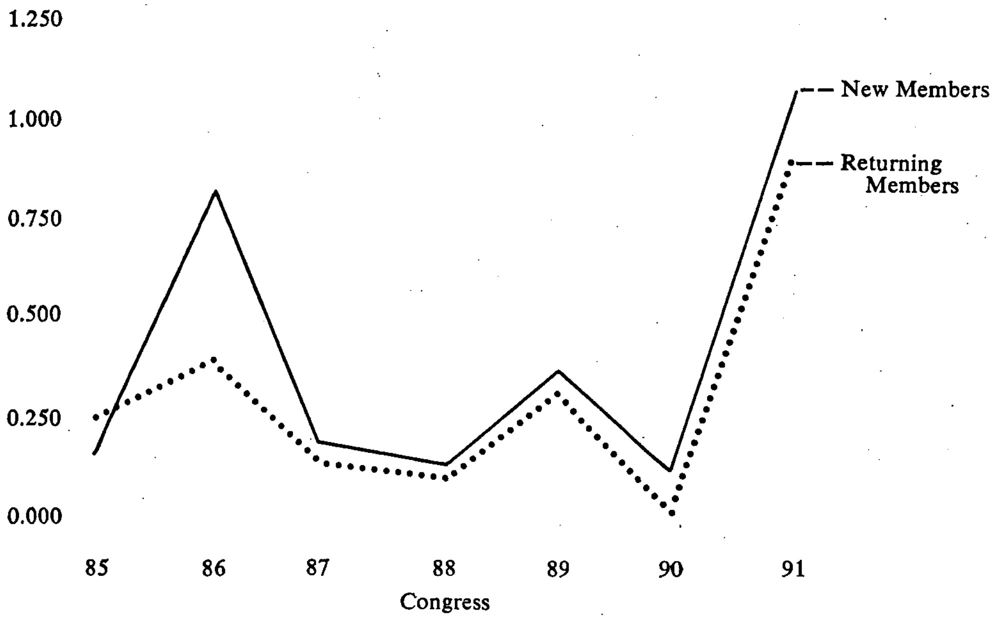


Figure 3—Distance of new and returning DSG members from the DSG centroid.

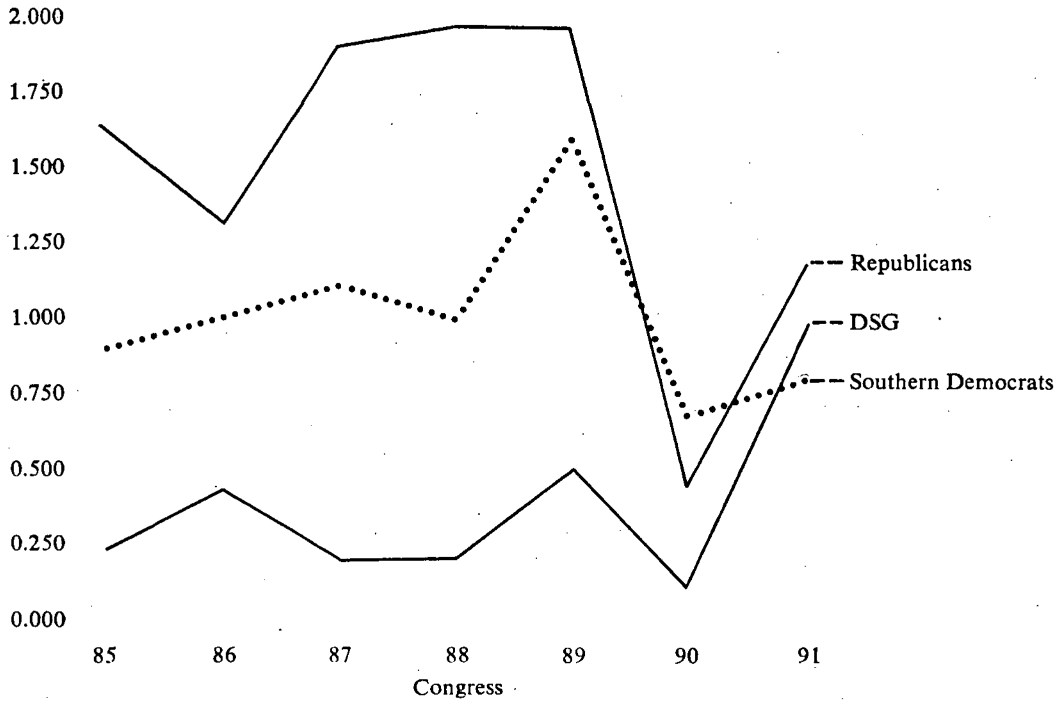


Figure 4—Distance of returning members from the centroid of their group.

Reversals of Trends in the 91st Congress. At a number of points in this paper, we have noted startling reversals of trends occurring in the 91st Congress. Interviews with our congressional respondents disclosed that that Congress had witnessed an unusually large amount of DSG dissension, most of it related to the Indochina war and the related questions of the balance of powers between President and Congress and the matter of national priorities. Indeed, a minority of DSG members in the 90th Congress had organized as a subgroup to press for U.S. withdrawal from Indochina. Labeling themselves "the Group," these fifteen or so members had by the beginning of the 91st Congress contributed substantially to the growing visibility of the priorities issue in the House—at no small cost to the internal harmony of the DSG. A glance at the factor score variance of DSG members on the three major factors underlying voting in the 91st Congress discloses unequivocally that their lack of cohesiveness relative to previous years was due almost entirely to the third factor. DSG variance on the three factors was .04720, .15782, and .74717, respectively. Indeed, when factor scores are recalculated on the basis of the first two dimensions only, the entries which would appear for the DSG in Figures 2–4 no longer appear aberrant.

Inspection of the factor analyses for the 91st and previous Congresses makes it clear that this third factor, which accounts for 15 per cent of the total variance, was unique to the 91st Congress. The nature of the roll calls loading heavily on Factor 3 substantially accords with our expectations: they occurred on bills dealing with placing limits on presidential spending, with military assistance to the Republic of China, and with national policy in relation to Vietnam. Factor analysis of the roll-call votes on bills emanating from the Foreign Affairs Committee documents the growth in the 90th and 91st Congresses of separate factors related to military spending and Southeast Asia policy. These war-related bills and resolutions from the Foreign Affairs Committee apparently evoked cleavages in the 91st Congress similar to those appearing on other matters concerning national priorities.

Examination of the factor-score variance of members of other groups on this factor shows that the DSG was far less cohesive on these votes than was any other group studied (see Table 5). The evidence strongly indicates that, inasmuch as normal cleavages were undisturbed for everyone except DSG members, the growth of this separate "national priorities" dimension can be attributed primarily to members of the Democratic Study Group. The

Table 5. Factor-Score Variances of Groups on Factor 3, 91st Congress

Group	Variance on Factor 3	Mean Score
DSG	.74717	.26198
Non-DSG Nonsouthern Democrats	.28293	−.56830
Southern Democrats	.16027	−.66446
Republicans	.33455	.13606

growing numbers of congressmen questioning U.S. involvement in Southeast Asia and its effects on domestic programs have come largely from the ranks of the DSG, although Republicans and non-DSG Democrats plus additional DSG members seem to be joining this dissent in substantial numbers in the 92nd Congress. The differences in direction of positions taken by members of the various groups on this factor in the 91st Congress are also indicated in Table 5.

Of perhaps vital importance to the continuing effectiveness of the DSG is the future of the third dimension of alignment which gained importance at the roll-call stage of the congressional process in the 91st Congress. As we have seen, this dimension has very strong foreign affairs overtones, but it also includes such matters as funding for the SST. Will the relative importance of this dimension continue to increase? If so, does this mean that the DSG will have difficulty maintaining even the degree of overall cohesion which it displayed in the 91st Congress? The answer to the latter question depends on at least two factors. The first has to do with the substantive composition of this dimension, specifically the proportion of related roll calls which are more or less purely in the realm of foreign affairs, as opposed to items such as funding for the SST. The second determinant has to do with shifting opinion on this set of issues within the DSG, and hence involves some mention of the role played by physical replacement of DSG membership.

Our factor analyses disclose that in the last two Congresses, Foreign Affairs Committee bills increasingly have been differentiated according to whether their content was primarily of a technical or a military nature. And, on the latter dimension, DSG members, badly split in the 91st Congress, since that time seem to have become increasingly united in opposition to U.S. military involvement abroad, especially in Southeast Asia.

If the cohesion of the DSG is indeed increasing on roll calls dealing with military aspects of foreign affairs, this is partially due to the physical replacement of DSG members. Unfortunately, the number of persons involved is so small that not much can be said about them systematically.

Conclusion

Our findings pertaining to cohesion indicate that for the most part DSG cohesion has increased relative to that of other groups and that the positions taken on issues by DSG members usually are easily distinguishable from those of nonmembers. More important, however, the evidence is unmistakable that the Democratic Study Group members are, on most issues, quite united and that they constitute a formidable bloc. The number and proportion of members comprising the cohesive core has increased rather steadily as the DSG has widened the scope of its research and communications network. The dissension in the DSG over matters pertaining to the war and national priorities appears not to have reversed this trend on issues within the major dimensions of conflict in the House, and there is some evidence that, even on roll-call voting, turnout of DSG members has been increased by DSG efforts.

The Democratic Study Group has been shown to be quite cohesive on most votes; even on the new third dimension, if the above argument is correct, DSG cohesion is increasing rapidly. Where, then, does the DSG go from here, in winning roll-call votes? This is a crucial question, because the DSG in the 92nd Congress, even with increased numbers, has lost several votes by very small margins. The optimal DSG strategy would appear to be to try harder to replace incumbent congressmen not identified with the DSG with freshmen likely to join. Just a few new members might make a great difference in vote outcomes. DSG leadership is not blind to this potential and, as indicated in our introductory comments, is increasingly emphasizing electoral activities.

The Measurement of Party Competition*

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To my knowledge, there are no general techniques to assess quantitatively the degree of party competition. The classification of systems as one-party, dominant one-party, modified one-party, two-party, and multiparty, as I will show below, does not even approximate such measurement.[1] Although there are several techniques for the United States, these measures designed for two-party systems cannot be generalized.[2] The attention devoted to this special case has reduced the range of comparison available to scholars interested in party-related phenomena. In addition it has actually obscured the nature of party competition since several conceptual problems arise in multiparty systems which do not confront the analyst of a two-party system.

Theoretical Justifications

Besides needing to measure party competition because so many scholars, commentators, and political actors use the term (often in conflicting ways), there are two major theoretical justifications for such an exercise.

Representative democracy entails the accountability of elected officials to the citizenry. This accountability may be achieved in several ways, but most procedures come down to some form of turnover in office. That is, there is an expectation on the part of representatives and voters that officials will not be self-perpetuating and that elections can, and in some cases do, lead to the replacement of one set of officials with another set. The chance, or probability, of turnover is perhaps the most salient feature of this system of accountability; and that probability, I argue, is the correct and most useful interpretation of the concept of party competition.[3]

Furthermore, since in most nations today parties are the primary means of organizing opposition and of presenting alternative slates of leaders or issues, the measurement of interparty competition may be viewed as the critical procedural datum in assessing the degree of "democracy" of a given system. Other features, such as justice, equality, and stability may be important values in a full assessment of democracy; but such "substantive" features may depend in part on the more "procedural" feature of party competition.

Indeed the degree to which highly valued goals of political life are furthered or hindered by increased party competition is an empirical question which cannot be answered unless one understands clearly the nature of competition and can measure it precisely. For example, to the extent that interparty competition is associated with greater accountability, one would expect to find less alienation in the electorate, more feelings of political efficacy, and in general a greater degree of legitimacy accorded to the regime. Similarly, competition in the sense of greater likelihood of turnover of elected offices might result in the reduction of social tensions because there is more chance that new groups will be represented in the government. (It might also be argued that high competition exacerbates social tensions because polarization of the electorate around party positions reinforces social cleavages.) In testing such hypotheses an accurate measure of party competition is needed.

Party competition, therefore, is an essential ingredient of any theory of representative democracy, at least as we understand it today. A second important theoretical reason for studying party competition concerns the eval-

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* Helpful advice and comments have come from Donald E. Blake, Alan C. Cairns, Ole R. Holsti, Jean A. Laponce, W. Phillips Shively, Paul M. Sniderman, and several anonymous referees for the *American Political Science Review*.

[1] One of the few discussions of the number of parties which takes seriously the complexity of multiparty systems is that by Giovanni Sartori, "European Political Parties: The Case of Polarized Pluralism" in *Political Parties and Political Development*, ed. Joseph LaPalombara and Myron Weiner (Princeton: Princeton Univ. Press, 1966). See also Sartori's "The Typology of Party Systems—Proposals for Improvement," in *Mass Politics*, ed. S. Rokkan and E. Allardt (New York: Free Press, 1970).

[2] See David G. Pfeiffer, "The Measurement of Inter-Party Competition and Systemic Stability," *American Political Science Review*, 61 (June, 1967), 457–467, and the references cited there. See also the provocative suggestions of Paul Hasbrouck, *Party Government in the House of Representatives* (New York: Macmillan, 1927), chapter 9 (as cited in Charles O. Jones, "Inter-Party Competition for Congressional Seats," *Western Political Quarterly*, 17 [September, 1964], 464ff).

[3] Mark Stern, for one, has also argued for a probabilistic conception of party competition: "Measuring Interparty Competition: A Proposal and a Test of a Method," *Journal of Politics*, 39 (August, 1972), 889–904. His method, however, is restricted to a two-party setting.

uation of political systems in comparative terms. Nations (and other organizations) clearly differ both in the degree and in the means by which opposition is organized. Opposition is a matter of degree in regard to whether it is permitted, effective, and reflective of important lines of cleavage in the society. For example, heightened competition between parties in one system may cross-cut other cleavages and thus contribute to the "suppression" of certain interests, while in another country party distinctions may coincide with important cleavages and thus competition may contribute to divisiveness, social tension, and instability by accenting social differences.

Whether one's concern is to construct an empirical, explanatory theory of representative democracy or to evaluate a political system, measurement of party competition in some sense of that term is essential. Beyond that, however, we may say that the measurement must exhibit certain properties in order to be useful. In particular, precise quantitative measures are needed, not just typologies or classifications. More important though less obvious, the measure needs to be cast at the interval level of measurement; this point will be defended in a later section of the paper after certain necessary preliminaries have been addressed.

Three arguments are particularly persuasive for building a more precise measure of party competition. All three apply whether one is concerned with cross-system comparisons or with longitudinal comparisons. First, the extreme cases of no possible competitiveness (for example, legal prohibition of opposition parties) or of extreme competitiveness (for example, four or five parties which regularly replace each other in power) are not difficult to identify but are also not generally of interest. Instead we are usually concerned with *small or intermediate* degrees of competition. For example, we wish to compare the relative competitiveness of the United States, Canada, Great Britain, and Australia so that we can see the relationship with other features of these systems, such as relative emphasis on social services. Similarly with longitudinal comparisons, small degrees of change are of greatest interest because that is what we expect to find in the short run. For example, are federal elections in Canada becoming more or less competitive? Or, is increasing urbanization in India associated with greater competitiveness?

Second, we generally wish to assert more than that party competition is related to some other variable. We also hope to assess the degree of importance of the one phenomenon in relation to the other, especially with certain factors partialled out. For example, in accounting for levels of voting turnout is party competition more important than the degree of exposure to mass media, with level of education of the population held constant or partialled out? Answers to such questions require very precise quantitative measures of all the variables.

Third, many scholars are concerned with whether a nation has a one-party, two-party, or multiparty system. Particularly in longitudinal analysis, one must have a precise and explicit method of determining the number of parties to count: Is system B changing from a multiparty to a two-party system? Any test of Duverger's hypothesis that single-member plurality electoral systems cause a reduction in the number of parties with a two-party system as a limit requires such precision of measurement. As I show below, the number of parties counted and party competition are two sides of the same coin.

In sum, then, the precise measurement of party competition supports the study of party systems and especially of the role of parties in representative democracy. Hence solutions to measurement problems may contribute more or less directly to theoretical advances in this area. At the same time, as will become clearer in the next few pages, final decisions about the measurement of party competition must await greater knowledge of party systems.[4]

Preliminary Approximations

The measurement of interparty competition (and possibly also intra-party factional competition, for example, in United States primary elections) has been based on three analytically distinct components. These are (1) *fragmentation,* or the number of parties (or candidates, depending on the formulation), (2) *fractionalization,* or the split of the votes (or seats of a legislature) among the parties or candidates, and (3) *uncertainty* about the outcome of an election or a vote in a legislature. The first two factors have been the major focus of attention of all previous measures, with results which I will spell out below. Uncertainty, I argue, is the critical dimension. Loosely, speaking, the less certain we feel about our ability to predict (or postdict) the outcome of an election or vote, the more competitive we feel it to be. In a competitive situation, the outcome could "go either way."

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[4] The interplay of "theoretical" and "operational" definitions is quite striking in the case of multiparty systems. Empirical findings using different measures of party competition "feedback" to clarify our intuitive understanding of competition.

In a two-party system the existence of three conceptually distinct aspects poses no problem for the measurement of party competition, since they are not empirically distinct and do not result in contradictory characterizations of the races. For example, fragmentation can pose no measurement problem because there are only two parties (by definition). In other words one of the three aspects is a constant. In addition, the split among the candidates or parties is no problem, since the vote for the loser is a function of the vote for the winner (i.e., 100 per cent minus the winner's vote).[5] Hence, one can express the fractionalization of the two-party system with a single number, whereas in a multiparty system this is impossible because of the greater diversity of situations encompassed by the same vote for the winner. Finally, uncertainty in two-party systems is thereby reduced to a bivariate situation: which of the two parties is going to win? Any way of predicting the two-party factionalization of the vote (or seats) is also a measure of uncertainty. In multi-party systems, on the other hand, most ways of measuring uncertainty confound the other two aspects. For example, in India an increase in the number of parties may signal *an increase or a decrease* in party competition (uncertainty) depending on other features of the contest.[6] Similarly, the amount of uncertainty when one party has, say, 40 per cent of the vote (or seats) is indeterminate until we know the fractionalization of the vote among the other candidates. For example, 40-39-21 is very different from 40-21-20-19, or, maintaining a constant level of fragmentation, note the differences between 40-39-21 and 40-30-30.[7]

The above problems distinguish a two-party setting from a multiparty one mainly because the latter is much more complex, and it is thus difficult to assign a single number which uniquely identifies the combination of factors involved. There is, however, a further aspect rarely mentioned in the literature which makes the measurement of party competition in most types of systems more difficult. I refer to the degree of party identification and party loyalty among voters and candidates.[8] This poses the following problem: since we want to measure the possibility of turnover from one election to the next, the degree of loyalty is crucial.[9] If the electorate remains more or less constant in size and composition and if everyone in the electorate is extremely loyal, and therefore very unlikely to switch to another party, a vote division of 53-47 (say) may in fact be a very uncompetitive situation. On the other hand where we find an expanding electorate, low degrees of loyalty, or sensitivity to issues and political events, a split of 60-40 or even 70-30 might be quite competitive because the cost of attracting new voters and the likelihood of getting voters to switch parties may be relatively low.

If this argument about the importance of party loyalty is correct, than it follows that we cannot generally measure the degree of competition solely by the *outcome* of contests as previous measures have done. Instead we must know, in addition, the distribution of voters (or officials, if looking at legislatures) in terms of loyalty and identification with party.[10] In other words, we must know the

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[5] Some measures of party competition rely on the difference between the winner and the loser. This is obviously a function of the same figures in two-party systems but not in multiparty systems.

[6] For a discussion of the Indian situation, see David J. Elkins, *Regional Contexts of Political Participation* (Doctoral dissertation, University of California, Berkeley, 1971), chaps. 3-5. Duff Spafford, "The Electoral System of Canada," *American Political Science Review*, 64 (March, 1970), 173, finds a similar situation in Canada: "When the number of minor-party candidates is increased, the share of seats won by a major party for a given own share of vote is augmented. . . . The dependence of major-party share of seats on the number of minor-party candidates explains the ability of parties to win majorities in the House of Commons with much less than a majority of the vote in their favor."

[7] Fragmentation and fractionalization should not be confused with the concept of fractionalization popularized by Douglas W. Rae in *The Political Consequences of Electoral Laws* (New Haven: Yale Univ. Press, 1967). His concept subsumes what, for present purposes, I have considered as two distinct concepts.

[8] By party identification I mean the psychological attachment of an individual to a party; by party loyalty I mean the pattern of voting with the party (as a citizen or as an elected official in the legislature). Obviously, one of the reasons for being loyal is one's sense of identification. Other reasons for loyalty, besides psychological identification, are analyzed by F. G. Bailey in terms of "moral" and "contract" groups in his *Strategems and Spoils* (Toronto: Copp Clark, 1969).

[9] It may turn out that party identification plays a lesser role in most other countries than it does in the United States. Regardless of the nature of that psychological attachment, the degree of loyalty to a party, which is surely a concept applicable everywhere, is the more important datum. Hence, when party identification is used in explaining the index below one could substitute any measure of loyalty instead—for example, caring which party wins or previous voting habits.

[10] One must be clear about the setting to which measures of party competition refer. In particular, one

"cost" of getting a voter or elector to switch to another party (or a new voter to join that party). If that cost is roughly constant (as it may be in a single system over time, for example), then we can measure competitiveness by the outcomes; but not otherwise.[11]

To gain a fuller understanding of the implications of my argument, let us examine some previous attempts at measurement and some special problems associated with multiparty systems. The first step involves clarification of what is meant by one-party, two-party, and multiparty systems.

One-party systems, strictly construed, are uninteresting from the point of view of competition. If there is by law or custom only one party and thus one set of candidates, interparty competition is impossible, whether competition is defined in terms of number of parties, disparities in vote shares or defection rates. More loosely construed, however, one-party systems are simply subsets of two- or multiparty systems. According to most previous analyses of party competition, if one party is "dominant" and there is only one other party, then it is a two-party system; if one party is dominant and there are several smaller parties, it is a multiparty system. Thus, I will not discuss these cases as a separate type, but focus instead on different ways of measuring competition in two-party and multiparty systems. The establishment of what we mean by "dominant," of course, is partly a function of party competition, since a dominant party is one we feel certain will nearly always win. Hence we must be mindful that *implicit* notions of competition enter unwittingly into concepts like dominance, and thus may not be serious counterexamples in discussing party competition in other contexts.[12]

Although obvious at first glance, the question of how many parties there are in a two-party system, and thus how to contrast it with a multiparty system, is extremely vexing.[13] As it turns out, it is particularly so for most discussions of party competition since the decision-rules for distinguishing between two-party and multiparty systems implicitly define the concept of party competition. For example, we all know that the United States has a two-party system, at least nationally, even though there are usually at least six or eight parties running candidates for President, the House of Representatives, and the Senate. The reason we disregard the minor parties in nearly all cases is simply that we believe that these parties have no chance of winning the presidency or winning enough congressional seats to influence legislation (and certainly not enough to control Congress).[14]

What does this imply about a definition of party competition? Clearly there is no doubt about *which two parties* have the *only* chances of winning, and thus, by corollary, we assume that the apparent impossibility of getting voters to switch to minor parties is diagnostic.[15] In other words, significant switching can occur only between the parties likely to win, and thus the number of parties in the system is defined

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must distinguish between competition for votes (for one seat or for all the seats) and competition for control of the government. In two-party systems, these are often the same, but not in multiparty systems. Generally this paper focuses on competition for votes for all the seats of a legislature or for an office such as the presidency.

A second distinction must be made between competition for a particular office and competition over a range of offices. In this paper I assume that measuring competition for a single office or type of office must be done before one combines values for a range of offices. (See also Stern, "Measuring Interparty Competition," pp. 891, 896.)

A third distinction contrasts the degree of competition over a long period with the measurement of competition at a particular moment in time. While averaging the single-moment values over the period in question may prove acceptable, it may overlook important trends; and the way in which one measures competition determines whether the index yields different values in the long and short run. When one considers the importance of party loyalty, the long-term commitments of individuals cannot easily be separated from competitiveness in the current election.

[11] This is another reason why Stern's measure is unsatisfactory: it is based solely on previous election returns and not on assessments of loyalty, even though constancy of voting pattern is assumed.

[12] The best definition of "dominant party" requires that the net defection rate between the dominant party and opposition parties does not significantly change the likelihood that the dominant party will retain control of the government. In other words, dominance is a structural constraint (of undefined origin) in the party system which inhibits the development of serious contenders. It is not an explantory term at all, but a purely descriptive one; we must still look for reasons for these structural rigidities.

[13] At the end of this paper I will show how my measure of party competition can be used to ascertain which parties are important enough to count in deciding whether the system is two-party, three-party, or more.

[14] This is true even of the Wallace vote in the 1968 presidential election. Wallace had no chance of winning the presidency, as he stated; his role was intended to be that of a "spoiler." Had he thrown the race into the Congress by deadlocking the Electoral College, we would still have no reason to reassess our estimation of the number of parties.

[15] Sartori, "European Political Parties," pp. 157, 167ff, likewise argues that the number of actual parties is not the same as the number by which we identify the system. Thus, he says if only four of ten parties are significant, then we call it a four-party system, not a ten-party one.

as those parties to which previous voters or new voters might shift in large enough numbers to change the party that wins. The situation is analogous in Britain where the Liberals get a substantial vote in some areas and much more overall than the minor parties contesting the American presidency. The chance of forming the government, however, is practically restricted to the Conservatives and Labour, and hence we call it a two-party system. By the same token, India is said to have a one-party-dominant system (federally), since only the Congress party has had any chance of forming the government. But India is a one-party-dominant-multiparty system, since there are many other important parties at the state level (not true in the United States) and since even federally it is conceivable for a coalition of small parties to form the government.[16]

This analysis should be related to Duverger's "classic" discussion of two-party and multiparty systems.[17] Classification into two- and multiparty has been based on percentage of the vote for minor parties over long periods of time. Thus, Duverger, for example, classified Britain as two-party because of the declining share of votes and seats to the Liberals after 1922.[18] This, however, is but an imperfect surface indicator of the shifts in party identification and loyalty which have made it increasingly difficult for the Liberals to gain any sizeable representation. The relation between my analysis and previous analyses, then, is not contradictory; instead I have shown why an emphasis on declining vote (and seat) shares is correct, even though there may remain many small parties which add up to a sizeable bloc.

So far, then, we may summarize as follows. First, the concept of one-party system is a proper subset of both two-party and multiparty systems. This is a surprising result only if we look primarily at the number of parties and vote shares without considering that competition depends less on fragmentation and fractionalization than on degree of commitment to one or more parties.[19] Second, we have seen that the distinction between two-party and multi-party systems also involves the concepts of party identification and party loyalty, since the distribution and intensity of loyalties determine the likelihood of sufficient vote shifts to change the winning party.[20] The overall conclusion, therefore, is that fragmentation and fractionalization (as measured by the outcomes of contests) do not measure competitiveness except when we can assume that the distribution of party loyalties and identifications is given. This requirement is generally met only for relatively short periods of time and is generally not met when comparing one nation with another.

A Further Approximation

Changeability is the salient feature of uncertainty. We consider a situation highly competitive if we believe the winner could easily have been different or is likely to be different in the near future. Low competition is, by contrast, characterized by our lack of "surprise" at the outcome since loyalties are fixed and change of support is more difficult.[21] The most competitive possible situation results from purely random influences which are unrelated to one another over time. The key step in translating this informal conception into a useful measure of party competition requires that we see changeability, randomness, and hence uncertainty as variable properties of party systems.

Regardless of how badly a party (or a legislative proposal) loses, there is some finite probability that it could have won. The closer the vote, the greater the probability, other things being equal. One thing which may not be equal, however, is the degree of attachment of voters to one alternative or the other. If voters are strongly committed to one alternative or strongly opposed to others, they are less likely to switch their votes. Hence, the degree to which we can conceive of the outcome being different depends on the degree of prior commitment in the system as well as on the actual division of the vote.[22]

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[16] Indeed, after the split in the Congress party, it is fair to say that a multiparty coalition controlled the Federal Government in India in 1969–71.

[17] Maurice Duverger, *Political Parties* (London: Methuen, 1959), pp. 206–280.

[18] *Ibid.*, p. 226.

[19] Note the similarity between this conclusion and the reasoning about competitiveness in Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957).

[20] Donald E. Stokes, "Party Loyalty and the Likelihood of Deviating Elections," in Angus Campbell, P. Converse, W. Miller, and D. Stokes, *Elections and the Political Order* (New York: Wiley, 1966), pp. 125–135, demonstrates this for the United States.

[21] By casting competitiveness in terms of "surprise," that is, amount of information conveyed, one can readily see that the measure of party competition is similar to the notion of entropy. See, for example, Henri Theil, "The Desired Political Entropy," *American Political Science Review*, 63 (June, 1969), 521–525.

[22] Sartori's concept of "polarization" approaches this from a different direction. By polarization Sartori means "distance" between the parties, which in turn

Table 1. Levels of Party Identification and Expected Votes in Five Hypothetical Electoral Systems (1000 Eligible Votes)

Stated Levels of Party Identification:[a]	Party A						Party B					
	(1) P_v	(2) P_c	(3) I	(4) N_i	(5) V_c	(6) M_i	(7) P_v	(8) P_c	(9) I	(10) N_i	(11) V_c	(12) M_i
System No. 1												
Strong	.95	.95	.90	300	270	14.25	.95	.95	.90	300	270	14.25
Medium	.75	.80	.60	200	120	30.0	.75	.80	.60	75	45	11.25
Weak	.50	.60	.30	100	30	20.0	.50	.60	.30	25	7.5	5.0
Total =				600	420	64.25				400	322.5	30.5
System No. 2												
Strong	.95	.95	.90	200	180	9.5	.95	.95	.90	300	270	14.25
Medium	.75	.80	.60	200	120	30.0	.75	.80	.60	75	45	11.25
Weak	.50	.60	.30	200	60	40.0	.50	.60	.30	25	7.5	5.0
Total =				600	360	79.5				400	322.5	30.5
System No. 3												
Strong	.90	.95	.85	100	85	4.5	.95	.95	.90	300	270	14.25
Medium	.69	.80	.55	200	110	41.4	.75	.80	.60	75	45	11.25
Weak	.42	.60	.25	300	75	50.4	.50	.60	.30	25	7.5	5.0
Total =				600	270	96.3				400	322.5	30.5
System No. 4												
Strong	.95	.95	.90	200	180	9.5	.95	.95	.90	200	180	9.5
Medium	.75	.80	.60	200	120	30.0	.75	.80	.60	200	120	30.0
Weak	.50	.60	.30	110	33	22.0	.50	.60	.30	90	27	18.0
Total =				510	333	61.5				490	327	57.5
System No. 5												
Strong	.89	.90	.80	200	160	17.8	.95	.95	.90	200	180	9.5
Medium	.70	.71	.50	200	100	54.6	.75	.80	.60	200	120	30.0
Weak	.33	.60	.20	110	22	14.5	.50	.60	.30	90	27	18.0
Total =				510	282	86.9				490	327	57.5

Key to symbols:

(1) P_v = probability of voting
(2) P_c = probability of voting for same party in two successive elections
(3) I = intensity = $(P_v)(P_c)$
(4) N_i = number of identifiers with stated strength of party identification
(5) V_c = expected number of consistent supporters = $(I)(N_i)$
(6) M_i = floating voters (expected number of supporters of other parties) = $(P_v)(1-P_c)(N_i)$

[a] There is no reason why one must use three levels of party identification. Any number will do, but three is a convenient summary for most voting studies since most questions about strength of attachment do not make many clear distinctions. Indeed, one might drop any reference to party identification and classify voters according to arbitrary points on the variables P_v or P_c. Degrees of loyalty, in other words, are more important than identification itself.

To explicate these observations more concretely, let us examine some hypothetical political systems in which we possess all relevant information to measure competition in the manner suggested here. By using hypothetical data we can more easily vary the assumptions and see the consequences of certain procedures. Assume an electorate of 1,000 persons in which each person may vote in several successive elections. Assume further that we have ascertained each person's voting record and intention so that we can determine the proportion of people with varying strengths of party identification who have always voted for the same party or who intend to in the near future.

Table 1 summarizes these data for five hypothetical systems. (These could be interpreted as the same system at five points in

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increases the difficulties of interchange between them. This is very close to an analysis based on the difficulty of defection. See Sartori, "European Political Parties."

time, such as presidential election years. For convenience, however, I will speak of them as separate systems.) Before drawing out the implications of this table for party competition, let me clarify some of the notations and assumptions. Since the purpose of this table is to demonstrate the importance of prior commitments in determining the possible outcomes of elections, it is particularly important to understand the variable called "intensity" (I) in columns three and nine. Intensity is used here in a fashion only partly similar to the ordinary laymen's term: it signifies a commitment, or at least a degree of psychological attachment. Unlike everyday usage, however, the variable has been explicitly defined here in terms of two other variables. (It may be that they are, in fact, effects and not causes of intensity, but that is irrelevant here.) It is quite possible to imagine a person who never considers the possibility of switching to a party different from his habitual one, and yet who does not vote regularly, for example if he assumed it was going to win anyway. Similarly we can imagine a person, say with high candidate orientation, voting dutifully but switching parties frequently. Hence, these two features, probability of voting and probability of voting consistently for the same party, are kept distinct, and their *product* is the operational indicator of degree of intensity. That is, only people likely to vote and to vote consistently score high on intensity. This, of course, avoids the problems of assessing directly voters' feelings about the matter.

These fractions labeled "intensity" must be interpreted carefully. They do not refer to how many people in a category will always vote a certain way; instead they reflect an expected level of support in that category over a specified period (for example, from early in the campaign until election day; between two elections; or for a longer period).[23] In a particular election, for example, all or none of the weak identifiers in System 1 might defect from Party A, but this is unlikely if the average intensity is .30. If we assume that the set of stimuli holding people to Party A and the set attracting them to Party B vary from election to election (and from person to person), we can see that the actual number or proportion of persons supporting Part A in any given category is a random variable. The mean of this variable is the proportion called intensity. Hence, the number of people in each category of party identification (N_i) times their intensity level (I) yields the expected support (V_c) for Party A in that category. V_c, then, is the number of people we would expect to find consistently supporting Party A in an indefinite number of replications of the election. (The "expected value" of a random variable is its mean.) The sum of these expected values is the total set of consistently loyal votes we expect Party A to receive in the long run if there are no secular trends in levels of identification or in intensities. In practical terms, this is the "hard-core" vote bank that Party A can count on consistently.

The consistent votes (V_c) are not the only votes a party may garner, but the votes picked up from other parties are harder to describe without further discussion of the data in this table. I will return to these "floating voters" shortly.

In the first three systems in Table 1, the intensities, the distribution of support among levels of identification, and therefore the expected votes are identical for Party B. Similarly Party B has identical parameters in Systems 4 and 5. The purpose of "holding Party B constant" is to see the effects on the outcomes when these features are varied in only one party. Note furthermore that the party division among people claiming a party identification is also identical across the first three systems, and again between Systems 4 and 5. Obviously if everyone voted their intentions, Party B would never win, just as the Republican Party in the United States would never win the presidency if everyone voted his current identification. The ways in which intensity levels influence the outcome may be seen here, as well as the effects of the distribution of supporters among the levels of identification.

In System 1 it is clear that Party B has an advantage over Party A in the larger proportion of its supporters who score high on intensity. Out of the whole electorate of 1,000, the hard-core voters differ by about ten per cent (roughly 42 per cent to 32 per cent) compared to the 60-40 split among all identifiers. This advantage may be seen even more clearly by turning to Systems 2 and 3. In System 2 Party A's intensity figures are the same as in System 1 but the proportion of its supporters in the strong identifiers category has declined. There we see that only about four percentage points distinguish the hard core supporters of the two parties. In System 3 the distribution across levels of identification is even less favorable to Party A, and in addition the intensity levels have dropped at each level of identification. (System 3 may describe the American presidential election in 1972, with Party A being the Democrats who have more identifiers but apparently at lower levels of commitment.) In this third case, the hard core supporters of Party B actually outnumber those of Party A.

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[23] In these examples, and in the empirical examples from Canadian elections in a later section, consistency is defined in terms of votes in two successive elections.

Systems 4 and 5 reveal the importance of prior commitments even more clearly. In the fourth example both parties have very similar distributions and identical intensities; thus the division of their consistent vote banks follows the party identifications very closely. Despite, or because of, the very close division of partisan identifiers, the outcomes will favor Party A more often than Party B; but it is manifest that it would not take much to shift the balance in B's favor. That is exactly what happens in System 5. By keeping exactly the same distribution of identification, and only shifting the intensity levels slightly, Party A's position erodes drastically as compared to the case in System 4.

From these considerations it is apparent that at least two pieces of information are needed to characterize a situation in terms of competition: the "distance" between the parties, and the degree of commitment (or conversely the "looseness" or "flexibility") in the system, such as the number of people with no party identification or weak identification. It is obvious that one cannot measure party competition solely by the degree of commitment (for example, by the percentage of people with weak party identification); one also needs to know how wide a gap there is between the parties for these "loose voters" to fill. For example, contrast Systems 1 and 4. The reverse situation is not obvious, however: the percentage difference is equally insufficient; we also need to know whether there are enough voters with low levels of commitment to bridge the gap between the parties at any given time. For example, contrast Systems 2 and 3 or 4 and 5.

In dichotomous terms, a situation is competitive when the potential number of loose voters (M_i) exceeds the difference between the two parties and is not competitive when it does not. (In this sense System 1 is not competitive; the rest are.) A dichotomy is not useful, however, we must devise a continuum. The *extent to which* potential loose voters exceed or fall short of observed party differences in vote or seat shares is such a continuum, and each point on that continuum has an associated probability value indicating the likelihood of turnover (of the seat or the government). Actually, that distribution is not as simple as it looks, since voters are not generally classifiable into "loose" and "committed." Instead as in Table 1 there is a function (P_c) relating the degree of attachment of any given voter to an alternative and the chance that he will switch. For example, Converse presents United States presidential election data on the likelihood of defection for each level of party idenitfication, and it is clear that even the most committed partisans will under some conditions defect, though their defection is less likely than that of less committed partisans or independents.[24]

One way to conceive of the probability distribution we seek is to imagine each person's potential vote as weighted by two fractions: (a) the probability of voting and (b) the probability of continuing to vote for the same party (or to vote for the party intended, in the case of new voters). The present usage of "intensity" assures this weighting. Such a calculation yields a number which does not correspond to any definable group of individuals but rather to the pool of individuals (different in different replications) who are relatively likely to switch. In Table 1, this group consists of the set (M_i). This number, as a percentage of the electorate (or legislature), would vary widely across systems and across time. The larger the proportion of individuals in that pool of loose voters, the greater the uncertainty (that is, the chance of turnover) given a specified percentage difference between any two parties. Similarly, given a specified pool of loose voters, competition would increase with reductions in the percentage difference between any two parties. Competitiveness thus depends on changes over time in the pool of voters who can be induced to support a party not now winning and on the observed difference between the parties. The measurement problem, then, reduces to how to estimate the degree of looseness and how to devise a "formula" for comparing the degree of looseness with the "distance" to be overcome. The key lies in the floating voters called M_i in Table 1.

A Proposed Measure (U)

The differences between N_i and V_c do not derive solely from defection to another party; votes are lost when eligible supporters do not turn out. Some parties lose more votes than others in this respect. Similarly, out of the potential party switchers many do not vote, especially at the lower intensity levels. The variable (M_i) reflects this fact: of party A's weak identifiers in system one who might switch parties, only half will vote, hence under column six one finds the number twenty instead of forty. Two critical conclusions may be drawn from this line of reasoning. First, one cannot assume that a voter lost to one party automatically migrates to another party. (This assumption will appear even more important later in discussing the multi-party case.) A related point concerns the fact that this "dropoff" may be due either to less commitment to consistency or to a lower propensity to vote at all. Second, if the parties have roughly equivalent distributions of levels of identification and of intensity, then the larger party suffers the larger dropoff and also provides more of the "loose voters" who are likely to vote and are available to vote for either party.

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[24] Philip Converse, "The Concept of a Normal Vote," Campbell et al., *Elections and the Political Order*, pp. 23, 27.

If we discount in advance the group I have called "dropoff," then we can estimate the size of the pool of floating voters who are likely to vote but could go either way. We can, of course, always discount the dropoffs, since it is quite simple in most voting systems to estimate them; and comparative evidence suggests that the figure is quite constant. For example, note the stability of turnout figures for any country in the last twenty years.

The interesting question is what to do with the "loose voters" or "floating voters." One might assume, for example, that they switch to the opposite party; that is, that everyone in columns (M_i) votes for the party opposite to the one they are now identified with. This assumption would ensure accountability in the sense that the parties would oscillate in office regularly. However, there would be no competition in the present sense, since there would be no uncertainty; each of the switching blocs would be, in effect, part of the party's vote bank in every second election. More importantly, however, we know this assumption to be unrealistic. The important question for any given election is what proportion of the floaters were garnered by which party. As I now show, this too may be considered a random variable and the probability associated with any given split in the floating vote is the measure of party competition which satisfies the criteria established in this paper.

It is apparent from the way in which V_c and M_i are calculated that they represent random variables. Furthermore, they are reciprocal: to the extent there is change in the intensities or in the proportions of people at the different intensity levels, V_c and M_i will be affected in opposite directions; they cannot both increase or decrease simultaneously unless voting turnout changes markedly. Hence, if we assume relatively constant levels of voting turnout in a particular system, we can safely focus only on M_i in determining the degree of "looseness" in that system, and hence the chance of turnover.

I have said that M_i is a random variable. In fact, in Table 1 every hypothetical system has six such random variables. Each of them has a mean (M_i) and a standard deviation (which can be calculated by means of the formula for a binomial distribution, since a "win" represents a vote for the party that a voter has stated an identification with). Therefore, if we combine these six random variables, we will get a single sampling distribution which represents the degree of flexibility in the system at the time of measurement. The standard deviation of that sampling distribution will be the metric for standardizing the distance between the parties for purposes of assessing the chances of turnover.

There are probably other ways of calculating the sampling distribution, but the procedure advocated here is simple and straightforward. Since in analysis of variance, the total sums of squares equals the between sums of squares plus the within sums of squares, we can use this formula to combine any number of random variables (M_i) and to calculate the overall standard deviation. The total sums of squares divided by the total population minus one yields the variance, and the square root of that is the standard deviation (SD hereafter).

Using this formula for hypothetical systems 1 and 4 (to provide contrasting situations) yields the following parameters:

(1) System 1: mean = 94.75 (or 9.475%); SD = 10.64 (or 1.064%) and (2) System 4: mean = 119.0 (or 11.9%); SD = 16.75 (or 1.675%). Just as we would expect from an inspection of Table 1, System 4 exhibits a greater degree of flexibility. This may be seen both in the larger mean number of floating voters (119 vs. 95, out of 1,000), and in the larger SD for the sampling distribution of floaters. That in itself, of course, does not ensure greater competitiveness, since we must also take account of the distance between the parties. Since the distance is also less in System 4, it is definitely more competitive as the figures below show clearly.

What we will use as the distance between the parties poses a small problem. In any real election, of course, the actual outcome in some sense determines the distance, though even there it matters whether one computes the percentage in terms of (a) major party voters only, (b) all voters, or (c) all eligible voters. On the basis of the hypothetical systems, however, I propose a different procedure which has the same effect. I will use as the measure of interparty distance the distance between the consistent vote blocs of the parties (V_c). Hence in System 1 the difference is 97.5 and in System 4 it is 6.0.

The reader might fear that this measure of distance renders actual election results less amenable to analysis. This is not the case. If the distance between parties is measured between the vote blocs (V_c), then the outcome of the election depends on the vote division *within* the floating vote, which of course is how the election was decided anyway. Using surveys one can disaggregate the actual election outcome into consistent voters (V_c) and floaters (M_i).[25] Then we know how the floaters divided, whereas in the hypothetical cases above we do not know that fact. In both cases we can calculate the probability distribution we seek. Given the consistent vote blocs in the hypothetical systems, how large a fraction of the floaters would the current smaller party need to overcome the larger party's lead in the vote blocs? How likely is that split of the floaters, assuming a random sampling distribution? In the case of an actual election, we know how the floaters divided, so we ask: *How would the floaters have had to split in* order for the loser to have won? How likely is that split given the observed split and assuming that the floaters' decisions are a random variable?

If A is Party A's share of the hard-core voters and B is Party B's share, and if A′ is Party A's share of the floating voters (either in the real election or in some hypothetical replication of it) and B′ is Party B's share, then A − B = the "distance" between the parties and $A' + B' = M_i$ (the number of floating voters available to overcome that distance). Hence, assuming Party A to be the larger party, B′ must exceed A′ by the amount A − B in order to reverse the election. Therefore, we must discover the probability that $B' - A' > A - B$, given the distribution of identifications and intensities in Table 1.

Assuming we cannot have fractional votes (though why not?), B′ − A′ must in system one exceed 98, that is 99 or more votes. Since the mean of the sampling distribution of floating voters is only 94.75 (=95), which is the expected value of that distribution, it is clear that it will take a very unusual election for Party B to win. First of all, the number of consistent voters must be below normal and hence the number of floaters above normal so that there will be enough floaters to make up the difference between the vote blocs. Second, should the proportion of floaters increase sufficiently, say by one standard deviation (i.e., to 105), Party B must convince virtually all of them to support it (at least 102 out of the 105). What is the combined probability of (a) such an increase in the number of floaters and (b) such a onesided division of the vote among the floaters?

An increase in the pool of floaters of one standard deviation would occur by chance only 16 times out of 100. Hence, we must multiply by .16 the probability of the required split (102 out of 105), which is infinitesimal. An even split of 105.4 (=52.7) is subtracted from 102 and the remainder divided by the SD of M_i (=A′ + B′) (10.64) yielding a z-score of 4.63. Hence the probability of turnover = (.16) (.0000034) = .00000054. If we call the measure of party competition (U_i), then in this case (U_i) ≐ probability of turnover = .00000054.

A different situation obtains in System 4. There only six votes separate the basic vote blocs of the two parties, and there are on the average 119 floaters. Hence, B has only to obtain 63 or more votes out of the 119 in order to overturn the current division. The chances of doing so, if the floaters behave randomly, are very high indeed.

Since here Party B needs a split among the floaters of 63 out of 119, we subtract half of 119 from 63 and divide by the SD of A′ + B′ (16.75), which shows that such a split is only .21 standard units away from the even split. Therefore, the value of $U_i = .42$ that is, about 42 times out of 100 Party B should replace Party A given the distribution of intensities and levels of party identification in System 4.

At the beginning I mentioned that an adequate measure of party competition must attain an interval level, that is it must specify a "distance" unit of measurement. The reason for this criterion can now be seen. The degree of commitment to alternatives in the system requires an interval level when compared to the "distance" to be overcome by potential loose voters. To argue otherwise is to assume that an individual could with equal ease switch to any alternative party, which appears to be a very strong, and probably false, assumption. Note that for a two-party system the ease of switching from party X to party Y does not need to be compared to the ease of switching to any other party. Hence a distance function does not seem necessary. To decide, however, that it is a two-party system, we must assume that the chance of switching to any of the minor parties is negligible. Hence, rank-order information is insufficient.

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[25] This procedure requires that we estimate party competition from survey data and not from official returns. Since we must know about party identification and intensity anyway, all procedures for measuring competition must rely on surveys.

Furthermore, to assume that an interval level is not needed is the same as saying that an interparty difference in vote or seat shares of x per cent is equally as difficult to overcome as $2x$ per cent or any other amount. This may be seen by recalling that the distance to be overcome in achieving turnover is a function of these percentages and the degree of looseness in the system. The absolute percentage differences must be standardized by reference to the potential variability in the system.

What is needed, therefore, and what the present method provides is a procedure analogous to z-scoring: the difference between the parties must be divided by the standard deviation of the distribution of all possible pools of potential voters, M_i, given the relative sizes of the groups which express stated degrees of identification with each party.

Such a procedure yields a unit which may be interpreted in the following manner: given the expressed levels of commitment in the system, the distance between the two largest parties (if that is the comparison of interest) is x standard deviations of looseness. This would allow precise comparison of nations (or longitudinal comparisons) because both objects of comparison would be measured in terms of the variability which is possible relative to the gap to be overcome.[26] For example, if two systems exhibit differences between their two major parties of m per cent and $2m$ per cent, both might be said to have the same chance of turnover, and hence the same degree of party competition, if the latter system also exhibited a standard deviation of potential looseness which was twice as large as that of the former system.

Application to Multiparty Systems

Although the examples so far have concerned two-party situations, the same procedures apply to more than two parties. The reason for this parsimony, which we expect any such measure to fulfill, is that the probability of turnover refers to the probability that *the most likely party* will replace the current winner. (Note that winner means the party which gains a plurality of votes. This does not preclude a game in which coalitions bargain to form the government, though that requires an additional set of computations.) We need to calculate U_i only once in a two-party system, since we know who the replacement must be, whereas for a system with several important parties we must calculate U_i for each comparison of winner and current nonwinner. In some cases it is apparent that a minor party is out of the running; nevertheless the calculation must include reference to all parties since even minute parties may contribute floating voters (M_i).

Some interesting features of multiparty systems may be exemplified by the three hypothetical systems in Table 2. (The U_i scores are reported in Table 3.)

Note first that the parameters (I, N_i and so on) are the same for Party A in all three hypothetical systems in Table 2. Similarly Party C's parameters are constant across the three systems. Hence the variation in degree of competition will be due to changes in the position of Party B when comparing Systems 6 and 7, and Parties B and D when comparing either System 6 or 7 with System 8.

Furthermore Party B has more identifiers (N_i) than Party C in every system. Hence by most measures of party competition it would have to be classified as the second largest party and the one most likely to replace Party A as winner. In the present analysis, however, the situation is more fluid and the distance between parties depends on their core of consistent voters (V_c). Since Party B has a smaller "hard core" of supporters than Party C in Systems 7 and 8, we would expect to find Party C being the party most likely to replace Party A in these two cases. Working out the sampling distributions of floating voters for each system, and making pairwise comparisons of Party A with each other party, we obtain the z-scores and probability of turnover (U_i) displayed in Table 3.

In Table 3 we see verification of these predictions. Party B is most likely to replace Party A in System 6, but in Systems 7 and 8 it is Party C which has this position. The reason, of course, that Party C's chances vary even though its parameters remain constant is the expansion and contraction of the pool of floating voters (and the variability of this pool) as the intensity levels of Party B change (and Party D in System 8). The importance to a party of being able to count on the support of its present identifiers and the importance of their turnout rates should not need further emphasis. It might be useful, however, to observe that in System 8, where there are four parties instead of three, and where we would thus expect to find more floating voters, we in fact find fewer than in either System 6 or 7. The reason for this is the "fanatical" character of the adherents of Party D. It is probably not uncommon to find very small parties in which nearly everyone who supports the party does so devotedly. Communist and Socialist Labor parties in the United States and Canada and Hindu revivalist parties in India might serve as examples of such groups. In other words, some of the flexibility of the party system is siphoned off into small parties which have no chance of winning but which make it more difficult for second and third parties to dislodge the current winner.

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[26] Although an interval level of measurement is mandatory, a ratio scale is not: first, because a zero point, while defineable mathematically, has no empirical interpretation (unless there is only one party, there is always some chance of turnover); second, because an interval level with an arbitrary zero point is just as useful as an absolute, or natural, zero point. For example, as z-scores get very large (say > 5), unity minus their associated probability approaches zero, and we can arbitrarily say that if $z > 5$, then there is zero competition for all practical purposes, regardless of what party system we examine.

An additional feature of the results in Tables 2 and 3 concerns the number of parties in these systems. On the basis of the rather low probability that any party might replace Party A, we might be tempted to say that all three of these systems are one-party or one-party dominant. Given the large size of Parties B and C, however, we might prefer to call them three-party systems. It is clear, whichever way we decide, that System 8 is not a four-party system, since the U-value for Party D is so small as to be meaningless. It is probably fair to say that any party whose chances of winning depend on an increase of the pool of floating voters of several standard deviations (nine SD's in System 8) must be considered out of the running. I will return below to this question of counting the number of parties. Here simply note that the U-values provide some guidelines for making an explicit decision based not on vague intuitions but on the apparent likelihood that a party could replace the current winner.

If there are three parties, A, B, and C, there are six possible rankings of the parties in terms of any given voter's preferred vote switching: ABC, ACB, BAC, BCA, CAB, and CBA. Furthermore, some voters may (a) be in-

Table 2. Vote Banks and Floating Voters in Three Hypothetical Multiparty Systems (1000 Eligible Votes)

Levels of Party Identification	Party A						Party B						Party C						Party D					
	(1) P_v	(2) P_c	(3) I	(4) N_i	(5) V_c	(6) M_i	(7) P_v	(8) P_c	(9) I	(10) N_i	(11) V_c	(12) M_i	(13) P_v	(14) P_c	(15) I	(16) N_i	(17) V_c	(18) M_i	(19) P_v	(20) P_c	(21) I	(22) N_i	(23) V_c	(24) M_i
System No. 6																								
Strong	.90	.95	.85	200	170	9	.95	.95	.90	150	135	7.1	.95	.95	.90	100	90	4.8						
Weak	.69	.80	.55	100	55	13.8	.75	.80	.60	100	60	15	.75	.80	.60	100	60	15						
Leaning/independent	.42	.60	.25	100	25	16.8	.50	.60	.30	75	22.5	15	.50	.60	.30	75	22.5	15						
Total =				400	250	39.6				325	217.5	37.1				275	172.5	34.8						
System No. 7																								
Strong	.90	.95	.85	200	170	9	.89	.90	.80	100	80	8.9	.95	.95	.90	100	90	4.8						
Weak	.69	.80	.55	100	55	13.8	.70	.71	.50	100	50	20.3	.75	.80	.60	100	60	15						
Leaning/independent	.42	.60	.25	100	25	16.8	.33	.60	.20	125	25	16.5	.50	.60	.30	75	22.5	15						
Total =				400	250	39.6				325	155	45.7				275	172.5	34.8						
System No. 8																								
Strong	.90	.95	.85	200	170	9	.89	.90	.80	100	80	8.9	.95	.95	.90	100	90	4.8	.95	.95	.90	10	9	.5
Weak	.69	.80	.55	100	55	13.8	.70	.71	.50	100	50	20.3	.75	.80	.60	100	60	15	.89	.90	.80	10	8	.9
Leaning/independent	.42	.60	.25	100	25	16.8	.33	.60	.20	100	20	13.2	.50	.60	.30	75	22.5	15	.75	.80	.60	5	3	.8
Total =				400	250	39.6				300	150	42.4				275	172.5	34.8				25	20	2.2

Key to symbols: see Table 1.

Table 3. Party Competition (U_i) Scores for Hypothetical Systems 6, 7, 8 (Based on Data in Table 2)

System and Parties:	Standard z-score	U_i	Most Likely Party to Replace Party A
System No. 6			
Party A vs. Party B	2.48	.007	Party B; $U_i = .007$
Party A vs. Party C	5.87	.0000001	
System No. 7			
Party A vs. Party B	6.04	.00000001	Party C; $U_i = .0002$
Party A vs. Party C	3.65	.0002	
System No. 8			
Party A vs. Party B	7.30	$< .000000001$	
Party A vs. Party C	5.66	.0000001	Party C; $U_i = .1 \times 10^{-6}$
Party A vs. Party D	12.29[a]	$\ll .01 \times 10^{-15}$	

[a] In addition, Party D needs an increase of the pool of floating voters of nine standard deviations, which has a chance of less than .0000001. Hence the probability of a deviation of 12.29 SD from an even split in the floaters has been multiplied by $.1 \times 10^{-6}$ to yield the U_i value.

different among the alternatives to their first choice, (b) prefer their first choice so strongly that they will not vote if they cannot support it, or (c) care so little for all the parties (or the electoral outcome) that they prefer in some circumstances not to vote at all. Each type of behavior influences the degree of flexibility in the system in different ways. The problem is to summarize this information in a single number.

The intensity figures serve as an adequate summary of this information if we interpret them as a product of two or more other figures. This is why I do not use (P_c) or (P_v) alone. For example, in Table 2, the intensity figure for the strong identifiers of Party A in all three systems is .85, which is the product of likelihood of voting (.90) and likelihood of voting for the party indicated—if the identifier votes—(.95). Similarly the identifiers who are "leaning" or independent have a probability of supporting their intended party of .60 (if they vote): their product equals the figure of .25 for Party A in these systems.

The problem of rank ordering the alternative preferences may be ignored, at least until scholars have had a chance to examine the usefulness of this measure of party competition. The reason is simple. The ranks are not crucial to the outcome if we assume that although the larger parties are likely to lose votes at a lower *rate* than the smaller (and the smaller to gain disproportionately), the actual *number* of transfers may be assumed to cancel out in a large number of replications when we also consider nonvoting (as in the previous paragraph). There is some evidence that this assumption holds in American presidential elections.[27]

Finally, when using survey data rather than hypothetical data, we already know how the floaters have divided their votes (if they voted). They have, thereby, *revealed their preference rankings*. The calculations of z-scores and U-values, therefore, reflect the comparison of their revealed rankings with an indefinite number of randomized replications of the election given the intensity figures. The point of the calculations is not to demonstrate that the outcome was random but to ascertain how far it deviates from a purely random outcome given the revealed preferences and the known intensity levels.[28]

Party Competition in Canada

To demonstrate that the procedures outlined above may be applied to multiparty systems with data which are normally available, let us examine recent elections in Canada. Although the 1972 election would be extremely interesting because it produced a minority government, no surveys are yet available so we must restrict ourselves to the 1965 and 1968 elections for which relatively reliable surveys *are* available.[29]

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[27] Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: Univ. of Chicago Press, 1954), p. 134.

[28] For a simple discussion of this type of "significance" testing, see Robert F. Winch and Donald T. Campbell, "Proof? No. Evidence? Yes. The Significance of Tests of Significance," *The American Sociologist*, 4 (May, 1969), 140–143. For more advanced discussions, see E. S. Edgington, *Statistical Inference: The Distribution-Free Approach* (New York: McGraw-Hill, 1969), and J. H. Chung and D. A. S. Fraser, "Randomization Tests for a Multivariate Two-Sample Problem," *Journal of the American Statistical Association*, 53 (September, 1958), 729–735.

Table 4 contains the data we need to calculate the *U*-values for 1965 and 1968. A few words of explanation might be helpful. First, note that P_v, P_c, and I exhibit the characteristics we have come to expect from the hypothetical examples. The intensity levels, for example, tend to decline as the strength of party identification declines, though the small number of cases in some instances makes the figures unstable. Also it may be noted that the New Democratic Party (NDP), a leftist trade-union oriented party, and the Social Credit and Creditiste parties, which are regionally based in the west and in Quebec respectively, do not exhibit the "fanatical" character of Party D in hypothetical System 8. They are relatively large parties rather than splinter parties with extremely dedicated hard cores.[30]

The category marked "all others" consists of a few respondents who indicated an identification with some other party (Communist, etc.) and of people with no stated identification, either because they had none or refused to reveal it to the interviewer. Needless to say most of these people are floating voters and a majority probably do not in fact vote, since the turnout figures they report are undoubtedly inflated. I have used their alleged voting rates, however, since there is no obvious way to "deflate" these figures accurately.

It is also of interest to observe that the totals and the intensities reflect the changing circumstances of the two elections. For example, the NDP did increase its support during that period, and the Liberals increased their majority. The intensity figures for the Liberals declined because they had garnered large numbers of floaters and former Conservatives, hence reducing the percentage of consistent supporters (P_c). Similarly the Social Credit and Creditiste parties were in disarray by 1968, and this is reflected in their

Table 4. Vote Banks and Floating Voters in Canada, 1965 and 1968 Federal Elections[a]

Levels of party Identification[b]	Liberal Party P_v (1)	P_c (2)	I (3)	N_i (4)	V_c (5)	M_i (6)	Progressive Conservative P_v (7)	P_c (8)	I (9)	N_i (10)	V_c (11)	M_i (12)	New Democratic Party P_v (13)	P_c (14)	I (15)	N_i (16)	V_c (17)	M_i (18)	Social Credit/Creditiste P_v (19)	P_c (20)	I (21)	N_i (22)	V_c (23)	M_i (24)	All Others[c] P_v (25)	P_c (26)	I (27)	N_i (28)	V_c (29)	M_i (30)
1965:																														
Strong	.90	.84	.76	225	170	32	.89	.84	.75	145	108	21	.90	.67	.60	71	43	21	.88	.63	.55	43	24	14	1.00	.38	.38	8	3	5
Medium	.88	.74	.65	398	259	91	.92	.72	.66	287	190	74	.90	.51	.46	109	50	48	.88	.43	.38	58	22	29	.71	.70	.50	14	7	3
Weak	.83	.61	.51	157	80	51	.88	.62	.55	114	62	38	.91	.33	.30	23	7	14	.76	.38	.29	17	5	8	.77	.41	.32	449	141	204
Total	.88	.74	.65	780	509	174	.90	.73	.66	546	360	133	.90	.55	.50	203	100	83	.86	.50	.43	118	51	49	.77	.41	.32	471	151	212
1968:																														
Strong	.91	.79	.72	347	250	66	.90	.80	.72	185	133	33	.93	.69	.64	98	63	28	.91	.38	.35	32	11	18	.86	.67	.58	7	4	2
Medium	.88	.66	.58	684	397	205	.90	.68	.61	329	201	95	.90	.57	.51	124	64	48	.84	.34	.29	49	14	27	.78	.22	.17	9	2	5
Weak	.81	.42	.34	236	80	111	.80	.51	.41	126	52	49	.82	.33	.27	33	9	18	.71	.10	.07	14	1	9	.75	.33	.25	494	124	248
Total	.88	.65	.57	1267	727	382	.88	.69	.61	640	386	177	.90	.59	.53	255	136	94	.84	.33	.28	95	26	54	.75	.34	.26	510	130	255

Data source: Federal Election Studies under direction of Professor John Meisel, Queen's University, Kingston, Ontario.

[a] Key: see note to Table I.

[b] The actual responses were coded as follows: (a) strong = "very strong," (b) Medium = "fairly strong," and (c) weak = "not very strong," "don't know," "refused to say," "not applicable" (i.e., no stated party identification at all but voted for the party indicated).

[c] This category contains two types of respondents: those who stated an identification with another party (for example, Communist) or who claimed no identification (the latter differ from "weak" identifiers above by not voting for one of the four major parties in the most recent election).

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[29] The Canadian surveys for 1965 and 1968 were kindly made available by Professor John Meisel, Queen's University, Kingston, Ontario. They may be obtained now through the Inter-University Consortium for Political Research. Some Gallup polls conducted by the Canadian Institute of Public Opinion also contain sufficient information for the computation of *U*-values, but the sampling frame is unstable and not as accurate as would be desirable.

[30] Brief histories of these parties may be found in Walter D. Young, *Democracy and Discontent: Progressivism, Socialism, and Social Credit in the Canadian West* (Toronto: Ryerson, 1969), and Hugh G. Thorburn, ed., *Party Politics in Canada* (Scarborough, Ont.: Prentice-Hall, 1967).

Table 5. Party Competition (U_i) Scores in Canada 1965 and 1968 Federal Elections

Year and Parties	Standard *z*-scores	U_i	Most Likely Party to Replace Liberals
1965:			
Liberals vs. Conservatives	2.43	.008	
Liberals vs. NDP	6.66	$.1\times10^{-8}$	Conservatives
Liberals vs. Social Credit/Creditiste	7.46	$.1\times10^{-10}$	
1968:			
Liberals vs. Conservatives	3.86	.00007	
Liberals vs. NDP	5.33	$.2\times10^{-6}$	Conservatives
Liberals vs. Social Credit/Creditiste	7.13	$.1\times10^{-10}$	

inability to retain their consistent supporters from 1965.[31]

Turning to Table 5, where the party competition scores for these elections are summarized, we find as expected that the level of competition declined from 1965 to 1968 in the sense that the Progressive Conservatives were not in as competitive a position in the latter year. However, the NDP improved its position, both in absolute numbers and by an increase in the intensity levels. Consequently, even though the NDP was obviously not a major threat to the Liberals, the party was more than holding its own in a year when Pierre Elliott Trudeau was leading the Liberals to a commanding majority in the House of Commons. The 1972 election saw a further increase in NDP votes and seats, apparently at the expense of the Liberals, although without survey evidence the exact pattern of party support is not entirely certain.

Therefore, even surveys which were not designed to measure party competition can clearly yield satisfactory data for the calculation of the measure (U_i) developed and exemplified in this paper. Since many other surveys in Canada, the United States. Britain, and Scandanavia. and perhaps elsewhere as well, contain information on party identification, strength of identification, and votes in successive elections, it should be easy to apply this measure to a wide range of circumstances to see how useful it is for comparative analysis.

Significance Levels and the Number of Parties

Starting from the premise that the most competitive situation is one in which random influences generate unstable outcomes over time, I have constructed a measure of the probability of turnover. The measure (U_i) yields a fraction which is, in certain respects, equivalent to "significance levels" in statistical tests. Unlike the usual interpretation of significance tests, however, in this usage there is no arbitrary rejection region. Instead the actual probability value can be interpreted as an estimate of the relative degree of "chanceness" of the observed outcome.[32]

If one interprets the values of U_i in Tables 3 and 5 as the literal probability of each party's opportunity to replace the current winner, then we must conclude that these replacements are highly unlikely. One must keep in mind, however, that the calculations are based on certain assumptions which are virtually always false. In particular, the assumption of complete randomness is a condition which is probably never satisfied. Thus, one must not interpret the U-values as indicating the exact probability of turnover in some literal sense. Rather they should be seen as indicative of relative likelihoods of replacement by the various parties. In other words, to establish an arbitrary rejection region of .05 or .01 or whatever would presuppose the satisfaction of the assumption about randomness and the designation of a zero-point. The categorization of regions of "significance" and "nonsignificance" is less important than the continuum which indicates approximation to a chance outcome. Hence, the less significant the results, the higher the level of party competition. (Again noting the parallel with entropy, one could say that the more significant the results, the greater the "surprise" value or information content when comparing the actual outcome to an indefinite number of replications.)

This interpretation of the significance levels of party relations allows a precise estimate of the number of parties in the system. As noted at the beginning of the paper, the number of parties by which we label a system is not usually equal to the actual number of parties which nominate candidates. This fact has been widely recognized, but the means of deciding exactly how many parties are important enough to count have been intuitive and nonformalized. Consequently there has been considerable controversy about particular systems such as Canada's.

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[31] Maurice Pinard, *The Rise of a Third Party: A Study in Crisis Politics* (Englewood Cliffs, N.J.: Prentice-Hall, 1971).

[32] See the references cited in footnote 28.

Since there is no single rejection region for the U-values, there can be no single value which clearly marks off the degree of importance that justifies counting a party. Nevertheless, two sorts of guidelines may be suggested for using the present measure of competition in making these judgments explicit. First, on the basis of experience with this measure, assuming that researchers apply it to several countries, one would want to set the "acceptance" level low enough so that every party which has ever replaced another party as winner or become the official opposition party during the period examined be counted in numbering the system. Only a wide range of application of this measure would clarify what the minimum U-value should be. Second, I suggest that no party be counted if the calculation of its U-value requires the assumption that the pool of floating voters expand above the mean value. For example, in Table 3, Party D of System 8 could not possbily win unless the pool of floaters expanded by approximately nine standard deviations. Obviously nine SD's are decidedly unrealistic, but even one SD reduces the chances of turnover markedly. Experience may, however, reveal instances of turnover occurring in such cases, and then we might wish to revise this rule of thumb. For the moment, however, I would count all parties which could win without an unusual expansion of the floating voter pool. Thus, on the basis of Table 5, Canada would have to be called a four-party system even though the U-values for the NDP and the Social Credit/Creditiste parties are extremely low. (If Social Credit and Creditistes are counted separately, since they have grown more hostile to each other, then it may be a five-party system.)[33]

This procedure can make no more claim for validity than the informed judgments that scholars have always applied. Unlike those judgments, however, it is explicit and precise and therefore readily corrigible. Disagreements about the numbering of particular systems can henceforth be cast in a common framework so that the basis for disagreement is more obvious.

Distinguishing Between Party Competition and its Causes

The emphasis on uncertainty, variability, and loyalty to a party may occasion some misunderstandings about the application of the present measure of party competition with regard to: (1) the uses of the measure in prediction and in description, (2) the distinction between party competition and effort by a party, and (3) the question of whose uncertainty is being measured and when. All three of these features are related to each other, and I believe that any difficulties they raise can be traced to a failure to distinguish between party competition and its causes.

For the practical politician, measuring the degree of party competition after the results are in is of scant interest; he is more concerned with predicting the outcome. The present measure was not designed to predict how a given contest (in one constituency, for example, or on one legislative bill) will turn out. Instead it is a descriptive statement of the *likelihood that the outcome could have been* different given the usual party differences and the average number of floating voters in the system. It is, however, useful for prediction in at least two ways. Before one can predict, or even explain, the outcome of such contests, one must be able to describe or measure the event. In other words, this measure delineates the phenomenon to be predicted. Similarly it is apparent that this method of measuring interparty competition is an aid to prediction in certain circumstances. As changes occur in the relative proportions of party identifiers or in intensity levels this method enables one to say how these will affect the impending election. For example, lowering the voting age in the United States introduces greater variability, and hence heightened competition, because the new voters are generally weakly identified with a party and have low scores on P_v and P_c.[34]

In order to provide a satisfactory basis for correct prediction, we must understand at least some of the most important causes of party competition. The present measure does not deal with that aspect of the analysis of party systems. In common parlance, however, many people (including political scientists) sometimes speak as though the causes of a given level of party competition were the level of competition itself. For example, sometimes the word competition is used to refer to the effort which parties expend in trying to gain voters. In the present context, on the other hand, effort is distinct from competition even though the level of competition may be partially accounted for by the efforts of the parties.

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[33] In fact, it is more reasonable to say that Canada has a three-party system because by 1972 Social Credit existed solely as a regional party. Thus, a further guideline might be that no purely regional party be counted in numbering the system. If one were calculating competition scores regionally, of course, then regional parties would still figure prominently.

[34] Hazel Erskine, "The Polls," *Public Opinion Quarterly*, 35 (Fall 1971), 489–495; and Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970), chapters 5 and 6.

It must be emphasized that competition refers to the probability that the outcome could have been otherwise given the distribution of loyalties. Unusually high levels of effort in any given election may result in turnover of seats, but it is the probability of turnover, not the effort or the actual turnover, if any, which we call party competition. To the extent that greater efforts by underdog parties loosen voter attachments and result in greater vote variability over time, party competition is enhanced, but when these efforts are counterbalanced by the similar efforts of all the parties, then competition will not generally increase because people will remain committed to previous attachments. Indeed, one interesting consequence of the present approach is that the efforts of any one party are generally negatively related to competition, since low competition is indicated by the large amount of effort it would take to get a voter to support another party than the one he supports (or plans to support).[35]

Uncertainty, in this discussion, is perhaps an unfortunate word, since it connotes for many people a subjective state which must be located in a person. Uncertainty is used here, however, more nearly in the information-theory sense; it is therefore an objective feature of the situation and not a subjective state. Uncertainty, then, means the likelihood of being wrong if one bets on a party other than the current winner, given the distribution of party identifications and loyalties and the vote differential in similar elections in the past. Obviously, if vote swings in the system under study have typically exhibited enormous variation, and parties have alternated irregularly in office, one is likely to be wrong to bet on one party consistently. If, on the other hand, the vote banks have been quite stable and the same party generally wins, then one would be wrong most of the time in betting on any other party. This conception of uncertainty does not, then, depend on the time at which a "bet" is placed (i.e. during the campaign, after the election) nor on the person placing the bet.

Related Research

Other scholars have proposed concepts which are relevant to the analysis presented here, though none have explicitly linked them to the measurement of party competition. It will be helpful to put the present approach in the context of these related theoretical constructs.

Converse's concept of the "normal vote" is closely related to the social and political processes conceived as underlying the analysis of party competition presented here. The analysis of the normal vote, he says,

> suggests a means of splitting the actual vote cast by any part of the electorate into two components: (1) the normal or "baseline" vote division to be expected from a group, other things being equal; and (2) the current deviation from that norm, which occurs as a function of the immediate circumstances of the specific election. . . . This leads immediately to the concept of net short-term partisan forces. As in other realms, the net force cannot be directly measured; rather, it is posited and measured in terms of its observed effects. In this case, the observable effects have to do with the defection rates of classes of party identifiers. . . . The probability that any individual will succumb to such pressures is a simple function of the strength of felt loyalty.[36]

Converse highlights the central point in the present analysis when he suggests that party loyalty or identification is a constraint on variation in party voting and hence on party competition as defined here. The distribution of party identifications as "predictors" of alternative vote outcomes has been used here to estimate the likelihood of the outcome of the election which is being measured. Besides these features, mention should also be made of Converse's analysis of the effects (or lack thereof) of differential turnout levels, which show for the American case that these can be ignored in calculating the sampling distributions.[37]

Stokes has come closer than anyone else to the present strategy of measuring party competition, although without overtly stating the relevance of his analysis for this question. Particularly in his article on "Party Loyalty and the Likelihood of Deviating Elections," he has stated the basic point succinctly: "Given the distribution of party identification found today—or any other distribution of party loyalties—what is the likelihood that the minority party will win?"[38] By combining his analysis with the observed vote differential, I have suggested a plausible measure of party competition.

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[35] See the provocative formalizations of this feature in William McPhee and Jack Ferguson, "Political Immunization," and William McPhee, Bo Anderson, and Harry Milholland, "Attitude Consistency," both in *Public Opinion and Congressional Elections*, ed. William McPhee and William Glaser (New York: Free Press, 1962).

[36] Philip Converse, "The Concept of a Normal Vote," in Campbell et al., *Elections and the Political Order*, pp. 11–16, 18.

[37] *Ibid.*, pp. 28–30; that is, defection rates are inelastic with respect to turnout, at least within the range of turnout variation found in American presidential elections.

In another article, Stokes and Iversen have forged a further link with my analysis.[39] They demonstrated the extremely remote probability that presidential and congressional elections in the United States could have remained within the narrow range of variation historically observed without some powerful forces working to restore party competition. By party competition here, they implicitly mean exactly what I do: namely, the probability that the minority party might become the majority.

Jean Laponce has shed further light on the forces underlying Stokes and Iversen's analysis by his experimental measurement of the tendency to equibalance.[40] Laponce's results suggest that societies (and groups within societies) differ in their propensity to support underdogs and thereby to enhance the possibility that future electoral verdicts will result in a different winner. This, in other words, is a cultural force working to create a given degree of "looseness" in attachment to a party over time.

Sellers's concept of an "equilibrium cycle" in American politics is also germane to the present anaysis.[41] If he is correct, then two conclusions stand out. First, there are long term "tides" in American voting patterns, as well as the short-term partisan forces which Converse deals with in the normal vote concept. These constrain the variability, and hence the chance of turnover, at various periods and for varying lengths of time. Second, his findings suggest that the calculation of distributions of loyalties should be restricted in time to a single period or tide. By focusing on a single era, one captures more accurately the range of variation possible or normal then, whereas over a long period the range is somewhat different and thus gives a misleading indication of the current chance of turnover.

Implications for Campaign Strategy

Although the measure of competition proposed here does not allow accurate prediction of outcomes, it does entail certain conclusions about campaign strategy. These reinforce common sense political wisdom and accord with considerable bodies of research findings on voting and elections in western democracies.

Referring back to Tables 1, 2, and 4, we note that the relative consistency and identification tendencies which form the base for calculating the *U*-values are the very components of electoral strategy that most astute politicians attempt to enhance. For example, at least the following propositions may be derived, if one's goal is to augment a particular party's support base and chances of winning, whether the party is currently a winner or not.

(1) Efforts should be directed to increasing the proportion of one's supporters in the strong identifiers category.

(2) Try to enhance the probability of consistent voting in every category. Which category will deserve the greatest attention depends on the relative sizes of the categories and on the level of consistency already achieved. For example, if the strong category has $P_c = .90$ or higher, very little gain will result from marginal efforts there even if many supporters manifest strong identification, whereas attempts to raise the moderate or weak categories from .30 to .50 would yield high dividends if there are large numbers of identifiers in these categories.

(3) Try to weaken the commitments of opponents' supporters by playing down the importance of party, by undermining the issue differences which attracted supporters to those parties, and by having an attractive candidate. In short, create cross-pressures or exploit cross-pressures within their ranks while trying to avoid cross-pressures between party, issues, and candidates in your own party.

(4) Campaign to "get out the vote" if your own supporters have low P_v scores; do not try to increase turnout if your supporters are more likely to vote than those of your opponents anyway.

Several strands of research have demonstrated the validity of these maxims in the American context, and one suspects that research will uncover similar processes in most open elections. The emphasis on the role of party identifications in general, and strong attachments in particular, in American presidential elections is so well known that no elaboration is needed. Nevertheless, one easily overlooked practical implication of the research on party identifications concerns the importance that candidates place on consolidating people who are already committed to the party. Only when you are sure of the hard core do you extend the appeals to independents and members of opposition parties.

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[38] Stokes, "Party Loyalty and the Likelihood of Deviating Elections," p. 128.

[39] Donald E. Stokes and Gudmund R. Iversen, "On the Existence of Forces Restoring Party Competition," *Public Opinion Quarterly*, 26 (Summer, 1962), 159–171.

[40] Jean A. Laponce, "An Experimental Method for Measuring the Tendency to Equibalance in a Political System," *American Political Science Review*, 60 (December, 1966), 982–993.

[41] Charles Sellers, "The Equilibrium Cycle in Two-Party Politics," *Public Opinion Quarterly*, 29 (Spring, 1965), 16–38.

McPhee, Anderson, and Milholland have shown that the most adequate model of campaigning is one which reflects the endeavor to create attitude consistency so that positions are more polarized at the end than at the beginning of a campaign.[42] In particular, one plays on the elements of a belief system which predispose an individual to one's point of view, and in many cases the other opinions will change to accord with that choice, thus bolstering the initial choice. This also works to solidify the consistency of voting support over time because people find it psychologically difficult to abandon a position that is more established.

The concepts of normal vote and of deviating elections clearly rest on the central role of campaign appeals designed to shore up one's usual base of support, and to try to erode the strength of commitment of opposition supporters.

The degree to which a party uses party labels and appeals, as opposed to stressing candidate image and issues, depends in large part on how numerous its hard-core identifiers are compared to those of its major opponents. Thus, in the United States, where Democratic identifiers outnumber Republicans by two to one, most Democratic presidential candidates emphasize party, while Republicans emphasize other features. (The Committee to Re-elect the President was not called the Committee to Re-elect the Republican President.)

The fruitfulness of this conception of party competition for campaign strategy is of course not important in evaluating its validity and usefulness as a measure. Nevertheless, the congruence between previous research and impressionistic observations of campaign strategy, on the one hand, and the logic of the measure, on the other hand, suggests that the overall conception is accurate even if later research leads to modification of details of the calculation of U_i.

Conclusion

The key to understanding the present approach to party competition is to realize that randomness is a property of party systems. One must be careful, however, not to infer too much from the word *random*. Stripped of its metaphysical overtones, the condition of randomness simply means variability owing to the operation of causes which are largely or wholly independent of each other. It would be surprising if some degree of randomness did not characterize all human actions particularly when considered in the aggregate, since "independent causes" is simply the opposite of monocausality. If there were a single overriding issue or concern for every voter in a given election, and each understood that issue in the same way, then there would be no variation in voting. This is clearly not the case.

Given what we know from voting studies and attitude surveys about the causes and correlates of voting, it is hardly remarkable that many causes operate independently and that the net result is, to one degree or another, random. At the same time, the stabilizing effects of party identification are well established, particularly in the United States, but also in several other countries. Party loyalties differ in strength from country to country and era to era within the same country.[43] Hence, the type and degree of constraint that party identification and loyalties place on voting are important in measuring interparty competition. To the extent that a party can assure itself of continued support of a group of people the flexibility of the party system, together with party competition, is decreased.

In order for the word competition to have meaning, there must be some possibility of change in the control either of a seat or of a government. If everyone is "locked in" to a particular party, then the only change can come from expanding the electorate. Hence, party competition requires additions to the electorate and defections across party lines. In this paper I have tried to develop these intuitive ideas, which have been well known for some time but applied to other problems, into a more formalized measure of party competition. There cannot be, at this stage, one correct procedure for the measurement of a complex phenomenon like party competition; but there must be a core concept to which we can refer each operational definition. Randomness or uncertainty is such a core concept.

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[42] See the references in footnote 35. Berelson et al., *Voting,* make the same point in less formal terms.

[43] The stabilizing effect of party identifications can change quickly in some cases. See for example, David R. Cameron, "Stability and Change in Patterns of French Partisanship," *Public Opinion Quarterly,* 36 (Spring, 1972), 19–30.

Toward a Model of Soviet Decision Making: A Research Note*

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While Western scholars have long focused on the study of Soviet elites and interest groups, they have virtually ignored the problem of constructing a dynamic model of decision making in the USSR.[1] Although such modesty is undoubtedly well founded in light of the complexity of the task and the limited availability of first-hand data, it is nonetheless possible to offer a partial model of decision making cast in terms of the level of conflict intensity within the political system and the corresponding mode of decision-making behavior. Such a model would, in addition to ordering our thinking about the political, social, and organization processes through which decisions are reached, also serve to clarify the debate over the rationalization of decision making in the post-Stalin era.[2]

The Rationalization of Decision Making

The literature on elite change in the USSR offers at best an inconclusive picture of the rationalization of Soviet decision making. While numerous studies point to the increased education and diversified occupational experiences of the elite, others remind us that the Kremlin's "professional politicians"—that is, leaders whose primary career experiences have been associated with political-administrative posts within the Party apparatus—still dominate top decision-making bodies.[3] Moreover, no attempt has been made to link changing recruitment patterns with corresponding changes in the behavior of the more technically educated or occupationally diversified members of the elite; rather it is assumed that such experiences must of necessity alter the frame of reference of decision makers.[4]

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* I would like to thank the International Research and Exchanges Board and the Ford Foundation for their support of my research on decision making in the Soviet Union.

[1] Ploss speaks of a "legislative procedure"; see Sidney Ploss, *Conflict and Decision-Making in Soviet Russia* (Princeton: Princeton University Press, 1965), pp. 283–287.

[2] By "rationalization of decision making," I refer to a process whereby an increasingly wider segment of the society's decisions come to be made on the basis of instrumental criteria. In the literature of political development, such changes are seen as the consequences of modernization, social differentiation, functional specialization, and the increasing complexity of both technical and social issues. Thus rationalization entails (1) increased reliance on rational-technical decision-making criteria, (2) the decline of ideology as a dominant decision-making criterion, and (3) an increased role in decision making for those elements of the society who possess technical skills. The newfound role for those holding technical skills may come about through the increased reliance on specialized groups or through a process by which the political elite itself acquires the necessary skills. Increased reliance on specialists will be noted below in the discussion of interest groups; for evidence of the political elite's acquisition of technical skills, see both the comments on elite change below and the writings of Paul Cocks and Jerry Hough on the rational-technical work style of Party administrators: Jerry Hough, *The Soviet Prefects: The Local Party Organs in Industrial Decision-Making* (Cambridge: Harvard University Press, 1969), pp. 272–320, and Paul Cocks, "The Rationalization of Party Control," in *Change in Communist Systems,* ed. Chalmers Johnson, (Stanford University Press, 1970), pp. 181–182. Philip Stewart's study of elite mobility suggests, however, that rational-technical performance criteria explain only a part of the upward mobility of secondary Party figures, with other factors such as patron-client ties and the level of political conflict equally important at times; see Philip D. Stewart, Robert L. Arnett, William Ebert, Raymond E. McPhail, Terrence L. Rich, and Craig Schopmeyer, "Political Mobility and the Soviet Political Process: A Partial Test of Two Models," *American Political Science Review,* 66 (December, 1972), 1269–1291.

[3] Fleron offers a useful distinction between "coopted" Party leaders who have had extensive career experience in nonapparatus technical or administrative roles and "recruited" leaders who enter full-time Party work shortly after the completion of their education. While the former have enjoyed increased membership in the Central Committee in recent years, the latter have continued to dominate the Politburo and the key apparatus posts. See Frederic J. Fleron, "Toward a Reconceptualization of Political Change in the Soviet Union," *Comparative Politics,* 1 (January, 1968), 228–244 and "Cooptation as a Mechanism of Adaptation of Change: The Soviet Political Leadership System," *Polity,* 2 (Winter, 1969), 176–201. For an updating of Fleron's figures, see Robert H. Donaldson, "The 1971 Soviet Central Committee: An Assessment of the New Elite," *World Politics,* 24 (April, 1972), 382–409. George Fischer offers similar observations in *The Soviet System and Modern Society* (New York: Atherton Press, 1968), pp. 47–64. For more general data on elite change, see Michael P. Gehlen and Michael McBride, "The Soviet Central Committee: An Elite Analysis," *American Political Science Review,* 62 (December, 1968), 1232–1241.

[4] To be sure, Milton Lodge offers a study of the changing attitudes of five elite groups. Two points, however, must be made regarding his findings: (1) His is a study of group *attitudes* toward decision making based on content analysis of specialized journals which speak for these groups and not a study of their actual

The literature on interest group activity is also inconclusive. Some studies unquestionably demonstrate that interest groups have been active and influential in the policy-formation process.[5] It remains clear, however, that the impact of such groups is frequently checked by the presence of countervailing groups or by the reluctance of top Party leaders to surrender their prerogatives on sensitive political questions.[6] Moreover, this body of literature fails to confront the question of whether consultation with such groups automatically means that more rational decision making will emerge. While it seems likely that some groups have exercised influence on policy because they had the needed technical expertise (e.g., the influence of Soviet educators on the 1958 school reform), it is also evident that others have gained influence simply because competing leaders sought to win the support of certain groups with politically relevant resources such as representation on the Central Committee (e.g., the influence of the non-technically specialized regional Party secretaries on the 1957 decentralization of Soviet industry).[7]

Multifunctional Decisions and Rationalization

One fundamental problem in assessing the rationalization of decision making in the Soviet Union stems from the simple fact, long recognized by students of administrative behavior, that *no* decision is ever reached completely on the basis of rational-technical considerations. While Herbert A. Simon attributes this inevitable element of nonrationality (*not* irrationality) to the limits of available information and the decision maker's inability to consider all possible courses of action, a deeper source of nonrational conduct becomes evident when the larger social and organizational implications of the decision-making process are brought into view.[8] This added dimension is the multifunctionality of virtually all decisions; they deal not only with the problem-solving situation confronting the decision maker but also with the interrelationships of a multitude of organizations and institutions and their self-images.[9] Thus a decision maker must operate simultaneously within two frames of reference. The first is the problem-solving situation, in which he is concerned with selecting a proper response to a problem which has mobilized the organization to action. The second—and the more complex—deals with the organization's "fit" in a complex social environment and with its sense of mission and purpose. This search for a proper "fit" is directed toward the extra-organizational environment and usually takes the form of bargaining and adjustment in light of the overlapping claims of competing organizations. The second process is internal to the organization itself and is directed at preserving its own identity, sense of mission, and role concept. Thus the task is primarily interpersonal and psychological—winning the continued support of the members of the organization and defending its self-definition and sense of mission against undesired alteration. Virtually all decisions made within an administrative or bureaucratic environment are multifunctional in the above sense.[10]

Therein lies the dilemma. While the technical and problem-solving aspects of a decision are subject to rational-technical (means-end) analysis, the larger questions of the organization's political and social role and its self-image are not. Paul Diesing has offered an instructive distinction between "economic" (goal-oriented) decisions, which maximize the achievement of given ends through the rational use of available means, and "social" or

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behavior in decision-making situations. (2) His study makes no effort to relate changing attitudes to the educational or occupational experiences of group members. See Milton Lodge, *Soviet Elite Attitudes Since Stalin* (Columbus, Ohio: Charles E. Merrill, 1969). Lodge has subsequently enlarged the focus of this original study to include an examination of the relationship between career experiences and policy preferences. See his article "Attitudinal Cleavages within the Soviet Political Leadership," in *Comparative Communist Political Leadership,* Carl Beck et al. (New York: David McKay, 1973), pp. 205–225.

[5] See H. Gordon Skilling, "Interest Groups in Communist Politics," *World Politics,* 18 (April, 1966), 435–451; Richard M. Mills, "The Formation of the Virgin Lands Policy," *Slavic Review,* 29 (March, 1970), 58–69; D. D. Barry, "The Specialist in Soviet Policy-Making: The Adoption of a Law," *Soviet Studies,* 16 (October, 1964), 152–165; Philip D. Stewart, "Soviet Interest Groups and the Policy Process: The Repeal of Production Education," *World Politics,* 22 (October, 1969), 29–50; and J. J. Schwartz and William Keech, "Group Influence and the Policy Process in the Soviet Union," *American Political Science Review,* 62 (September, 1968), 840–851. A good summary of such group activity is to be found in *Interest Groups in Soviet Politics,* ed. H. Gordon Skilling and Franklyn Griffiths (Princeton: Princeton University Press, 1971).

[6] Donald R. Kelley, "Interest Groups in the USSR: The Impact of Political Sensitivity on Group Influence," *Journal of Politics,* 34 (August, 1972), 860–888.

[7] *Ibid.*

[8] Herbert A. Simon, *Administrative Behavior* (New York: Free Press, 1957), pp. 81–84.

[9] William J. Gore, *Administrative Decision-Making: A Heuristic Model* (New York: John Wiley & Sons, 1964), pp. 125–126.

[10] Of course it is possible that such a decision will not satisfy both criteria. In this case, it will either fail to deal with the objective problem or produce organizational tensions which, if severe, will require a second series of decisions to return the organization to equilibrium or to redefine its place in the society.

"integrative" decisions, which deal with the institutional and social impact of decisions. In the former, "maximizing" decisions are reached through the rational calculation of the greatest possible output from the limited available resources; in the latter, "social" or "integrative" decisions are made by balancing the interests and self-images of the participating organizations and by searching for ways of anticipating and reducing the strains which inevitably accompany decisions.[11] Diesing posits a relationship between the nature of the decision and the style of decision-making behavior:

> A broadly integrative method is called for if a problem is basically one of disorganization and conflict, while supplementary maximizing considerations are necessary if the solution to a disorganization problem requires the use of scarce resources. A basically maximizing method is called for if a problem is basically goal-oriented, while supplementary integrative considerations are necessary if incidental elements of disorganization or potential disorganization are also involved. A blend of both methods is called for if a problem includes both important goals and important elements of internal conflict.[12]

Diesing acknowledges, however, that there is no way to combine both maximizing and integrative approaches, leading decision makers to rely basically on one or the other and to deal *ad hoc* with the factors not suited to their chosen method.[13] While he holds that a combined method is possible which "subordinate(s) integrative decision processes without destroying their essential characteristics," he offers only a few practical suggestions for how this balance might be struck.[14] One is to allow for a margin of waste in allocational decisions, since such "wasted" resources are used as concessions in the bargaining process and are therefore important factors in preserving organizational stability. In the Soviet context, however, this approach is difficult because of the continuing scarcity of key resources. Alternatively, a decision maker may either allow for a margin of organizational inefficiency in certain areas which are necessary for the stability of the body, or he may search for minor changes in programmed activities which reduce organizational tensions without seriously endangering the achievement of goals. But as Diesing readily admits, even these alternatives do not provide a true integration of maximizing and integrative decision-making styles. In each case, one factor is held constant while the other is varied within relatively limited boundaries.[15]

Chart 1. Conflict Intensity and Decision-Making Behavior

Level of Conflict Intensity	Pattern of Decision-Making Behavior
ANALYTIC CONFLICT	*Maximizing (Technical) Decision Behavior:* Research and persuasion *Key Actors:* Specialists and technicians
ORGANIZATIONAL CONFLICT	*Mixed Maximizing and Integrative Decision Behavior:* Mixed analytic and bargaining techniques Possible politicization of the conflict *Key Actors:* Institutional interest groups
SYSTEMIC POLITICAL CONFLICT	*Integrative (Political) Decision Behavior:* Informal high-level bargaining Voting in Politburo, Central Committee Coalition formation *Key Actors:* Leadership factions and interest groups with political resources

Conflict Intensity and Decision-Making Behavior

Applied to Soviet politics, the distinction between the maximizing and integrative functions of a decision permits us to develop a partial model of decision-making behavior which reveals a relationship between the level of conflict intensity and the dominant patterns of rational and nonrational decision making. Relying initially on Diesing's threefold description of maximizing, integrative, and mixed decisions, we may outline three levels of conflict intensity: (1) analytic conflict, (2) organizational conflict, and (3) systemic political conflict. (See Chart 1.)

Analytic conflict exists when disputes arise over the instrumental means to be used in reaching a commonly accepted and unambiguous goal. Like the maximizing decisions described by Diesing, these involve technical questions of choosing the most efficient allocation of scarce resources. Underlying such disputes is the commonly held assumption that a correct solution can be found if all necessary information can be brought to bear on the problem. The fundamental interests of the participants in the decision are not threatened, permitting them to confront the problem-solving situation instrumentally.

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[11] Paul Diesing, "Socioeconomic Decisions," in *The Making of Decisions*, ed. William J. Gore and J. W. Dyson (New York: Free Press, 1964), pp. 64–84.

[12] *Ibid.*, pp. 69–70.

[13] *Ibid.*, pp. 70–71.

[14] *Ibid.*, pp. 75–76.

[15] *Ibid.*, pp. 77–78.

In such a setting, the primary forms of decision-making activity are research and persuasion, the former motivated by the hope that new information may clarify the problem and the latter by the attempts of decision makers to convince others that their proposals most efficiently attain the desired goals. All decision makers in such analytic conflicts share the same frame of reference, i.e., the same set of rational-technical criteria; thus a "correct" solution is easily recognized by all.

In the Soviet context, analytic conflict usually occurs within a bureaucratic or institutional environment and is most often associated with the resolution of nonpolitically sensitive technical questions. It is at this level that technically competent specialists are most active and influential in the decision-making process, for their relevant expertise and instrumental frame of reference best equip them to deal with such conflicts.[16] Analytic conflicts may also occur between separate institutions to the extent that the decision makers in both share a common frame of reference, reducing the likelihood that the conflict will spill over to endanger the organizations' self-images and perceived roles.[17]

Organizational conflict occurs when disputes over policy choices are perceived by those involved to affect the social role or relative power of the organizations participating in the decision-making process.[18] Such conflict may impinge on institutions in two ways. First, the decision may involve the allocation of scarce resources or jurisdictional disputes over the scope of authority allotted to competing organizations. Second, it may affect the organization's self-image and sense of mission, forcing decision makers to seek an acceptable redefinition of their "fit" in society.

Underpinning such limited organizational conflict is the commonly held assumption that a compromise solution is possible which will meet both the goal-oriented and equilibrium-oriented needs of all participants. But unlike analytic conflicts, no common instrumental frame of reference exists, aside from an overall commitment to preserving the stability of the system. Agreement is limited to a common presumption that all participants will bargain in good faith, seeking an acceptable compromise rather than the complete destruction of their opponents.[19] Typical of such conflicts are the annual battles for budgetary allocations and the attempts of competing agencies to acquire (or to defend) a "piece of the action."

Decision-making behavior in limited organizational conflicts is marked by a mixture of analytic and bargaining techniques in which decision makers attempt to confront both the problem-solving task before them and to deal with the intra- and inter-organizational conflicts which arise. Such decisions are the most difficult, for the decision makers must deal simultaneously with both rational (problem-solving) and nonrational (political and integrative) elements of the decision. Moreover, the primary adaptive strategy in such cases—the acceptance of planned waste and inefficiency—is made difficult in the USSR by continuing scarcities.

Given the problems associated with a combined approach, decision makers frequently turn to alternative modes of behavior. On the one hand, they may simply seek to avoid the problem by appealing to higher authorities to decide the question. On the other, they may attempt to redefine the conflict or to alter its scope. In the former case, conflict then centers on competing definitions of the problem or disputes over the suitability of competing frames of reference. In the latter, an attempt is made either to restrict or to enlarge the circle of actors participating in the decision in the hope of shifting the power balance. Such changes in the scope of conflict may occur either laterally, leading to the involvement of other groups and organizations at the same level as the original participants, or vertically, leading to the politicization of the dispute.[20]

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[16] Kelley, pp. 860–876.

[17] In the long run, however, even maximizing decisions may have more far-reaching social and political consequences; what makes them "maximizing" rather than "integrative" or "political" is the way in which they are perceived by the decision makers.

[18] As we have noted, organizational conflict does not immediately involve the interests of competing leadership factions, although they may ultimately be touched by these decisions. A good example of this relationship is to be found in the 1958 school reform decision, for while the decision to shift to vocationalized training had little immediate impact on any leadership faction, it would ultimately have exercised profound influence on the elite itself.

[19] The decision-making style of each participant closely resembles what March and Simon have termed "satisficing," i.e., the search for a solution which satisfies the minimal requirements of each of the participants; see James G. March and Herbert A. Simon, *Organizations* (New York: John Wiley & Sons, 1958), pp. 140–141.

A wide range of actors becomes involved in the resolution of organizational conflicts. The technical specialists, who dominate decision making on analytic conflicts, are joined by the more politicized administrators and bureaucrats who head the organizations involved in the dispute. While these administrators may themselves have technical training similar to that of the experts, they nonetheless operate at a higher organizational level and therefore work within a politicized bureaucratic framework. To them falls the complex task of balancing the instrumental and integrative aspects of the decision, for it is they who have presumably learned the skills of bargaining and bureaucratic politics so necessary to this end. Precisely for this reason, institutional interest groups—that is, managers, state bureaucrats, etc.—have a great impact on policy at this level of conflict intensity, for they combine both the relevant expertise and political-bureaucratic skills needed to survive intense organizational conflicts.

Organizational conflict is especially frequent in the Soviet system for several reasons. First, the state's role in economic and social life places decisions on a wide range of questions within a centralized and competitive bureaucratic environment; in less centralized systems, such decisions would be more widely distributed throughout the society. Second, the continued scarcity of resources intensifies the struggle over allocations and makes it more difficult to buy concessions through organizational duplication and planned waste (although these do occur in some cases).

Systemic political conflict occurs when disputes involve the fate of top leaders and their supporters.[21] While such conflicts are seemingly about more practical questions of policy or personnel assignments, there is a clearly perceived relationship between the available options and the relative power positions and perhaps the ultimate fate of leadership factions. In their most extreme form, such conflicts may involve the immediate removal of key elite members, as in the attempted coup in June, 1957, and its successful restaging in October, 1964.

The distinction between *systemic political conflict* and *organizational conflict* is blurred to the extent that alliances are formed between institutional interest groups and leadership factions, and the more permanent or rooted such alliances, the more likely that organizational conflicts will rapidly grow into more fundamental political rivalries and vice versa.[22]

Underlying systemic political conflict is the assumption that the stakes of the game involve the relative power positions and ultimately the political survival of competing leaders. While defeat has come to mean little more than demotion or retirement in recent years, it is still clear that top political figures devote great energy to anticipating and coping with potential clashes. Adding to the sensitivity of systemic political conflict is the relative loss of power usually suffered by the lower-echelon allies of a defeated leader. Thus the stakes are especially high for organizations and groups that become closely associated with an individual or faction, especially if they have played a primary role in his coalition.

Several forms of decision-making activity are associated with the resolution of systemic political conflicts. The most important is the process of informal high-level bargaining between individual leaders or coalition spokesmen. Should such bargaining fail, the question is usually decided by a vote within one of the higher Party bodies. While the Politburo usually performs this function, more important conflicts may be shifted to the Central Committee. The infrequency with which disputes actually reach the Committee may be attributed to the success that the initial process of informal bargaining usually enjoys; competing leaders are understandably reluctant to test their uncertain coalitions in intense conflict unless the stakes are high. Of course, a deadlock may develop in which no decision is reached. From the perspective of competing leaders, such a nondecision simply means that no faction was willing to test its coalition on that particular question.

Since Stalin's death, decision making within the Politburo has placed greater stress on consensus building and on reducing direct conflict over most policy questions. Formal votes are apparently taken on only a small number of issues when consensus cannot be reached. In those cases when voting might upset the delicate political balance, issues are frequently postponed (or perhaps never brought to the level of Politburo deliberation because of their recognized disruptive potential), shifted downward to the staff level, or sent back to the agencies most directly involved in the hope that a decision can be reached at the secondary level. According to Brezhnev, unresolved disputes are now frequently handed over to *ad hoc* committees within the Politburo itself for decision, although the General Secretary was tantalizingly vague on the composition of these groupings or the method of their selection.[23] These arrangements can reasonably be understood as an institutionalized buffer purposely designed to reduce conflict at the top level. This is not to argue, of course, that such conflict does not occur; in any system characterized by complex patron-client ties between top leaders and politicized bureaucracies and uncertain tenure in office for individual leaders, such conflict is inevitable, especially when seemingly routine policy or personnel decisions may affect the stability of delicately balanced coalitions. Great care, however, is apparently taken to control *which* issues escalate from the level of organizational conflict to systemic political conflict. Competing leaders seek to introduce issues resulting in favorable coalition alignments during periods of intense factional conflict and to restrict the ability of their secondary coalition partners or potential opponents to force disruptive issues in periods of relative stability.

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[20] This concept has long been applied to the study of American political behavior. See E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960).

[21] This level of conflict intensity is similar to that described by Myron Rush during succession periods. Unlike Rush, however, I maintain that such conflict may occur at any point in time, inasmuch as leaders since Stalin have seemed unable to consolidate their power completely and thus have been constantly compelled to try to keep their coalitions together or to prevent the emergence of a hostile coalition. What then separates systemic political conflict from organizational conflict is the perception by one or another leadership faction that a particular decision will affect its political fortunes. See Myron Rush, *Political Succession in the USSR* (New York: Columbia University Press, 1965).

[22] It is obvious that competing leaders must regard *time* as an important variable in estimating the political implications of organizational conflict.

Two other forms of behavior are associated with the tactical aspects of systemic political conflict. The most important is coalition formation, that is, the search for allies with politically useful resources.[24] To the extent that this process requires alliances with non-Party groups, it results in subtle bargaining between the leader and group on policy questions. From the point of view of competing leaders, this strategy implies the search for support among the most politically powerful groups and not necessarily those with the most relevant knowledge and expertise to deal with the instrumental aspects of a policy question. Thus the technical specialists and specialized interest groups, so important in both analytic and organizational conflicts, carry far less weight in the solution of systemic political conflicts, unless, of course, they possess additional political resources as well.[25]

Also characteristic of systemic political conflict are attempts to control the selection and definition of issues and to frame the proposed response. It is virtually axiomatic that the faction that selects and defines the issue and sets the tone of the debate has acquired important leverage over the eventual decision. Thus it is not accidental that Khrushchev chose in 1957 to define the problems of a sagging economy in terms of overcentralization and to recommend the creation of regional economic councils, for he undoubtedly realized that this definition of the problem would both undercut the ministerial base of support of his opponents and draw the regional Party secretaries to his side.

Future Research

In addition to contributing to the debate on the rationalization of decision making, this threefold model also suggests a number of areas for future research. A comparison of the activities and relative influence of various groups in analytic, organizational, and systemic political conflicts comes immediately to mind. For example, this author has recently examined the impact of technically specialized interest groups under different levels of conflict intensity and found that such groups exercise far greater influence on less sensitive technical and organizational questions (i.e., in analytic and organizational conflicts) than on the resolution of more intense political disputes. Future research could well focus on the comparative effectiveness of the politically relevant resources of various groups (e.g., representation on Party and state decision-making bodies, expertise, prestige, formal organization, access to the media, etc.) and upon the political skills and lobbying styles of their leaders. Such a systematic approach would undoubtedly contribute to a more comprehensive understanding of interest group activity and decision making in the Soviet Union.

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[23] Theodore Shabad, "Brezhnev, Who Ought to Know, Explains Politburo," *New York Times*, June 15, 1973, p. 3. Informal subject-matter committees were formed within the Politburo even in Stalin's day, and a special Bureau of the Presidium was formed after that body was enlarged at the 19th Party Congress. In the latter case, formal membership in the Bureau was never revealed; apparently the identity of those involved in the Bureau's work shifted from time to time in response to the political fortunes of Stalin's lieutenants. See Robert Conquest, *Power and Policy in the USSR* (New York: Harper, 1967), pp. 159–163.

[24] In the Soviet case, this has usually come to mean seeking the support of groups with representation on the Central Committee.

[25] Kelley, pp. 876–888.

Provision of Collective Goods As a Function of Group Size*

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Introduction

Interest in the theory of collective goods has been increasing among political scientists since the publication of Mancur Olson's *The Logic of Collective Action.*[1] Drawing on developments in the theory of public goods by Samuelson and others,[2] Olson shows how this theory can be used to analyze the formation of voluntary associations including, among others, such important political groups as international alliances and special interest groups. Among Olson's major conclusions are the following:

(1) "unless the number of individuals in a group is quite small, or unless there is coercion or some other special device to make individuals act in their common interest, *rational, self-interested individuals will not act to achieve their common or group interests.*"[3]

(2) "The larger a group is, the farther it will fall short of providing an optimal supply of any collective good, and the less likely that it will act to obtain even a minimal amount of such a good. In short, the larger the group, the less it will further its common interests."[4]

Two separate questions are addressed in the conclusions cited above. The first question concerns the amounts of a collective good that will actually be provided by groups whose only difference is in size. The second question is a relative one, involving a comparison between the amount of the collective good actually provided and that amount which would be provided if the optimality conditions of the economic theory were satisfied. The conclusions cited above assert that everything else being equal, small groups will fare better than large groups in both actual and relative performance in providing collective goods. The analysis that follows is concerned with the first of these questions—the relationship between group size and the amount of the collective good actually provided by the group. The closest that Olson comes to specifying the form of this relationship is the following paragraph:

> In a small group in which a member gets such a large fraction of the total benefit that he would be better off if he paid the entire cost himself, rather than go without the good, there is some presumption that the collective good will be provided. In a group in which no one member got such a large benefit from the collective good that he had an interest in providing it even if he had to pay all of the cost, but in which the individual was still so important in terms of the whole group that his contribution or lack of contribution to the group objective had a noticeable effect on the costs or benefits of others in the group, the result is indeterminate. By contrast, in a large group in which no single individual's contribution makes a perceptible difference to the group as a whole, or the burden or benefit of any single member of the group, it is certain that a collective good will *not* be provided unless there is coercion or some outside inducements that will lead the members of the large group to act in their common interest.[5]

While this paragraph is not explicit about the form of the relationship in question, it seems not unreasonable to infer from it and the second of Olson's conclusions cited above that he considers the relationship to be a decreasing one. It remains open to question whether the relationship is a gradually decreasing one, or a step function, with the step (down) occurring at the point where any one individual's actions become imperceptible to others. It is shown below that the relationship between group size and the provision of a collective good is more complex than Olson asserts and that, in many cases, it is the opposite of that suggested by Olson.

The next section discusses collective goods and the framework in which group activity will be analyzed. The paper then analyzes the relationship between group size and the provision of collective benefits. The last section discusses the results, compares them with those of Olson, and comments on some additional factors which influence a group's ability to provide its members with collective benefits.

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* The author wishes to thank Phillip Gregg and Mancur Olson for many helpful comments on earlier drafts of this paper. Research support was provided by the Institute of Public Policy Studies, The University of Michigan.

[1] Mancur Olson, Jr., *The Logic of Collective Action: Public Goods and the Theory of Groups* (Cambridge, Mass.: Harvard University Press, 1965). A somewhat different and more general formulation of Olson's analysis subsequently appeared in an article co-authored by Olson and Richard Zeckhauser, "An Economic Theory of Alliances," *Review of Economics and Statistics,* 48 (November, 1966), 266–279.

[2] See Paul Samuelson, "The Pure Theory of Public Expenditure," *Review of Economics and Statistics,* 36 (November, 1954), 387–389; "Diagrammatic Exposition of a Theory of Public Expenditure," *Review of Economics and Statistics,* 37 (November, 1955), 350–356; and John Head, "Public Goods and Public Policy," *Public Finance,* 17 (November 3, 1962), 197–219.

[3] Olson, p. 2.

[4] *Ibid.*, p. 36.

[5] *Ibid.*, p. 44.

Derivation of the Model

A collective good is generally characterized by one or both of the following two properties:[6]

(1) Consumption of the good by one individual does not subtract from others' consumption of the good. This property is often referred to as nonrivalness of consumption.[7]

(2) Individuals who do not share in paying for the good cannot be excluded from enjoying the benefits of the good. The benefits accrue automatically to all individuals. This is referred to as the nonexclusion property.

These two properties result in individuals having incentives to misrepresent their preferences for collective goods and to engage in strategic behavior to avoid paying for the benefits of these goods. The ensuing problems make the provision of an optimal amount of a collective good via a market or the political process extremely unlikely. Of primary interest in this analysis are those goods for which the nonexclusion property holds. The activities of interest groups clearly fulfill this condition, for if political action is undertaken by others and an individual benefits from government policy as a result of this action, the individual cannot be excluded from these benefits. The nonrivalness property plays an important part in the analysis as well. Using this property, Olson classifies groups as "inclusive" or "exclusive" according to the type of collective benefits their members receive.[8] These categories correspond to the endpoints of the continuum measuring the degree to which the nonrivalness of consumption property is met. A pure collective good, which exhibits perfect nonrivalness, is an "inclusive" good, and the group which receives the benefits of the good may also be labeled "inclusive." An individual's evaluation of an "inclusive" collective good is not affected by the number of persons in the group who receive the benefits. On the other hand, an "exclusive" good exhibits the same rivalness of consumption as does a private good, but the nonexclusion property still holds. In this case, the benefit any single individual receives from a unit of the good decreases as the size of the group increases. An instance of such a good is Olson's example of a price increase brought about in an industry through output restriction. The benefits of this price increase (increased profits) are available to any producer (or potential producer) who supplies some of the demand for the industry's product. The total amount of increased profit is limited, however, and the consumption of these profits is subject to perfect rivalness. In contrast, an "inclusive" good, such as a decrease in the corporate income tax, is not subject to rivalness at all. An unlimited number of firms can receive the benefits of the lower tax without detracting from the benefits received by the other firms.

In arriving at the conclusions cited earlier, Olson assumes that there is no cooperation among the parties involved, that is, no cost sharing arrangements are permitted, and each individual must bear the entire cost of any increase in the amount of the collective good which he initiates. The amount of the collective good provided is thus determined through a process in which the individual reacts independently to the behavior of others in deciding how much of the collective good to provide by himself. The noncooperative nature of this process is also crucial to Olsen's "exploitation" hypothesis, for this hypothesis is based upon an examination of the properties of the equilibrium of this noncooperative process.[9] This process may be illustrated as fol-

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[6] See Head, "Public Goods and Public Policy," for a full discussion of the characteristics of collective goods.

[7] Samuelson makes the following distinction between the polar cases of private and pure collective goods. For a private good,

$$\sum_{i=1}^{n} x^i = X,$$

while for a pure collective good $x^1 = x^2 = \ldots = x^i = \ldots = x^n = X$, where x^i is the consumption of individual i, and X is the amount of the good produced. See Samuelson, "The Pure Theory of Public Expenditure," p. 387.

[8] Olson, pp. 36–42.

[9] It is important that the nature of this process be understood. In particular, it is important to note how the "imperceptibility" of an individual's actions is treated. The process is one of dynamic adjustment, in which each individual reacts to the behavior of others on the assumption that they will not perceive his action (more correctly, will not change their behavior as a result of his action), even though in reality this is not the case. The model is equivalent to a Cournot duopoly model. See Kalman Cohen and Richard Cyert, *Theory of the Firm: Resource Allocation in a Market Economy* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1965), chapter 12, for one discussion of this model. It is important to distinguish this treatment of "imperceptible" actions from the cases in which the effects of an individual's actions are either imperceptible to himself because of uncertainty, lack of information, etc., or are imperceptible to others for the same reasons. The analysis in Olson's book and in the article by Olson and Zeckhauser assumes that each individual has perfect knowledge of the (past) actions of all individuals and of the production function for the good in question. For an examination of the problem of an actor's behavior under uncertainty, see Norman Frolich and Joe Oppenheimer, "I Get By With a Little Help From My Friends," *World Politics,* 23 (October, 1970), 104–120. For a discussion of Olson's "exploitation" hypothesis (that the small exploit the great), see Olson, p. 35.

lows. Consider an individual faced with a choice of allocating his income to purchases of two goods, one a private good (y) and the other a collective good (x). His preferences for these good are shown in Figure 1 by a set of indifference curves. His budget constraint is given by the line AB, and he maximizes his satisfaction by purchasing a commodity bundle on the highest indifference curve consistent with the budget constraint. In Figure 1, his optimum allocation is at point E (with amounts x^0 and y^0 of the two goods), the point of tangency between indifference curve I_2 and the budget line AB. Suppose another individual provides an amount $\bar{x}$ of the collective good. Because of the non-exclusion property, this amount of the collective good is automatically available to the first individual. This has the effect of shifting the vertical axis in Figure 1 to the right by the amount $\bar{x}$. The individual's budget line is also shifted to the right by the amount $\bar{x}$, since he now consumes this amount of the collective good without having to pay anything for it. If the private good has a price of one, this action by the other individual has the effect of increasing the first individual's income by an amount AD, and he is free to spend his income as he chooses so long as he consumes at least an amount $\bar{x}$ of the collective good. The individual now finds his equilibrium at $\bar{E}$, and he pays for an amount $(x^1-\bar{x})$ of the collective good. If all possible amounts of the collective good provided by others were considered, the locus of the equilibria of the individual would be the line EE', known as the "income-consumption". curve. Note that if others provide an amount x^s of the collective good, the individual will devote his entire budget to the purchase of the private good. The locus of equilibria is shown in a slightly different manner in Figure 2, where the vertical axis now indicates the amount of the collective good provided by other individuals and the horizontal axis measures the total amount of the collective good provided. A reaction curve for the individual can be derived by considering the horizontal difference between the 45° line and the locus of equilibria in Figure 2. The reaction curve is shown below in Figure 3, indicating that the individual will provide an amount x^0 of the good if no one else provides any, and that he will provide none if the amount provided by others exceeds an amount x^s. The form of the reaction curve depends upon the individual's income elasticity of demand for the collective good. Goods may be placed in three classes on the basis of their income elasticities—inferior goods, normal goods, and superior goods.[10] In the case of an inferior good, an extra unit of the collective good provided by others induces the individual to cut back his own provision of the good by at least one unit. For a normal good, an extra unit provided by others induces a reduction in the provision by the individual of less than one unit, and for a superior good, an extra unit provided by others induces no reduction, and perhaps an increase, in the amount provided by the individual. These categories of goods have corresponding reaction curves with the following slopes:

type of good	*slope of reaction curve*
inferior	negative and ≥ -1
normal	< -1, but finite
superior	infinite or positive

Three representative reaction curves are shown in

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[10] For a discussion of this classification, see Cohen and Cyert, pp. 75–76 as well as Olson and Zeckhauser, p. 270, footnote 15.

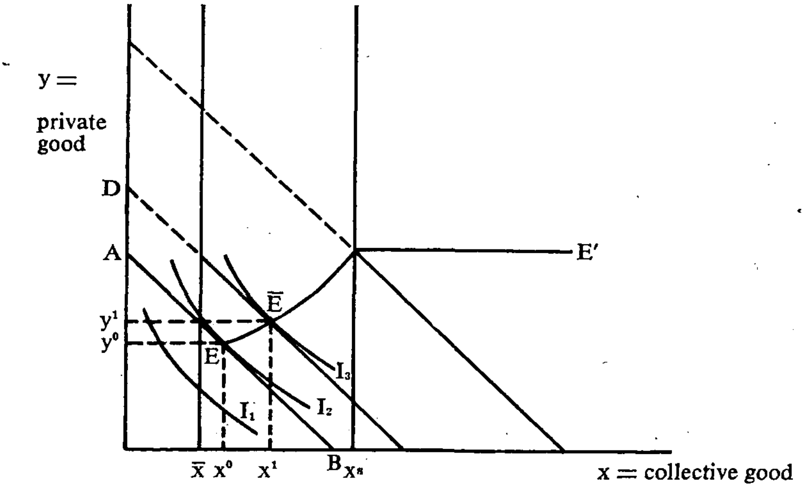

Figure 1. Income consumption curve

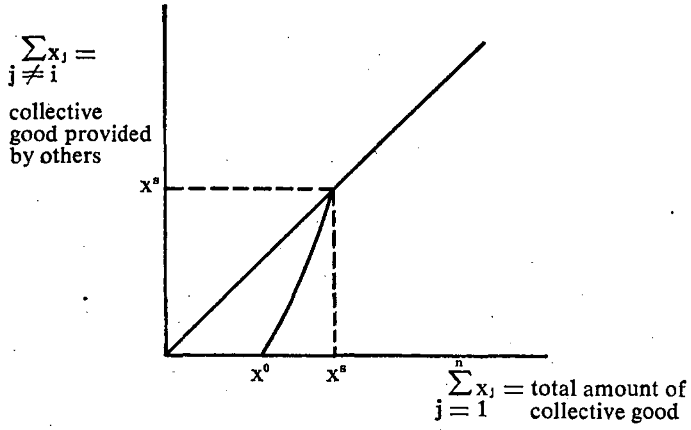
$\sum_{j \neq i} x_j =$
collective good provided by others
x^s
x^0
x^s
$\sum_{j=1}^{n} x_j =$ total amount of collective good

Figure 2. Locus of equilibria

Figure 4.[11] It is to be expected that most collective goods will have normal income elasticities.[12]

The case in which two such individuals are independently reacting to one another is shown in Figure 5. The independent adjustment process will reach an equilibrium *E* where the reaction curves intersect.[13] The equilibrium results in *A* purchasing x_A^E of the good and *B* purchasing x_B^E of the good. Both individuals consume an amount of

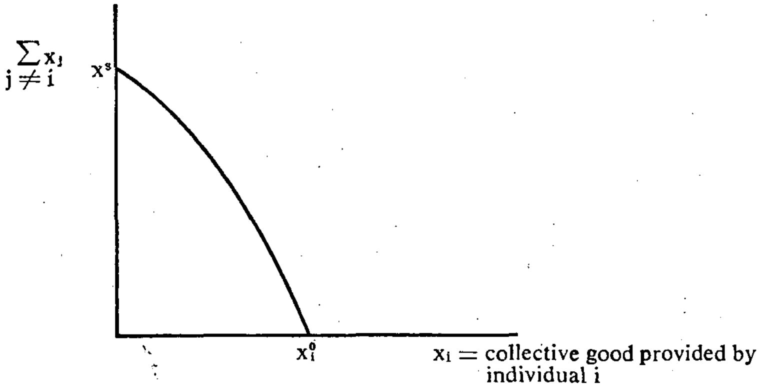

Figure 3. Reaction curve

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[11] In Figure 4, the curves are drawn in such a way that it appears that a good is always inferior (or normal, or superior). This is generally not the case. The good may fall in different classes for different individuals, and as the level of consumption of the good rises for an individual, the good may move from one class to another.

[12] In *The Logic of Collective Action,* Olson ignores income effects, considering only the case in which the slope of the reaction curve is -1 (the case of no income effects). Olson and Zeckhauser consider income effects more fully, particularly on p. 270, footnote 15. If collective good is a superior good in the framework under consideration, groups will be led either to invest all of their resources in the provision of the good (if the slope of the reaction curve is positive) or to provide an amount of the good in direct proportion to group size (if the slope of the reaction curve is infinite). Since such behavior is highly unlikely, superior goods will be ignored in the analysis to follow.

[13] Under the assumptions of the model, an individual will change his behavior if the point (x_A, x_B) corresponding to the amounts of the collective good provided by the individuals does not lie on the individual's reaction curve. Since the intersection of the two reaction curves is the only point at which neither individual will change his behavior, this is the only possible equilibrium. If both reaction curves are continuous, an equilibrium will exist. Sufficient assumptions for a continuous reaction curve are that the individual have a continuous utility function and that the indifference curves derived from this function obey the condition of strong convexity (i.e., if two possible allocations are indifferent, then their weighted average with positive weights is preferred to them). The particular configuration shown in Figure 5 is not guaranteed, but is the most likely case. Other possibilities are shown and discussed in the Appendix. It should be noted that this equilibrium also results if the problem is viewed as a noncooperative game rather than as a Cournot duopoly problem.

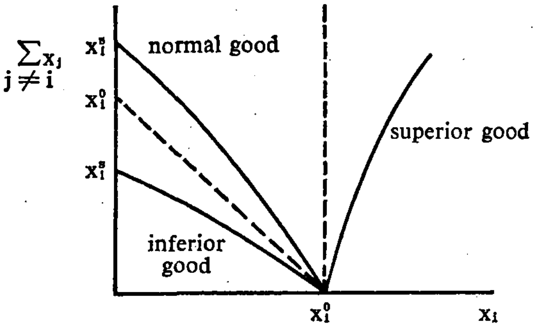

Figure 4. Types of reaction curves

the collective good equal to the sum of these two purchases.

Analysis

Attention may now be turned to examining the relationship between group size and the amount of a collective good provided by the uncoordinated activities of the group members. For any group, this amount is found by determining the equilibrium of the process described above. One further assumption will be made at this point. In order to facilitate the analysis, it will be assumed that all individuals in a group are identical in terms of preferences and resources.[14]

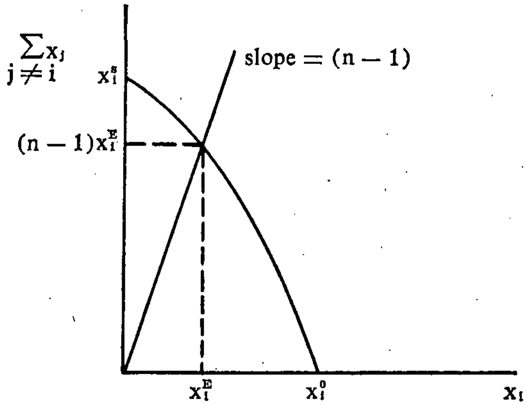

Figure 6. Equilibrium for an inclusive good

Consider first the case of an inclusive collective good. The reaction curve for the *i*th individual is shown in Figure 6. Since the good is "inclusive," the reaction curve is unaffected by group size and thus this one curve will serve as a basis to analyze groups of different sizes. The assumption that all individuals are identical makes solution for the equilibrium quite easy, for if the individuals are identical, each will provide the same amount of the collective good at the equilibrium. In Figure 6, if x_i^E is the amount of the good provided by the *i*th individual at the equilibrium for a group of size n, then the amount provided by others must be $(n-1)x_i^E$. Thus the equilibrium for a group of size n is found where a straight line through the origin with slope $(n-1)$ intersects the reaction curve. As the size of the group increases, the equilibrium moves along the reaction curve from x_i^0 (when $n=1$) to x_i^s (as $n \to \infty$), as shown in Figure 7. Two characteristics of the provision of an "inclusive" collective good are evident at this point:

(a) $x_i^E \to 0$ as $n \to \infty$
(b) nx_i^E (the total amount provided)$\to x_i^s$ as $n \to \infty$

The relationship between nx_i^E and n may now be established. The total amount of the good provided is equal to the sum of the horizontal and vertical components of the equilibrium $(nx_i^E = x_i^E + (n-1)x_i^E)$. If the reaction curve is stated as a function of x_i, say $f(x_i)$, then $nx_i^E = x_i^E + f(x_i^E)$. The relationship between nx_i^E and n is established by examining the derivative of $(x_i^E + f(x_i^E))$ with respect to x_i^E,

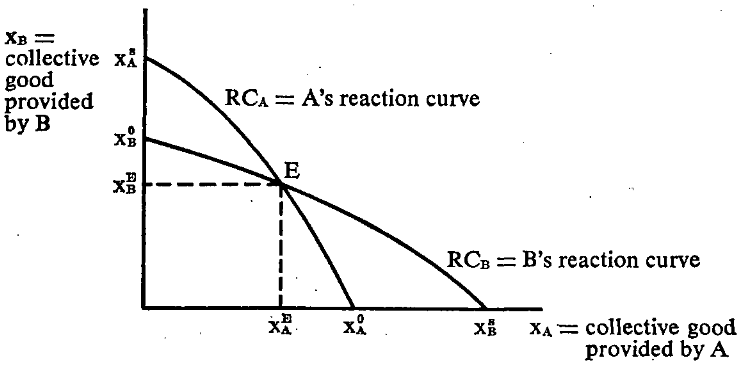

Figure 5. Equilibrium with two individuals

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[14] This assumption greatly simplifies the analysis to follow without affecting the substance of the results.

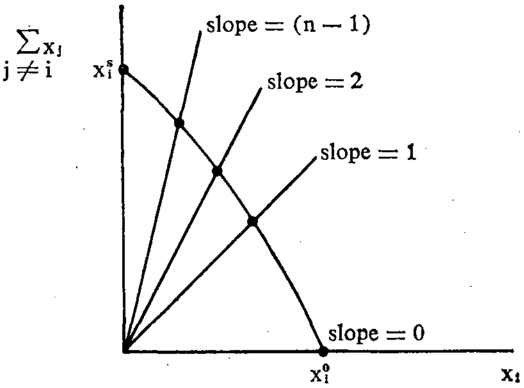

Figure 7. Locus of equilibria for an inclusive good

$$\frac{d}{dx_i^E}(x_i^E + f(x_i^E)) = 1 + f'(x_i^E),$$

where $f'(x_i^E)$ is the slope of the reaction curve at $x_i = x_i^E$. If $f'(x_i^E) > -1$, nx_i^E increases as x_i^E increases, meaning that nx_i^E decreases as n increases. If $f'(x_i^E) = -1$, then nx_i^E is unaffected by group size, and if $f'(x_i^E) < -1$, nx_i^E increases as n increases. This result indicates that if the collective good is not an inferior good (i.e., the slope of the reaction curve is less than -1), then the amount of the good actually provided by a group increases with group size (approaching x_i^s as n becomes infinite). Note that while nx_i^E is increasing with n, x_i^E is decreasing. The decrease in the contribution of each individual (x_i^E) is more than offset by the increase in group size. Thus, in the case of "inclusive" collective goods which are not inferior goods, the relationship between group size and the amount of the good actually provided is the opposite of that asserted by Olson.

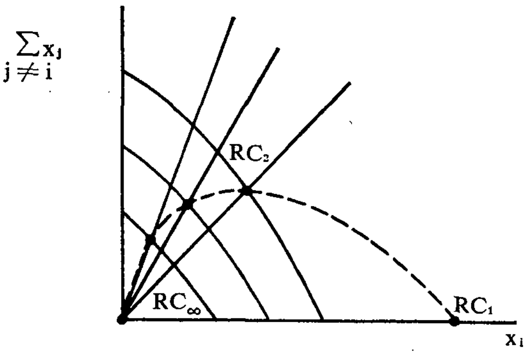

Figure 9. Locus of equilibria for an exclusive good

The case of an "exclusive" collective good may be analyzed in the same way, with one exception. If the good is "exclusive," the reaction curve for an individual changes as group size changes. A possible set of reaction curves for an individual are shown in Figure 8, where RC_n is the individual's reaction curve for a group of size n. This set of curves indicates that as group size becomes infinite, the reaction curves converge to the origin. This is an extreme case of an "exclusive" good, and it is possible that the reaction curves, although shifting toward the origin, will not converge to the origin. This case will be considered later. The equilibrium for a group of size n is found in this case where a straight line through the origin with slope $(n-1)$ intersects the reaction curve for that size group. The path of these equilibria is shown by the broken line in Figure 9. From the figure it is clear that, for an "exclusive" collective good:

(a) $x_i^E \rightarrow 0$ as $n \rightarrow \infty$
(b) $nx_i^E \rightarrow 0$ as $n \rightarrow \infty$
(c) nx_i^E decreases as $n \rightarrow \infty$.

These conclusions correspond to those of Olson.

For a good in the interior of the "inclusive-exclusive" continuum, the reaction curves bear some similarity to those for an "exclusive" collective good. For these "intermediate" goods the

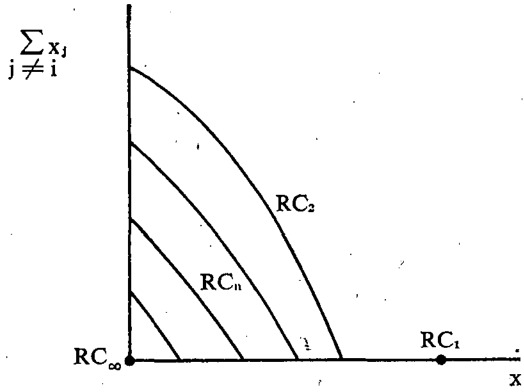

Figure 8. Reaction curves for an exclusive good

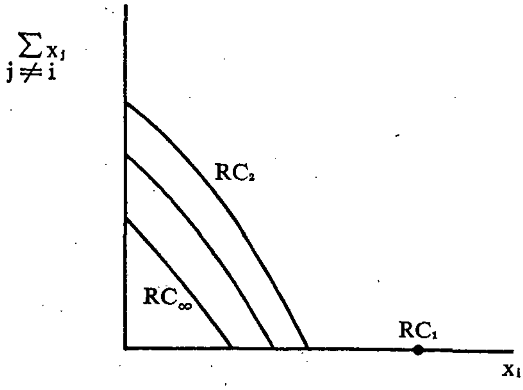

Figure 10. Reaction curves for an intermediate good

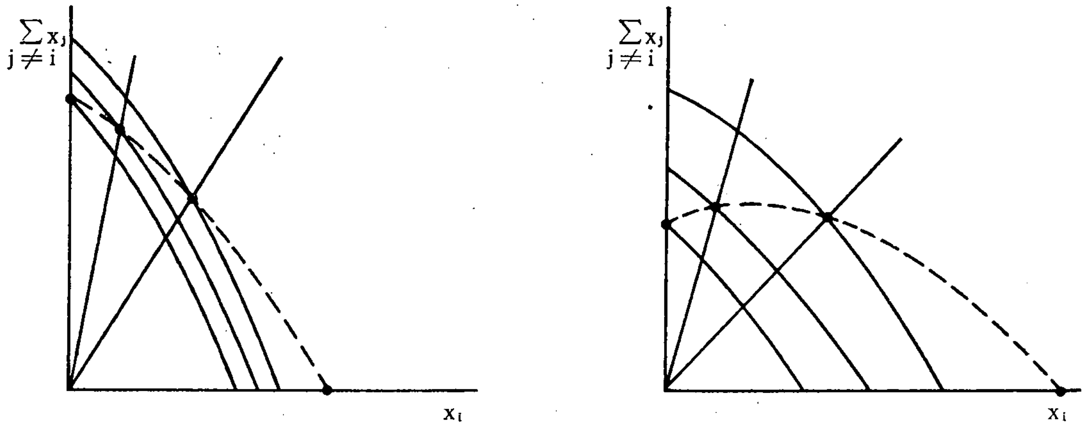

Figure 11. Loci of equilibria for two intermediate goods

reaction curve shifts toward the origin as group size increases, but the set of curves converges to some curve above the origin, an indication of the fact that, even as the group becomes infinitely large, the nonrivalness property still holds to a certain degree. Such a set of curves is shown in Figure 10. The path of equilibria is found in the same manner as that of an "exclusive" good. Two possible cases arise, however, as shown in Figure 11. In both cases:

(a) $x_i^E \rightarrow 0$ as $n \rightarrow \infty$
(b) $nx_i^E \rightarrow x_i^s > 0$ as $n \rightarrow \infty$.

But nx_i^E may increase *or* decrease as n increases, as indicated by Figures 11a and 11b respectively. In the first case, nx_i^E increases with group size, indicating that the income effect outweighs the crowding effect, while in the second case the opposite is true. Based on this finding, it would seem that any collective good can be placed into one of two broad categories, "inclusive" and "exclusive," depending upon whether the amount of the good actually provided (nx_i^E) increases or decreases as group size increases.

Discussion

Olson recognizes the possibility of members of a large group providing some amount of a collective good, but the tone of the following remarks indicate that he considers this possibility of little general interest or of little empirical significance or both:

> There is one logically conceivable, but surely empirically trivial, case in which a large group could be provided with a very small amount of a collective good without coercion or outside incentives.[15]

> Moreover, apart from a special case . . . , it can be demonstrated that even the provision of a minimal amount of a collective good is less likely in large groups than in small ones when behavior is rational and self-interested.[16]

Nothing in the above analysis would seem to support Olson's statement that the result obtained for "inclusive" goods applies only to a "special case." Only three major assumptions were necessary for the analsysis, and none of these is, in theory, less valid for large groups than small groups. The behavioral assumption that an individual will act as if his actions will not affect the actions of others is, if anything, more likely to be valid for large groups than for small groups. The assumptions implicit in the use of a budget line—i.e., that there exist no fixed costs in the provision of the collective good and that the price faced by an individual is unaffected by the actions of others—affect all groups similarly, since they present the same barrier to action to all individuals.[17] Finally, the assumption that each individual has perfect information about the actions of others and the effects of his own actions applies equally, in theory, to all groups regardless of size. In reality, these information conditions no doubt represent a greater obstacle to large groups than to small groups, but differences in group performance reflecting this fact cannot be derived from a model based on certainty, such as Olson's or the one provided in the previous section. Olson's lack of clarity regarding the relationship

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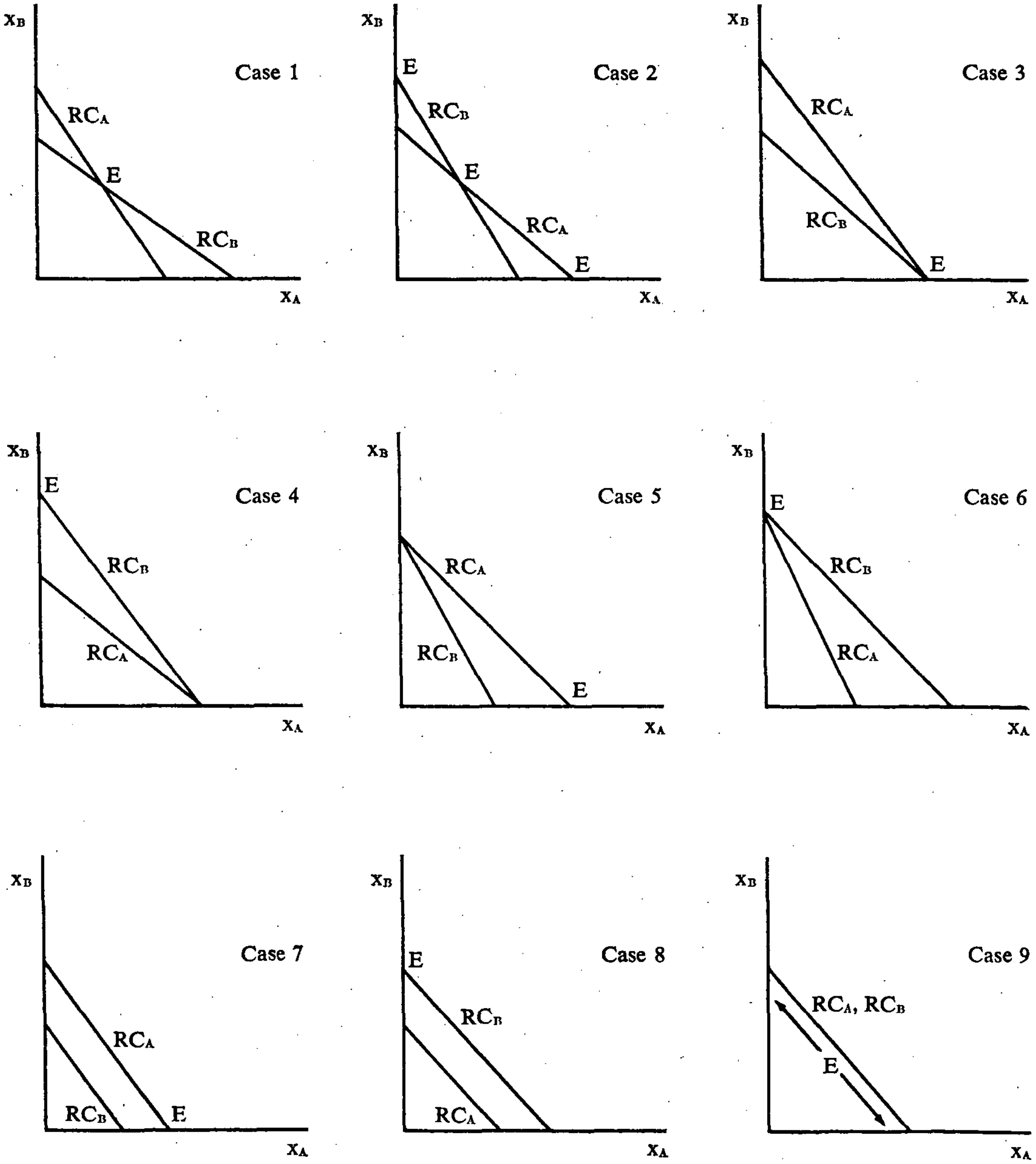
[15] Olson, p. 48, footnote 68.

[16] Mancur Olson, "Increasing the Incentives for International Cooperation," *International Organization,* 25 (Autumn, 1971), 866–874, at p. 866, footnote 1.

[17] Olson, (*The Logic of Collective Action* [p. 22]) points out the importance of fixed costs as a barrier to collective action. He suggests that cooperative action may be necessary to overcome this barrier. In making use of a budget line, the present analysis ignores fixed costs. It should be emphasized that the inability of a group to overcome these costs via noncooperative action will not be a function of group size.


x_B
Case 1
RC_A
E
RC_B
x_A
x_B
Case 2
E
RC_B
E
RC_A
E
x_A
x_B
Case 3
RC_A
RC_B
E
x_A
x_B
E
Case 4
RC_B
RC_A
x_A
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Case 5
RC_A
RC_B
E
x_A
x_B
E
Case 6
RC_B
RC_A
x_A
x_B
Case 7
RC_A
RC_B
E
x_A
x_B
E
Case 8
RC_B
RC_A
x_A
x_B
Case 9
RC_A, RC_B
E
x_A

Figure 12. Possible equilibrium configurations

under consideration may stem partly from his use of the concept of "the fraction of the group gain" (benefit) received by the individual.[18] For an "exclusive" good, the fraction of the group benefit received by an individual plays an important part of the individual's decision, and its consideration leads to the inverse relationship between group size and amount of the good provided that was found for an "exclusive" good in the previous section. For an "inclusive" good, however, the fraction of the total benefit received by a single individual is not so important, since there exists no rivalness of consumption. The addition of an individual to a group does not decrease the benefit any individual receives from a unit of the collective good, although it does decrease the fraction of the total benefit the individual receives. This decrease in the fraction of the total benefit results in the enlarged group's providing a more suboptimal amount of the collective good, but not necessarily a lesser absolute amount, as was shown. Olson's failure to differentiate in his formal analysis between "inclusive" and "exclusive" collective goods, and the important difference this distinction makes in the use of the "fraction of total benefit" concept may partially explain his conclusions regarding the possibility of action by large groups.

Another reason for Olson's conclusions may be his emphasis on the degree of suboptimality of group performance rather than on the amount of the collective good actually provided. It is with respect to the former criterion that large groups fare so poorly. Since in Olson's analysis smaller groups appear to fare better under both criteria, the choice of which criterion to use does not appear to be particularly important. In light of the analysis presented above, however, this choice takes on greater importance. With respect to the second criterion, all groups are constrained by a rather low upper bound on the amount of the good actually provided (x_i^s). The fact that this is the amount at which an individual will provide none of the good himself makes the upper bound seem particularly low. But there seems to be no reason to suggest a priori, as Olson seems to, that the actual amount provided by a large group will have little empirical importance, while the amount provided by a small group will be empirically important. Such a conclusion must be the result of a fuller consideration of the appropriateness of the two measures of group performance.[19] The political process may be a sphere in which the amount actually provided is the appropriate criterion, particularly if several groups are involved and are in opposition to one another. The outcome in such a case is likely to be a function of the relative *actual* strengths of the groups' efforts. For one interested in understanding and predicting the outcomes of such conflicts, the actual amount of the collective good provided (in this case, political action, defense, etc.) will be more useful than measures of suboptimal performance. This is particularly true since in such a case the optimal amounts are themselves relative, being a function of the behavior of the opposing group. Most of the work making use of the theory of collective goods has thus far dealt with the actions of a single group. In light of this emphasis, an important next step in applying this theory to the study of political phenomena would be the consideration of the behavior of several groups in competition. Such a consideration may yield a better understanding of the relative powers of competing groups and of the appropriate criteria by which group action should be judged.

Appendix

The configuration of reaction curves shown in Figure 5 is only one of nine possible configurations. Figure 12 shows these configurations, with Case 1 being the one considered in the paper. The equilibria are denoted by the letter *E*. While Case 1 may be considered the most likely case, the other cases deserve some comment. The analysis to follow will consider only linear reaction curves in order to simplify the discussion.

Two important characteristics of a configuration of reaction curves are the uniqueness and the stability of the equilibrium. An equilibrium is stable if a change from the equilibrium sets in motion a pattern of behavior that results in a return to the equilibrium. Inspection of Figure 12 will show that except in Cases 2 and 9 the equilibrium is unique and stable. In Cases 2 and 9, multiple equilibria exist and all the equilibria are unstable. If either of these cases were to occur with any regularity, the usefulness of the theory would be greatly impaired because of the inability to make precise predictions about behavior and because of the inherent instability of behavior in these cases.

The nine configurations can arise from many different situations, each of which is determined by the income effects exhibited by the preferences of the individuals and by the relationship be-

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[18] *Ibid.*, p. 23.

[19] It may well be true, although no empirical evidence is available, that there are differences between the types of collective goods desired by small and large groups, and that these differences are such that an amount x_i^s of one good may be an "adequate" amount for the small group but an amount x_i^s of another good may be "inadequate" for its group, a large one. Such a possibility seems of sufficient interest to merit further thought.

Table 1. Possible Configurations for Various Combinations of Income Effects

		Individual A		
		Normal Good	No Income Effects	Inferior Good
Individual B	Normal Good	Cases 1, 3, 6, 7, 8	Cases 1, 3, 6, 7, 8	Cases 1–9
	No Income Effects	Cases 1, 3, 6, 7, 8	Cases 7, 8, 9	Cases 2, 4, 5, 7, 8
	Inferior Good	Cases 1–9	Cases 2, 4, 5, 7, 8	Cases 2, 4, 5, 7, 8

tween the reaction curves $(x_A^0 >, =, < x_B^s$ and $x_B^0 >, =, < x_A^s)$. These combinations give rise to 51 different situations. Table 1 summarizes briefly the configurations which can arise for different combinations of income effects. It will be noted that Cases 2 and 9 can occur only if at least one of the individuals considers the collective good to be an inferior good. Since it can reasonably be expected that most collective goods will be normal goods, the uniqueness and stability of the equilibrium derived from the theory seem to be assured.

COMMUNICATIONS

TO THE EDITOR:

Since the publication of my article on "Where the Votes Come From: An Analysis of Electoral Coalitions, 1952–1968"[1] I have been asked a number of times what a parallel analysis of the 1972 presidential election would reveal. Few people are in a position readily to generate the required statistics for themselves because an iterative procedure is necessary to correct for overreporting of voting and especially overreporting of voting for the winner. So for those who wish to gloat or mourn over the landslide of Richard Nixon over George McGovern, here are the figures. The figures are presented with the data from the original article so that all six presidential elections since 1952 can be readily compared.[2] The coalitions are described in terms of the three components of a group's contribution to a party, namely the size of the group, its turnout, and its loyalty to the given party. The *contribution* of a group to the electoral coalition of a party is the proportion of all the party's votes that come from members of that group. The *loyalty* of a group to a certain party is simply the proportion of the votes of that group which are cast for that party.

Before looking at the coalitions themselves there are two well known facts about the 1972 election that bear repeating. The first is that it really *was* a landslide, with the Democrats getting only 37.5 per cent of the votes to the Republicans' 60.7 per cent.[3] The second basic fact is that the turnout was the lowest in at least twenty years, with only 56 per cent of the adults of voting age actually voting.

More interesting than these overall facts is what happened to the "traditional" coalitions of the two major parties. Let us consider the Democrats first (Table 1). The Democrats are often thought of as a coalition of diverse overlapping minorities: the poor,[4] Blacks, union families, Catholics and Jews, Southerners, and city dwellers. In 1972 this combination suffered serious defections. The Blacks and central cities remained highly loyal in both absolute and relative terms (col. 20, 24, 28 and 32). In contrast, although the poor, the unions and the Catholics were each six to eight points more loyal to the Democrats than the nation as a whole (col. 27, 29 and 30), all three of these groups gave less than half of their votes to the Democrats (col. 19, 21 and 22). The South was not even relatively pro-Democratic in 1972 (col. 23 and 31). Looking at it another way, the contribution of the Blacks has continued to grow until in 1972 they accounted for 22 per cent of all Democratic votes (col. 2). Nevertheless, both the union and the Catholic contributions to the Democrats (col. 3 and 4) were substantially larger than the Black contribution due to the much larger size of these two groups (col. 9 and 10).

The Republicans did well among all six of their traditional coalition groups with sixty per cent or more of the vote of the nonpoor, the Whites, the nonunions, the Protestants,[5] the Northerners, and those outside the central cities (Table 2, col. 19–24). But even more impressive is that the Republicans received a majority of the votes from the complements of four of these groups, namely the poor, the unions, the Catholics and the Southerners, as we have seen in examining the traditional Democratic supporting groups.

The young, who were previously not part of anyone's coalition, made a large contribution to the Democratic coalition in 1972. In each previous election since 1952 people under thirty years of age accounted for only 13 per cent to 15 per cent of the Democratic votes, but in 1972 they accounted for fully 32 per cent of the Democratic votes. There are three reasons for this dramatic increase. First, their size went up from 18 per cent to 28 per cent

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[1] *APSR*, 66 (March, 1972), 11–20.

[2] The survey material is again from the Survey Research Center of the University of Michigan and this time has a post-election sample size of 2285. I gratefully acknowledge the help of James Beniger, my research assistant. I also wish to thank the Institute of Governmental Studies of the University of California, Berkeley for its financial support of this study, and the Inter-University Consortium for Political Research for making the survey data available.

[3] The Democrats, however, still had a big lead in party identification with 40 per cent against 25 per cent for the Republicans and 28 per cent independent.

[4] The proportion of people earning less than $3000 per year has shrunk from 16 per cent in 1968 to 12 per cent in 1972 (Table 1, col. 7), partly because of inflation. The magnitude of the inflation is indicated by the Consumer Price Index: using 1957–59 = 100, the figure for 1968 is 121.2, and the figure for 1972 is 145.7.

[5] As in previous years, Protestants are taken to include the nontraditional Christian sects.

Table 1. The Democratic Coalition, 1952–1972

	Percentage Contribution						=	(Size						×	Turnout						×	Loyalty)						÷	(NT×NL)	
Year	P	B	U	C	S	CC		P	B	U	C	S	CC		P	B	U	C	S	CC		P	B	U	C	S	CC		NT	NL
1952	28	7	38	41	20	21		36	10	27	26	28	16		46	23	66	76	35	68		47	83	59	57	55	51		63	44.4
1956	19	5	36	38	23	19		25	9	26	25	29	14		40	23	64	72	39	63		47	68	55	53	52	55		60	42.0
1960	16	7	31	47	27	19		23	10	25	25	34	13		46	31	60	74	50	74		48	72	66	82	52	65		64	49.7
1964	15	12	32	36	21	15		19	11	23	26	28	12		45	42	69	72	49	65		69	99	80	75	58	74		63	61.1
1968	12	19	28	40	24	14		16	11	24	26	31	10		44	51	61	68	53	63		44	92	51	61	39	58		62	42.7
1972	10	22	32	34	25	14		12	11	25	25	34	8		37	47	58	65	44	60		45	86	45	43	36	61		56	37.5
Column	1	2	3	4	5	6		7	8	9	10	11	12		13	14	15	16	17	18		19	20	21	22	23	24		25	26

Year	Percentage Deviation in Loyalty to Democrats					
1952	+2	+38	+14	+12	+10	+ 6
1956	+5	+26	+13	+11	+10	+13
1960	−2	+22	+16	+32	+ 2	+15
1964	+8	+38	+19	+14	− 3	+13
1968	+1	+49	+ 8	+18	− 4	+15
1972	+8	+49	+ 8	+ 6	− 2	+24
Column	27	28	29	30	31	32

P	poor (income under $3,000/yr.)
B	Black (and other nonwhite)
U	union member (or union member in family)
C	Catholic (and other non-Protestant)
S	South (including border states)
CC	central cities (of 12 largest metropolitan areas)
NT	National turnout
NL	National loyalty to Democrats

Table 2. The Republican Coalition, 1952–1972

	Percentage Contribution =						(Size					×	Turnout					×	Loyalty)						÷ (NT×NL)	
Year	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NP	W	NU	P	N	NCC	NT	NL
1952	75	99	79	75	87	84	64	90	73	74	72	84	72	67	61	58	73	61	56	57	61	61	57	57	63	55.1
1956	84	98	78	75	84	89	75	91	74	75	71	86	67	64	58	56	69	60	59	59	63	62	60	60	60	57.4
1960	83	97	84	90	75	90	77	90	75	75	66	87	70	68	65	61	71	63	50	51	55	63	50	52	64	49.5
1964	89	100	87	80	76	91	81	89	77	74	72	88	67	66	61	60	68	63	40	42	45	44	38	40	63	38.5
1968	90	99	81	80	80	92	84	89	76	74	69	90	65	63	62	60	66	62	44	47	46	49	47	45	62	43.4
1972	93	98	77	74	73	95	88	89	75	75	66	92	58	57	55	52	62	55	61	66	63	64	60	63	56	60.7
Column	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

Year	Percentage Deviation in Loyalty to Republicans					
1952	+1	+2	+6	+ 6	+6	+2
1956	+1	+1	+5	+ 4	+2	+2
1960	0	+1	+5	+13	0	+2
1964	+1	+3	+6	+ 5	−1	+1
1968	0	+3	+2	+ 5	+3	+1
1972	+1	+5	+3	+ 3	−1	+2
Column	27	28	29	30	31	32

NP nonpoor (income over $3,000/yr.)
W White
NU nonunion
P Protestant
N Northern (excluding border states)
NCC not in central cities of 12 largest metropolitan areas
NT National turnout
NL National loyalty to Republicans

of the adults of voting age principally due to the lowering of the voting age, but also due to the baby boom. Second, their turnout went up relative to the nation as a whole from 15 per cent below average to 9 per cent below average. Third, their loyalty which had never been more than 3 per cent pro-Democratic since 1952 went up in 1972 to 12 per cent pro-Democratic.

The future of the electoral coalitions cannot be predicted with certainty, but several important shifts which have taken place in recent years can now be seen more clearly. The members of union families used to be 13 per cent to 19 per cent pro-Democratic but since 1968 they have been only 8 per cent pro-Democratic (Table 1, col. 29). The South, which used to be 10 per cent pro-Democratic, has continued the pattern started in 1960 of voting within a few percentage points of the nation as a whole (Table 1, col. 31). To offset these losses the Democrats can at least be pleased that the young in 1972 have finally made a large contribution to their electoral coalition.

ROBERT AXELROD
University of California, Berkeley

TO THE EDITOR:

The article by Searing, Schwartz, and Lind on "The Structuring Principle" (*APSR*, 67 [June, 1973], 415–432) sets forth a theoretical reconstruction of the basic assumptions underlying research on childhood political socialization which seems to me neither conceptually satisfactory nor adequately tested by the authors' data analysis. Their reconstruction draws eclectically from works on pre-adult politicization by a number of writers, and tends to homogenize and drastically simplify various assertions and implications in the existing literature. I agree with Searing and associates that many of the assumptions underlying political research on children have been insufficiently explicated, but disagree that the literature presumes the model they set forth. Rather than engage in the sterile exercise of debating "who said what," let me summarize their reconstruction, indicate why it is inadequate, and briefly allude to deficiencies of their empirical procedure. Then I will offer a number of answers to the question with which they begin their essay but never answer: "Of what interest to political science are children's attitudes?"

Searing et al. argue that students of political socialization seek to establish chains of causal linkage connecting learning of political dispositions in childhood with adult political dispositions and behavior and, most distantly, with such states of political systems as their capacity to persist and the distributions of attitudes in day-to-day politics. The authors do not attempt to deal with the last portion of this causal chain—the aggregation of individual behavior into system states—but focus instead on their own idiosyncratic reconstruction of the steps from childhood learning to adult dispositions and behavior, which has the following elements:

Children learn generalized political dispositions in contradistinction to attitudes on specific political issues. (For the former, Searing et al. introduce the term "orientations".)

These generalized orientations tend to persist unchanged into adult life due to an unexplicated process the authors call "the primary effect."

During the individual's adult years, he confronts stimuli emerging from the political arena and as a result of the structuring effect of his childhood-derived general orientations, he acquires (more or less as deductions from these general orientations) specific attitudes on concrete issues of the day.

This last sequence, which Searing et al. dub the structuring principle, is treated as occurring only at the adult stage of the childhood political socialization→adult political behavior sequence. Their reconstruction further assumes that adult political behavior is largely mediated by specific attitudes on concrete issues. But, their reconstruction continues, if the political socialization literature to date is empirically sound, the specific issue attitudes held by adults would have to be strongly associated with (predicted by) adult *general* political orientations. Otherwise some or all (the authors are evasive on this point) of the research that has been done on pre-adult political dispositions would not be of interest to political science.

Through the foregoing reasoning Searing and associates arrive at a test of the validity of political research on *children* by manipulating survey data on *adults*. By using (single) survey items they consider as indicators of general political orientations and specific issue positions, they emerge with matrixes of weak correlation coefficients. Rather than interpreting their findings merely as one more of the many demonstrations of weak attitudinal constraint in adult populations, they treat them as a test of the weakest link in the putative chain of assumptions underpinning the political socialization literature—the structuring principle.

There are major—I would say overwhelming—shortcomings in the way Searing and associates conceptualize, operationalize, and test their reconstruction.

Among the conceptual difficulties are: (1) an overly dichotomous distinction between general orientations and issue positions, given the general souplike quality of both pre-adult and adult psychological processes; (2) a failure to include in their reconstructed model the learning of political cognitions, even though much of the literature deals with cognitive learning and its structuring effects, many of which occur in the pre-adult years; (3) a failure of their conceptualization to take into account the diverse and inconsistent "general orientations" individuals acquire, even though it is in part such basic value conflicts and the resulting trade-offs that keep general orientations from being strongly predictive of behavior; (4) an overriding failure to treat individual *political behavior* rather than issue positions as the dependent variable, even though much of the literature on which they draw makes inferences from the entire array of learned political dispositions to political behavior, and does not presume the sequence: general orientations → specific issue positions → behavior; and (5) a complementary lack of attention to the way that socialization-acquired dispositions become behaviorally consequential as a result of interactions with specific environmental contexts, and to the likelihood that socialization-derived dispositions will vary among population categories. (As a result of this last difficulty the authors in their empirical test emphasize the primary effects and ignore the many fascinating statistical interactions reported, for example, in the work of Jennings and his associates on such matters as the division of political labor in the family and the effects of school curricula.)

It would be possible to expand on the previous points and to expound at comparable length on problems of operationalization. On the latter I will simply note that the use of single survey items as indicators contributes to the weakness of the correlations, that no external validations are used to justify the authors' assignment of some items to the general orientations category and some to the specific issue category, and that the matrixes contain many cells in which the associations are weak or nonexistent precisely because there is no reason in principle to expect anything else.

Let me conclude with a suggestive, abbreviated list of some of the reasons that the study of children may be of interest to students of politics, beginning with what seems to me a more useful recasting of the "principles" the authors describe as the primacy and structuring effects.

1. *Under some circumstances* (and these need to be specified) early-learned political dispositions may have a distinctive tenacity and potency in affecting aspects of later political behavior. (Possible circumstances include learning of certain kinds of low-saliency dispositions that do not later become challenged, learning that is culturally reinforced, and learning that becomes implicated in personality formation.)

2. Pre-adult political learning can (depending upon numerous additional conditioning factors) in fact structure later political learning with consequences for adult political behavior. Furthermore, contra one of the conclusions of Searing et al., even pre-adult learning and structuring sequences which do *not* persist into adult life may be consequential for adult political dispositions and behavior. For example, until English children are adolescents, they appear to believe that the Queen is the effective ruler of their nation. No English adults appear to have that view, but it remains a task for longitudinal political socialization research to establish if believing in childhood that one's nation is ruled by a hereditary monarch is consequential for how one's other political dispositions—e.g., the disposition to seek to influence public officials—take shape.

3. Children also are interesting as naive cultural informants. Insights into the adult (political) culture can be gained by establishing what they absorb, what adults feel it necessary to expose them to, and so forth.

4. Most political systems invest resources in explicit processes of political socialization—for example, civics courses. At the very minimum, research of a narrow cost-benefit sort is warranted to see if such procedures are effective.

5. Political learning is a property of political systems, since systems have to replenish their personnel. In some systems this occurs with exceptional continuity between generations. In others the discontinuity is striking at particular times in history. At least in this pair of extreme cases, the study of political learning seems likely to be of great interest.

6. Political dispositions, whether of children or adults, can be conceived of as properties of populations, as well as of individuals. Many studies show that the same individual when requestioned shifts in response to questions about issues. Surveys of the distribution

of opinion on issues, however, show consistently patterned response distributions or readily explicable distribution shifts over time. Some of the instability at the individual level results from well-known attributes of mass publics; some may result from deficiencies in our standard survey procedures. But as long as the distributions are politically consequential and interesting they can be profitably studied among any age group that exhibits political dispositions. Lack of continuity and constraint in political orientations is one of the central focuses for pre-adult and adult political psychology—not a reason for failing to develop these literatures.

FRED I. GREENSTEIN

Princeton University

TO THE EDITOR:

The recent article by Searing, Schwartz, and Lind ("The Structuring Principle: Political Socialization and Belief Systems," *APSR*, 67 [June, 1973], 415–432) is interesting, insightful, and stimulating, but it leaves us with several reservations.

The authors conclude, we think a little too confidently, that "our findings cast considerable doubt upon the structuring principle, and thereby, upon the theoretical relevance of much childhood research under allocative politics and system persistence models" (p. 430). The validity of this broad claim is contingent, in our view, upon the accuracy of their interpretation of the literature dealing with the structuring principle ("basic orientations acquired during childhood structure the later learning of specific issue beliefs"), and the reasonableness of their test of this principle. It is our position that the authors have interpreted the literature in such a manner as to construct a "straw man," and that certain portions of the test put forth are not as "reasonable" as they might appear upon initial examination.

Before dealing with these reservations, we would note that one cannot help but appreciate the quandary confronting the authors. The models of socialization are poorly articulated, and the falsification rules are often not self-evident. In order to cope with this problem, Searing et al. carefully limit the range of their research. It does not therefore cover the *primacy principle* ("childhood learning is relatively enduring throughout life"), the link between demands and policy outputs, and *behavioral constraint* ("a subclass of demand variables which do not involve issue beliefs"). After circumscribing the boundaries of their investigation, the authors focus on the relationship between political orientations and issue beliefs, and personality factors and issue beliefs. It is worthwhile to note, therefore, that the authors are concerned with only a very small portion of the literature on political socialization.

While Searing et al. provide us with a rather penetrating and sensitive survey of various facets of the socialization literature, their treatment of material concretely dealing with the structural principle is less satisfying and is somewhat ambiguous. We are told that there are two broad models (allocative and persistence) for relating socialization data, with the explication of these models drawing primarily on Easton. Only one *specific* example (Hyman's use of party identification) is given of an orientation structuring new issue beliefs, while Greenstein, Easton and Dennis, Hess and Torney, Dawson and Prewitt, and Campbell et al. (*The American Voter*) are cited in footnotes as subscribing to the structuring principle. What is significant here is the vague treatment of this literature. We are never told who in the field specifically subscribes to the allocation and persistence models *as they relate to the structuring principle*. Certain scholars are cited as subscribing to the structuring principle, but a crucial question is *to what extent?*

Which scholars, if any, argue that many political orientations are related to a wide range of issue beliefs? Who suggests that many political orientations are associated with a particular issue belief or beliefs? Do any contend that select orientations are connected with a wide range of issue beliefs? If the authors cannot identify those who emphasize the structuring principle or those who stress the centrality concept, then they are simply conducting an exploratory investigation. They are setting up, at least in part, a "straw man."

The "straw man" facet of the problem becomes more meaningful when we take into account the literature of public opinion. This literature indicates that elements of mass belief systems show little constraint, that there are few relationships between orientations and issue beliefs, and that frequently issue beliefs are not highly related with each other. These data, if anything, suggest that we not overemphasize the salience of issue beliefs. Hence it might be used to question the wisdom of the type of exploratory study undertaken by the authors.

Cognizant of this literature, Searing et al.

argue that it does not constitute an adequate test of the structuring principle for such findings either "are restricted to very few orientations used in socialization research, or they focus upon relationships among issue beliefs alone." This argument begs the question—why conduct an exhaustive investigation of orientation/issue beliefs at all? One reason might be to test specific literature in the field. As we have previously pointed out, however, the authors are vague with respect to identifying exactly what literature is being tested. Another reason might be to break new ground and put forth new theoretical hypotheses. It would be useful, for instance, to explain why a particular orientation (e.g., political cynicism) should correlate with a particular issue belief position (e.g., government aid for health care). This the authors fail to do. Basically they end up contending that only a few orientations have been correlated with issue beliefs in public opinion research, and that many thereby merit investigation. In view of the public opinion literature on attitudes/non-attitudes, the inability of the authors to identify specific literature to be tested, and their failure to offer new hypotheses, we conclude that the authors' exploratory examination while interesting and exhaustive does indeed set up a "straw man."

Our second area of concern is with the test itself. One difficulty rests with the choice of issues. The fifteen Survey Research Center (SRC) and the fourteen Southeast Regional Survey (SERS) issue beliefs were chosen "for their salience and importance in American politics during these years." Despite the fact that public opinion research has consistently demonstrated weak correspondence between such a major political orientation as party identification and foreign policy issues as well as between domestic and foreign policy issues (e.g., Campbell et al., V. O. Key, Alfred O. Hero, and Page and Brody, etc.), the authors selected six foreign policy issues from the SRC data for political orientation/issue belief testing. When six of the fifteen issue beliefs and 40 per cent of the 195 relationships tested from the SRC data concern foreign policy, one should hardly be surprised that many of the relationships are not significant.

By way of contrast, only two domestic economic issue beliefs from SRC (government aid for health care and governmental guarantee of a good living standard) and three from SERS (the former plus progressive taxes) were chosen for investigation. This is unfortunate for it is the domestic economic issue domain that looms prominently in establishing relationships with select political orientations (party identification, evaluation of governmental power, approval of conservatives). In the authors' own analysis of the SRC data, the single highest relationship of the 195 examined is between party identification and government aid to health care (.32). The relationship between party identification and government guarantee of a good living standard is also statistically significant (.18), but it does not meet their additional criterion of .20 as a marginal but acceptable relationship. The second and third highest relationships in the political orientation/issue belief SRC data are between evaluation of government power and government aid to health care (.27), and the former orientation and government guarantee of a good living standard (.25). Both are statistically significant and meet the .20 acceptability criterion. The fourth highest relationship is between approval of conservatives and government aid to health care (.23) and it too is both statistically significant and acceptable. The relationship between the former and government guarantee of a good living standard (.17) is statistically significant (with evidence of nonmonotonicity) but not acceptable. In the SERS data the two highest relationships of eighty-four tested are between party identification and government aid to health care (.22) and between the preceding orientation and government promoting good living standards (.29). These correlations are significant and acceptable, but show evidence of nonmonotonicity. *In short, the authors' own data demonstrate relationships between certain political orientations and economic issue beliefs.*

Moreover, other studies have shown comparable relationships between select political orientations (primarily party identification) and economic issue beliefs. Gerald Pomper, for example, utilized the same 1968 SRC data ("From Confusion to Clarity: Issues and American Voters, 1956–1968," (*APSR*, 66 [June, 1972], 415–428) and found significant gamma relationships between party identification and four economic issues as well as between party identification and school integration. These relationships are reported in the accompanying table on page 724.

It should be noted that three of these four economic issues were *not* utilized by Searing et al. (only health care was selected), and that Pomper also found significant linear relationships between these four issues and party identification in the *1964 SRC data.* Erikson and Luttbeg (*American Public Opinion: Its Origin,*

SRC—1968. Comparison of the Relationship of Party Identification to Select Domestic Issue Beliefs in Three Separate Studies

	Searing Schwartz Lind (Kendall's t_c)	Erikson Luttbeg (gamma)	Pomper (gamma)
Medical Care	.32	.433	.41
Standard of Living	.18	.325	
School Integration	.14	.237	.43
Aid to Education			.36
Job Guarantee			.25
Fair Employment			.24

Note: The differences observed between the Erikson/Luttbeg and Pomper analyses of the same data are presumably the result of slightly different scaling procedures.

Content, and Impact, p. 90) have identified significant gamma correlations in the *1968 SRC data* for party identification and two economic issues (medicare and standard of living) and between the former and school integration Seven other correlations between party/noneconomic domestic issues and party/foreign policy issues were not found significant.

In comparing the analysis of the 1968 SRC data between Searing et al., Pomper, and Erikson and Luttbeg (see table preceding) on the relationship of party identification and select issue beliefs, one should bear in mind that Searing et al. use Kendall's tau while both the latter use gamma. Hence we might expect their correlations to run about .05 to .10 lower for the same test. In view of this, we conclude that all three are roughly in agreement on party/Medicare. Erikson and Luttbeg find a higher relationship between party/standard of living than Searing et al. and Pomper finds relationships between party and three economic issues not tested by either of the former. On party identification/social integration Pomper finds a much higher relationship than either the authors or Erikson and Luttbeg, with the latter's data suggesting a stronger relationship than the authors. In view of the disagreement found using different techniques of analysis and different scaling procedures on the same data, this particular relationship merits considerable reinvestigation. Overall, the data suggest relationships between party identification and five economic issues and possibly an important relationship between the former and school integration. We conclude, therefore, that the *selection of issue beliefs* (foreign vs. domestic, and within the domestic issue domain, primarily economic vs. noneconomic) to be tested and the *choice of political orientations* play an important role in substantiating or disproving the structuring principle.

Another weakness in the Searing et al. test is their operational definition of the centrality concept. Positing that political orientations must cue individuals for a wide range of issue beliefs, the authors calculate a *mean* correlation for given political orientations as they relate to fifteen issue beliefs (SRC) and fourteen issue beliefs (SERS) respectively. They lump together foreign and domestic issue beliefs, as well as domestic-economic and domestic-noneconomic preferences, and insist that a particular political orientation relates to a wide range of issue preferences. Is it any wonder that centrality is not established even for party identification? If one were to disaggregate the data into *issue domains* and focus primarily on the relationship between certain major orientations (primarily party identification) and economic issues, a case for centrality can be made. Using the authors' SRC data, for instance, one would find that the mean correlation between party identification and the two economic issues is .25, between the evaluation of government power and the latter .26, and between approval of conservatives and the economic issues .20. All would be significant, as well as meet the Searing et al. acceptability criterion of .20. Drawing on Pomper's data, we would discover that the mean gamma correlation between party identification and his four economic issues is .315, and that a similar correlation between party and the two economic issues in Erikson and Luttbeg is .379.

The authors also purport to test the supposedly widespread assertion (although only one source is cited) that "the greater a political orientation's importance in adult behavior, the earlier it is learned in childhood." In our judgment, such a proposition involves *developmental* processes which can only be examined through *longitudinal* research. There is no way to answer this question as it is set forth here by employing the SRC and SERS adult research based on cross-sectional samples. Yet the authors purport to do so by comparing the mean correlations for trust and party identification (learned early in life) with those of political cynicism orientations (learned later in life). The comparison shows no substantial difference. In the SRC data, for example, trust in government has a mean of .05 compared with .08 and .04 for the two political cynicism measures.

Apart from the dubious nature of such a

test, we would again question their wide-ranging centrality assumption. Assuming for purposes of argumentation that their cross-sectional test is valid, we could again disaggregate the data into issue domains and look particularly at the economic issues. In the SRC data, the mean for party identification is .25 compared with .06 and .01 for the two cynicism measures. We do not suggest that such a disaggregation proves that party identification is more important than adult behavior on the grounds that it was learned earlier in childhood, but only that one can draw different conclusions from the authors' own test and results. Thus we would contend that they exaggerate (or at best are premature) in their claim that "evidence has also been presented contradicting the widespread assumption that the more important an orientation is in adult behavior, the earlier it is learned during childhood."

Finally, we would like to comment upon the future direction of political socialization research. Although Searing, Schwartz, and Lind have presented us with a useful contribution to the literature, their effort clearly demonstrates the need for longitudinal research. The relationship that we ourselves have suggested between party identification and economic issues can only be viewed as a *prima facie* case, because it is based on cross-sectional data. Given the entire attitude/non-attitude problem and the generally low salience of issue beliefs, longitudinal research of adult issue beliefs is necessary before sweeping conclusions, such as those presented in the article in question, are appropriate with respect to either the structuring or primacy principles.

The analysis of subgroups and orientation/issue beliefs is also desirable. In doing so we need to stipulate clearly the theoretical rationale. Upper middle-class McGovern supporters and blue-collar Wallace identifiers, for example, may both score high on cynicism, but the direction of their issue beliefs is likely to be quite different. Here we must ascertain the object or direction or both of the cynicism—cynicism about whom and over what? To combine both groups in a national sample may be useful for some purposes, but it is not appropriate if one is interested in relating orientations to issue beliefs, since the preferences of the two groups are likely to cancel each other out.

In addition to longitudinal research and subgroup analysis, we would also recommend a more eclectic research orientation. Along with the traditional use of structured survey questions, researchers should also consider the possibility of using open-ended interview material to investigate questions of issue beliefs. A good example of this type of research is David E. RePass, "Issue Salience and Party Choice," (*APSR*, 65 [June, 1971], 389–400). Other approaches besides survey research should also be explored, including psycho-historical efforts. These efforts would take into account previous in-depth studies by Lasswell, Erikson, Lane, the Georges, Barber, etc. Such research is not without its limitations (and strengths), but it is a body of research that must be considered before we can confidently dismiss or substantiate the structuring principle or other principles involved in the socialization process.

JAMES W. CLARKE
HENRY C. KENSKI
University of Arizona

TO THE EDITOR:

Like any new subfield, political socialization includes some important assumptions which are insufficiently articulated. Efforts to sharpen them are constructive contributions to growth and development. We certainly wrote the article in this spirit and trust that the present exchange can be seen in the same light.

An efficient regimen for clarifying such assumptions is the attempt to state them in terms amenable to empirical investigation. Clarification risks simplification. And, in these circumstances, Greenstein's reaction is neither unusual nor unjustified: operationalizations homogenize diversity, shunting aside many implications of the assumptions examined. No doubt we have done something like this. But, if our model is not *one* reasonable interpretation of the first two statements listed in his communication's conclusion, we are indeed puzzled. And should those statements mean something other than what they say, the burden would seem to be upon their author to explicate them more satisfactorily.

These statements appear to us little different from their equivalents in the socialization literature upon which we drew. The first offers, among other things, the primacy principle: early learning has a distinctive tenacity or persistence. The second puts forward the structuring principle: basic orientations acquired during childhood structure subsequent adult political learning. Greenstein emphasizes the qualifications. But we said quite clearly that everyone made the arguments conditional. They presented them cautiously as suggestive hypotheses. What we have claimed is simply this. If many orientations used in socialization research are unrelated to the key policy issues of the day, then it is difficult to understand how their political significance lies in structuring later political learning with consequences for political outcomes. It is not convincing to dismiss this as nonsense and then, in the end, restate the same structuring principle, emphasizing that it applies only under "numerous conditioning factors" which have yet to be satisfactorily specified.

On Strawmen

The article's model does not encompass everything our colleagues meant by statements we have characterized as the structuring principle. We believe, however, that the model incorporates central aspects of what was said; aspects which hang together in explicating one version of a widespread assertion that childhood learning is related to political outcomes. This is a presumptuous exercise. No matter how hard one tries to avoid it, such work can easily turn out a straw man. For exactly the same reasons, it is every bit as easy to label a straw man whatever is turned out. Greenstein implies the entire model deserves the appellation. Clarke and Kenski would award the title only to the structuring principle link. Let us examine what they have said.

While not rejecting the structuring principle as stated, Clarke and Kenski pose two questions. One is: Who, besides Hyman, said it? Assumptions like the primacy and structuring principles are not baldly stated wherever one looks. If they were, they would undoubtedly have been clarified and examined some time ago. While Clarke and Kenski's question is surely a fair one, the burden of our argument, it must be emphasized, is that the structuring principle seems essential to the claim that childhood orientations affect political outcomes *by shaping later political learning*. That said, how many statements need be put forward to justify an empirical look? The article discussed Hyman's position as an illustration and referred to others which, we believe, have the same implications. Some of these citations are, in fact, mentioned in Clarke and Kenski's sentence preceding their query. Perhaps the following quotations will reinforce the case:

> Orientations learned early in life determine much of the form and substance of attitudes toward specific personalities, activities, and policies which are acquired later. (Dawson and Prewitt, *Political Socialization*, p. 43).

> The concepts, information and feelings that are first acquired serve as "filters" through which later perceptions must pass. Thus, the child who "knows" that he is a Democrat . . . will learn to orient himself toward the utterances of Democratic politicians and to acquire issue commitments consistent with his initial loyalty. (Greenstein, "Political Socialization" in *International Encyclopedia of the Social Sciences*, p. 554.)

Clarke and Kenski's second question is more difficult: Did anyone mean that political orientations are related to a wide range of issue beliefs? We think so—but how wide a range is admittedly unclear. Still, the argument's logic requires that, collectively, the orientations must at least be related to beliefs about key policy issues of the day. Otherwise, it is difficult to see how a few orientations could, by filtering later learning, have a meaningful impact upon demands and supports.

Greenstein's reconstruction of our model conveys the impression that it may have been a figment of our imaginations. Yet, he does not say that children's orientations have *not* been studied because they are believed to persist into adult life with consequences for the political system; that researchers have *not* distinguished orientations from issue beliefs and suggested that the former shape the latter; that, in fact, any particular relationship in our model has *not* been asserted in the literature. Instead he implies it—and dismisses the whole as inadequate. This seems unsatisfactory without explaining exactly what was said, or even what might be said to make a more compelling argument linking childhood learning with political system consequences. What he does say in his explicit subsequent criticisms are all points of considerably lesser sweep. And even if each was largely correct, they would not, we believe, turn the entire model into anything like the chimera he appears to feel it is. Before considering these more substantive comments, the discussion should be placed in proper perspective.

Thus, the article's findings reflect only upon those aspects of children's orientations which were: (a) surveyed under the aegis of allocative politics and system persistence models; (b) assumed to affect political outcomes by structuring the later learning of issue beliefs; and (c) available for examination in the data analyzed. That certainly does not cover all socialization research. And, we never claimed it did. Political socialization is well established in the discipline, has considerable resilience and can only profit from critical self analysis. From this perspective, our research program anticipated finding many more relationships than turned up, using these to suggest conditions under which the structuring principle might and might not apply. Actually, the data did suggest a condition that we missed. Clarke and Kenski have pointed this out.

Conditions

The structuring principle, it is often said, may hold only under specific conditions. But, these conditions are rarely stated. Clarke and Kenski put one forward: The principle's applicability might be restricted to domestic economic issues. Comparing our findings with Pomper's and Erikson and Luttbeg's the argument appears quite persuasive. It surely deserves further investigation. At this point, the items examined are few, relationships weak, and criteria for inclusion in this issue area need to be specified. Nonetheless, it would be foolish to begrudge a ray of light. Clarke and Kenski's refinement is a welcome guide for further steps in the right direction. And, if established, it might well provide the desired political relevance for orientations involved.

A different sort of condition would restrict the structuring principle's applicability to particular population subgroups. Greenstein and Clarke and Kenski mention this possibility, as we did ourselves in the article. Groups with comparatively homogeneous environments and, hence, reinforcing socialization experiences, would be prime candidates here. In fact, we tried to construct such groups, seeking individuals whose residence, social class and school locations had remained stable throughout their lives. Unfortunately, with national samples of mobile American society, the Ns quickly become too small for satisfactory analysis. It is, of course, true that the paper's negative results do not apply to groups of this nature—as is the case for any findings based on national samples. Group analysis along these lines may produce additional conditions under which the structuring principle could fulfill its expectations.

Other Criticisms

We turn now to more detailed criticisms of the article's analysis. Greenstein's comments will be examined first. They are, we think, of two types. One focuses on what he believes the paper might have done. These can most usefully be interpreted as directions for further research and clarification—not as critiques of the paper. The other sort do pose questions which meet our analysis. We will do our best to respond to these, though they are sometimes too pithy to advance the argument as much as we would like.

1. It is quite correct that the paper did not investigate: (a) relationships between behavior and beliefs; (b) relationships between behavior and system states; (c) the primacy principle; or, (d) the role of political cognitions. Explicating and testing the structuring principle provided more than enough material for one article.

2. Modesty requires that we disclaim credit for having "introduced the term 'orientations.'" In his letter, Greenstein prefers "dispositions." But it was "orientations" in his earlier work, as it has been in the work of nearly everyone else. Moreover, the paper did not conceptually dichotomize general orientations and issue beliefs. Instead, a continuum was posited by level of generality in attitudinal objects. Nonetheless, the level of generality dimension is far from crystal clear. Little advance has really been made since Hadley Cantril's seminal monograph in 1932 ("General and Specific Attitudes," *Psychological Monographs* 42, 5, no. 192). In any event, it seems to us that items the paper treated as orientations and those treated as issue beliefs are conceptualized this way in socialization literature. Greenstein offers no specific examples of misclassifications. And, we find his objection that all these attitudes are somehow "soup-like" neither telling nor helpful.

3. General orientations are usually said to structure issue beliefs through metaphors such as: shape, determine, condition, constrain, filter, cue, and political eyeglasses. Surely these terms imply some sort of predictive relationship. But Greenstein misunderstands us when he suggests we believe these to be necessarily *deductive* links.

4. The structuring principle was treated as occurring "at the adult stage of the childhood political socialization → adult political behavior sequence" because: (a) young children do not possess many issue beliefs; and (b) it is usually only adult beliefs which may be transformed into demands and supports; a step on the way to shaping political outcomes and thereby establishing theoretical relevance.

5. Why is it "an overriding failure to treat individual political behavior rather than issue positions as the dependent variable . . ."? This is the behavioral constraint linkage which relates orientations to behavior directly, by-passing intervening issue beliefs. The article circumscribed this position as best it could and clearly stated that the notion was not being tested. Instead, the second major linkage, the structuring principle, was examined. Treating only one of the two paths by which orientations shape demands and supports may be a

disappointment, but not, we hope, an overriding failure.

6. It does indeed seem appropriate to test the "validity of research on children by manipulating survey data on adults" wherever: (a) theoretical relevance of children's orientations depends upon both their persistence and the role they play in adult life; and (b) the test is restricted to one portion of the generalized socialization model: the structuring principle. The article argued this case at considerable length. And, we should like to stress that Greenstein does not offer any concrete reasons for supposing that the test was, in fact, theoretically inappropriate.

7. Similarly, we need to know what exactly is wrong with "using (single) survey items as indicators of specific issue positions," and why this should necessarily produce "matrixes of weak correlation coefficients." There is undoubtedly something to be said for Greenstein's position here. But, without elaboration, this rather extreme formulation cannot be accepted as either true or as undermining the article.

8. Our major finding was that many orientations used in socialization research are generally unrelated to beliefs about the outstanding issues of the day. Should this be interpreted "merely as one more of the many demonstrations of weak attitudinal constraint in adult populations"? We think not. Its relevance to socialization research is found in statements (1) and (2) from the conclusions to Greenstein's communication.

9. We agree with Greenstein that respondents may hold "diverse and inconsistent general orientations" which, in part, may prevent orientations from predicting other beliefs and behavior. Until these diverse patterns are mapped out, however, they can neither be incorporated in models nor tested. The article did attempt an exploratory multiple regression analysis. Its failure to raise predictability underlines the need to map explicit patterns so these may be examined empirically.

10. Finally, Professor Greenstein suggests that so few relationships were found between orientations and issue beliefs "because there is no reason in principle to expect anything else." What might that mean?—that children's orientations are not expected to persist and structure the later learning of issue beliefs? Clearly, the expectation was put forward by Greenstein in the section of *Children and Politics* entitled, "Effects of Early Learning on Later Learning" (pp. 81–83). His creative and subtle book offers much more by way of theoretical relevance than this particular claim. Actually, Greenstein stresses the structuring principle less, perhaps, than do other researchers in the field. But he did say it; and it has been widely regarded as one of the ways in which childhood learning is connected with political outcomes.

We now proceed to a consideration of Clarke and Kenski's comments.

1. There was a reason that foreign policy issues were used in the analysis, and that more domestic economic issues were not. It is because to produce a meaningful impact upon demands and supports via the structuring principle, orientations must presumably be related to beliefs about the key policy issues of the day. In 1968, Vietnam was a prominent policy issue. Federal aid to education failed the test. Moreover, some foreign policy issues have, in the past, been successfully related to at least one broad political orientation (see, for example, the article's footnote 33). Still, Clarke and Kenski have a legitimate point. The paper might have been more comprehensive and might have utilized all issue beliefs available in the SRC and SERS data.

2. One can readily agree that a better case might be made for party identification's centrality by examining relationships with particular types of issue beliefs. But then, of course, one is not examining party identification's centrality in "political belief systems"—rather only in one segment of such belief systems. Although this is a sensible strategy for mapping belief systems, it misses the point of the test. Party identification is the most widely cited example of how political orientations serve as frames of reference in formulating issue beliefs. Its putative performance seems to have been generalized to other orientations used in socialization research. From this perspective, it is instructive to see how party identification compares with other orientations in its associations with beliefs about the leading issues of the day; not just with beliefs about a particular subset of those issues.

3. Clarke and Kenski believe the following proposition can only be tested with longitudinal data: the greater a political orientation's importance in adult behavior, the earlier it is learned during childhood. Yet, we do know when many orientations are learned by the present generation of children. And we have other data on the orientations' importance during adulthood. They are saying that what is really required is one set of observations on one set of respondents over time. Such data are not yet available. But cross sectional analysis can nevertheless shed some light on the

proposition by introducing one assumption: when the adult respondents were children, they learned the orientations in the same order as do children of the present generation for whom information is at hand. We utilized that assumption because much socialization literature makes it on what appears to us good ground, including the notion that stages of cognitive development should not vary much across generations. Still, the test is a weak one, undertaken for exploratory purposes. It is weak as well in the sense that our measure of importance in adult behavior is only one of several meanings this concept has in socialization inquiries. One might, of course, obtain different results by limiting the test to particular issue domains. But then it wouldn't really be testing the proposition, would it?—unless that proposition were turned into several explicit propositions restricted to particular orientations and particular issue domains.

Conclusion

In conclusion, we should like to express our appreciation to Greenstein and to Clarke and Kenski for putting forward so many thoughtful and provocative criticisms. Our frequent disagreements should not obscure the fact that these criticisms have improved the interpretation of our findings and embossed those assumptions which require further examination.

The paper's main concern was theoretical relevance. Such inquiries are often obscure, even a bit boring. Yet they have had an unusual attraction in socialization research, being discussed at what must seem inordinate length from the outset. Perhaps this reflects, in part, uneasiness over the very distant links between children's attitudes and political outcomes. It may turn out that socialization has unnecessarily gone out on a limb in chasing policy outputs. No doubt many of the desired connections can be satisfactorily articulated and will, in time, be established empirically. But surely, the value of mass socialization research is not limited to elucidating the sources of political system outcomes. That goal, after all, might best be sought through research on elite socialization. And, why must we always examine everything from the grandiose perspective of the system anyway? The tendency to treat citizens' beliefs and values only as independent variables shaping regime performance could easily become a strait jacket binding the field.

By contrast, many data at hand can be interpreted from a character-shaping viewpoint which regards the individual as the center piece for analysis. Over time, different regimes may have quite different effects upon their members' beliefs and even personalities. It has been found, for instance, that in the United States young children are taught uncritical and intense emotional attachments to the presidency; beliefs that powerful people attend quite carefully to their thoughts and feelings; and, religious-like devotion to their country's symbols—many children actually feel they are praying when saluting the flag! Perhaps these attitudes do eventually have their impact on the political system. But isn't it clear as well that people are being molded in ways which have important consequences for how they experience their society and polity? The "Democratic Character" and "New Soviet Man" constructs are familiar cases in point. And isn't this, by itself, of sufficient interest to justify the research effort? Richard Merelman put the position succinctly in a paper presented at the 1973 APSA Meetings: "What is there to prevent our assuming that it is the individual's political life that counts, rather than the 'life' of the political system?" ("Political Reasoning in Adolescence: Some Bridging Themes").

If we read him correctly, Greenstein believes that we have gone overboard in seeing system perspectives everywhere in the field; that a good deal of theoretically eclectic research is already under way. We hope that the article and exchange may stimulate further theoretical discussion and development, not necessarily of alternatives, but rather of additions to the still predominant systems focus.

DONALD D. SEARING
JOEL J. SCHWARTZ
*The University of North Carolina,
Chapel Hill*

TO THE EDITOR:

Game theory is a subject in which the most obvious, commonsense ideas turn out to be disconcertingly full of holes. As Anatol Rapoport has argued, this is surely the best, if not the only reason why social scientists should be interested in game theory. Undoubtedly it is the reason why Matching Pennies and Prisoner's Dilemma are exhibited to so many non-mathematical students in political science classrooms. These trivial-seeming games demonstrate clearly: (a) That in some situations it may be impossible for two actors simultaneously to choose rationally (in the sense of optimizing). This, the dilemma of Matching Pennies, can only apparently be resolved at the cost of saying that rationality may require decisions to be made *at random*—a solution that appears to some rather like going from the fryingpan into the fire. (b)

That in other cases two actors who both act "irrationally" (i.e., minimize their payoffs) do better than if both act rationally (by optimizing). This is the Prisoner's Dilemma, to which so much attention has been paid in recent years.

Does this exhaust the conceptual booby-traps that two-person game theory has in store? Your readers should know that there is a third, vitally important booby-trap. My book *Paradoxes of Rationality: Theory of Metagames and Political Behavior* reveals it for the first time, calling it the "third breakdown of rationality." Following it, moreover, a proof is given that (in finite two-person games) no further breakdowns exist; that is to say, it is proved that if a game has an equilibrium (thereby avoiding the Matching Pennies breakdown) which is undominated (thereby avoiding the Prisoner's Dilemma breakdown) and if in addition the game does not suffer from the third breakdown, then it is essentially a "no-problem" game. I prove in fact that under these conditions it has a so-called "perfect optimum" —i.e., what amounts to an obvious solution.

What is this third breakdown of rationality? It consists in the fact that what game theorists have called a "sure-thing" strategy (meaning a strategy that is uniformly best, or optimizing, or "rational," against each and every strategy of the opponent) is usually a disastrous choice for a player. It is the strategy of a "sucker" who invites, and is ready to yield before, the most extreme ultimatum in the possession of his opponent, and is thus willing to surrender his position before any bargaining begins. Specifically, I prove that in any finite two-person "mutually ordinal" game, a sure-thing strategy (if one exists) will "induce" an equilibrium that is best for the other player. When there are many equilibria, as is usual in bargaining situations, such an equilibrium is of course very unlikely to be the best equilibrium also for the player who induces it. Indeed, the essence of these situations is precisely a struggle as to whether one obtains an (undominated) equilibrium that is better for oneself, or whether one has to accept one that is better for the opponent. Hence indeed to choose a sure-thing strategy is to "give in" before the struggle starts.

I am bringing this to your readers' attention because John C. Harsanyi, in reviewing my book for your June, 1973 issue, made an unfortunate mathematical error which in effect denies the existence of the third breakdown of rationality. It is not the case, as Harsanyi asserts, that in the particular class of games considered, "all equilibrium points will yield the *same* payoff to the other player." Harsanyi's dismissal of the third breakdown is therefore based upon a mistake, and this should be put right before your readers form incorrect ideas on the subject. The class of games considered in the theorem (so-called "mutually ordinal" games) is very inclusive—in fact I argue in my book that it includes most real-life game situations. A game is mutually ordinal provided only that one player is never indifferent between two outcomes when the other player is not also indifferent—an assumption that is justified if we assume that a player otherwise indifferent between two outcomes will be residually influenced (either *pro* or *con* an outcome) by his opponent's preference. But the third breakdown does not of course require that this assumption should be satisfied in all games—only that it should be satisfied in some cases of importance. Hence the breakdown is real and serious.

I may say that there is now no difference among game theorists on the mathematical point involved here. Dr. Harsanyi in private communication has generously admitted his error, and recommended publication of a Comment setting it right that will appear in the International Journal of Game Theory. I am writing to you to correct the error for your readers' benefit, and in so doing to bring the third breakdown of rationality to their attention.

NIGEL HOWARD

The Conan Institute, Ottawa

TO THE EDITOR:

I had to admit I was mistaken when, in my review of Nigel Howard's book in the June 1973 issue, I asserted that, in the relevant class of games, "all equilibrium points will yield the same payoff to the other player." But I wish to emphasize that correction of this regrettable mistake *does nothing to rescue* Howard's so called "*third breakdown of rationality*" (and is, of course, wholly unrelated to my criticism of the other two alleged "breakdowns"). Consider the following game:

	X	*Y*
A	2, 1	0, 0
B	2, 1	1, 2

This game has two pure-strategy equilibrium points, viz. (*A*, *X*) and (*B*, *Y*). The former is more favorable to player 1 (the Row player) while the latter is more favorable to Player 2 (the Column Player). Moreover, (*B*, *Y*) is associated with Player 1's sure-thing strategy, viz. strategy *B*. Therefore, if 1 follows the sure-thing principle then he will use strategy B, and the outcome will be the equilibrium point (B, Y). But, according to Howard, it is unreasonable for Player 1 to use strategy B, and thereby to accept the equilibrium point (B, Y) less favorable to him as the outcome of the game.

In contrast, in my own view, the only rational course open to Player 1 is exactly to accept (B, Y) as the outcome. This is so because in this game *one* of the two players *must* make a concession: *either* Player 1 must accept the payoff vector (2, 1) *or* Player 2 must accept the payoff vector (1, 2)—if they are to avoid *(0, 0)*, which would be worse than either, and which in any case would not represent an equilibrium situation. But Player 1 is in a *much weaker* bargaining position than Player 2 is; therefore there is nothing unreasonable in suggesting that *he* has to make the concession.

The weakness of Player 1's bargaining position can be seen from the fact that he cannot use strategy A (which corresponds to the equilibrium point (A, X) that he prefers) if he thinks that there is the slightest positive probability p that Player 2 will use strategy Y (which corresponds to the equilibrium point (B, Y) that Player 2 prefers)—because, with any positive p, strategy A will always yield him a *lower* expected payoff than B would. In contrast, Player 2 is free to use strategy Y even if he thinks there is a substantial positive probability q that Player 1 will use A (viz. so long as q is not larger than one half).

To conclude, contrary to Dr. Howard's arguments, there is no reason whatever to modify the conclusion (rather common in the game-theoretical literature) that there are very good reasons to avoid using dominated strategies in a noncooperative game except in the case where such a strategy would yield an outcome preferable to *both* (or to all) players—and that this conclusion in no way represents a "breakdown" of rationality.

JOHN C. HARSANYI
University of California, Berkeley, and University of Bielefeld

TO THE EDITOR:

It has been called to my attention that S. J. R. Noel is not a historian, as I mistakenly called him in my review of his book *Politics in Newfoundland* (*A.P.S.R.*, 67 [September, 1973] 1078–1079), but a political scientist in good standing, and a member of the department at the University of Western Ontario. I regret the error.

STEVEN B. WOLINETZ
Memorial University

TO THE EDITOR:

One proper object of a scholarly review is to sharpen the debate between legitimate contending positions. In this respect, I am reasonably gratified by Professor Haskell Fain's review of *On Political and Historical Knowing* in the September 1973 issue of the *Review*. Sometimes, however, the necessary shortness of a review makes it difficult for the issues to be joined sufficiently and I would appreciate the opportunity to sharpen them further.

My book had two main objectives: to investigate the limitations on knowledge that at some point are invoked by the framework of inquiry and to assess how things are known and communicated. My illustrations in the introduction are designed to indicate the range of this principle, although the book proper is restricted to the social sciences. At this level of discourse my statement in the introduction concerning Gödel's theorem is both correct and adequate, although in a fuller mathematical treatment such minor clarifications as "with an axiomatic structure sufficient for the derivation of arithmetic" would justify Professor Fain's cited exception. I am sure he would be the first to agree that this is unrelated to the basic issue of my book and that, therefore, it would have been pretentious for me to introduce such detail in what is only a set of illustrations.

With respect to how knowledge is known and communicated, Professor Fain seems to suggest that I am an advocate of Hempel's position. Some might even believe that Professor Fain implies that I misunderstood Hempel's position when he writes "For Hempel, an explanation is basically a kind of linguistic entity—an *argument* having premises and a conclusion, and not a single sentence as many of Professor Kaplan's examples erroneously suggest." I am sure that he would agree that my statement on the same page as the cited example—"Properly expressed, it does so in a universalistic nomological deductive form in which laws connect the explanandum events with the particular conditions cited in the explanans"—is a more literal statement of Hempel's position than his own and that the next several pages explicate this position. On page 35 when, in answer to Scriven, I say "the effects of poison are dependent on the kind of poison and its strength as well as on the physiological system into which it is introduced," the same result is achieved as in his own precise statement in the review.

I am certain, therefore, that Professor Fain merely wanted to communicate to those readers who lack his expertise, and who might not persist to page 35 in reading the book, the caution that my words "Properly stated" might be insufficient guidance with respect to the transition from my treatment of truisms to my account of explanations in the Hempelian case, just as his comment on Gödel's theorem served a similar purpose for the uninformed reader. I suppose that some of our difference lies in what we assume to be the level of competence in the profession. Unfortunately, Professor Fain's attempt to compensate for what he apparently perceived as my failure adequately to instruct my readers gave him less space to treat a central issue between us, which lies in my rejection of Scriven's solution to the problem I suggest Hempel ignores. The subsequent discussion in the book—which Professor Fain does me the honor to call most interesting—distinguishes explanations from other types of communicated knowledge, as earlier I distinguished what can be known from what can be communicated.

To the extent, therefore, that the issue has not been joined succinctly, I am regretful. In the first place, we might have learned whether to any extent the seeming disagreement is a matter of style of expression. In the second place, had the issues been joined more tightly, the debate, to the extent the differences are genuine, might have been advanced more than I feel it has.

I hope that I may be allowed one final example to make this point. Professor Fain believes that because I illustrated my contention that the type of freedom is related to the type of constraint in a single paragraph, I do not see the important distinctions between the various types of freedom. I had not anticipated this inference. Let me hasten to add that I believe that the differences between a ball's freedom to roll and a human's freedom to act morally are even greater than they are obvious. I do not doubt that Professor Fain believes that he has made a telling rejoinder to my position. I can only respond that, however adequate his subjective understanding of that effort, communication has failed, surely to my loss and perhaps to his as well.

MORTON A. KAPLAN

University of Chicago

EDITORIAL COMMENT

In this Corner. In the course of preparing his contribution to this issue, Professor Philip Converse has had some hard words to say about the general idea of the scholarly symposium, regretting that too often these turn into a schoolyard scrap, which ends up merely bruising participants and entertaining—but not enlightening—onlookers. This is certainly one way to look at the matter, but perhaps it is not the only way. There are such things, for example, as civilized conversations, or honest differences of viewpoint, or correctable errors. The juxtaposition of different scholarly treatments of a single theme might easily stimulate this sort of description as well.

The publication of a scholarly article or book is, after all, as the term suggests, a public act. It is intended for use in the community of scholars; it is, the author must hope, a contribution. It is sometimes the case that the ideas we send out into the world in this way are misunderstood, or found fault with, or are used in some unanticipated way. When this happens, we may without impropriety abandon them to their fate, and get on with our new interests.

Or, alternatively, we may accept continuing responsibility for our earlier work, and conceivably thereby add a little more to the general understanding of the ideas that work addresses. In our view, a willingness to enter into a discussion of this kind is an act of responsibility, and of sober citizenship in the scholarly community. Of course such an act can be turned into an ego trip, or be so misconstrued by perennial sophomores, but our opinion is that these are chances worth taking in the interests of genuine interchange among serious students.

Thus we applaud Professor Converse's willingness to put his misgivings aside in order to address his colleagues in this format on a matter of some scholarly significance.

New Math. Where does the APSR stand on the issue of mathematics in political science? Nowhere. If political scientists find it helpful to use numbers, or algebraic symbols, or mathematical operations in doing their work, the chances are that articles reflecting these tastes will appear in the *Review*. Insofar as political scientists go off on other tracks, and make their statements without mathematics, we assume we will reflect that too.

Many observers have noticed, however, that as time has gone on over the last few years literacy in mathematics has been more and more necessary for readers to travel comfortably from one cover of the *Review* through to the other. It is now possible to supplement this purely literary sentiment with hard empirical facts. What follows is a portion of a memorandum prepared by Professor William Riker of the University of Rochester:

"[Political Science] has indeed been somewhat mathematicized in the last decade. In Table I

Table 1. Distribution of Papers in American Political Science Review by Categories Concerning the Use of Numbers and Mathematics

	Kind of Paper in Percents (rounded)					
	Verbally Interpreted Numbers	Numerical		Non-Numerical	Total	*N*
		Statistical Interpretation	Mathematical			
1964	29%	9%	3%	59%	100%	35
1965	28%	20%	0	52%	100%	40
1966	32%	25%	7%	36%	100%	44
1967	23%	24%	2%	51%	100%	55
1968	38%	32%	15%	15%	100%	40
1969	10%	45%	12%	33%	100%	51
1970	13%	52%	10%	25%	100%	52
1971	13%	38%	21%	28%	100%	56
1972	12%	36%	13%	39%	100%	52
1973 (2 issues)	—	50%	17%	33%	100%	24
Total	20%	33%	10%	37%	100%	449
N	(88)	(150)	(45)	(166)	(449)	

is reported a categorization of papers in the *American Political Science Review*, the most prestigious journal in the field, over this period. There are three categories of papers using some kind of numerical evidence:

1. Numerical. Here the raw numbers are subjected to mostly verbal interpretation, perhaps with the use of the most elementary kind of statistics such as means and percentages.
2. Statistical. Here the numbers are interpreted by some kind of statistical device more complicated than, say, means.
3. Mathematical Here there is some sort of deductive development of applied mathematics.

Finally there is a fourth category of all papers not using numerical or mathematical methods. This includes everything from the loosest kind of history writing to the tightest kind of philosophizing.

As is apparent from this table, there has been a notable shift from using literarily interpreted numerical evidence to the use of statistical methods. In 1964 about thirty per cent of the papers used numerical evidence and about ten per cent used statistical evidence. In 1972 these proportions were about reversed. Furthermore, the proportion of non-numerical articles had fallen from about sixty per cent in 1964 to about thirty per cent in the years around 1972. One can genuinely speak of the conversion of at least the best journal in the field from being numerically naive to being somewhat sophisticated in formal methods."

There is always the possibility that letting information like this get around will be bad for business, that somebody will identify too strongly with one or another entry on Professor Riker's table, and go away unjustifiably happy or sad. On the other hand one way to live up to our obligation to go about our work without fear or favor is to pass information like this along when we can, and hope for the best.

Nothing in the table should be interpreted as constituting a determination on our part to print or to suppress any kind of article. If the figures show the APSR has gotten more mathematical over the years, this ought to be interpreted as telling us what's on political scientists' minds these days.

On Deadlines. Occasionally readers tell us that the *Review* does not reach them in the same month as the date indicated on the cover. Many scholarly journals from time to time suffer from this difficulty, but in recent years the *Review* has on the whole made its deadlines and arrived promptly at the doorsteps of political scientists all over the world. Maintaining this record takes no small effort. It entails the coordinated activity of our manuscript editor, proofreader, and book review editorial assistant here in this office, plus the cooperation of our far-flung authors and book reviewers, who must attend to their galley proofs on schedule. The whole thing is supervised by the assistant to the managing editor, Betsey Cobb, in association with our friends at the George Banta Company. Last December, so it seems, Banta made the supreme sacrifice, and got our December issue out almost on schedule. It was the middle of January, however, before we received a Christmas card from the Banta Company. Greater love hath no printer, we think.

Articles Accepted for Future Publication

Neal Andrews, Wayne State University, "Integration and Community in Communist Theory"

Paul Allen Beck, University of Pittsburgh, "Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior"

Samuel H. Beer, Harvard University, "Tradition and Nationality: A Review Essay"

Robert A. Bernstein and William W. Anthony, Texas A&M University, "The ABM Issue in the State, 1968–1970: The Importance of Ideology"

Gordon S. Black, University of Rochester, "Conflict in the Community: A Theory of the Effects of Community Size"

Bruce Bueno de Mesquita, University of Rochester, "Need for Achievement and Competitiveness and Determinants of Party Success in Elections and Coalitions"

Walter Dean Burnham, Massachusetts Institute of Technology, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate' "

Edward G. Carmines, State University of New York, Buffalo, "The Mediating Influence of State Legislatures on the Interparty Competition-Welfare Expenditures Linkage"

Richard Allen Chapman, University of Montana, "*Leviathan* Writ Small: Thomas Hobbes on the Family"

John P. Clark, III, City College, Loyola University, "On Anarchism in an Unreal World: Kramnick's Views of Godwin and the Anarchists"

Claude S. Colantoni, Terrence J. Levesque and Peter C. Ordeshook, Carnegie-Mellon University, "Campaign Resource Allocations Under the Electoral College"

Wayne A. Cornelius, Massachusetts Institute of Technology, "Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities"

Douglas Dobson, Northern Illinois University and Douglas St. Angelo, Florida State University, "Party Identification and the Floating Vote: Some Dynamics"

Lawrence C. Dodd, University of Texas, "Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis"

Dennis L. Dresang, University of Wisconsin, Madison, "Ethnic Politics, Representative Bureaucracy, and Development Administration: The Zambian Case"

Claude S. Fischer, University of California, Berkeley, "The City and Political Psychology"

G. David Garson, Tufts University, "On the Origins of Interest Group Theory: A Critique of a Process"

Mark Gavre, University of California, Los Angeles, "Hobbes and His Audience: The Dynamics of Theorizing"

Sheldon Goldman, University of Massachusetts, Amherst, "Voting Behavior on the U.S. Courts of Appeals Revisited"

Ted Robert Gurr, Northwestern University, "Persistence and Change in Political Systems, 1800–1971"

Larry B. Hill, University of Oklahoma, "Institutionalization, the Ombudsman, and Bureaucracy"

Richard Child Hill, Michigan State University, "Separate and Unequal: Governmental Inequality in the Metropolis"

Kenneth Jowitt, University of California, Berkeley, "An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems"

Jae-On Kim, University of Iowa, John R. Petrocik, University of Chicago and Stephen N. Enokson, University of Iowa, "Voter Turnout Among the American States: Systemic and Individualistic Components"

David Koehler, "Vote-Trading and the Voting Paradox: A Proof of Equivalence"

Walter Korpi, University of Stockholm, "Conflict, Power and Relative Deprivation"

J. A. Laponce, University of British Columbia, "Prolegomenon to the Study of Spatial Archetypes and Political Perceptions"

Peter M. Leslie, Queen's University, "Interest Groups and Political Integration: The 1972 EEC Decisions in Norway and Denmark"

Jesse F. Marquette, University of Akron, "Social Change and Political Mobilization in the United States: 1870–1960"

Alan Marsh, Social Science Research Council, "The 'Silent Revolution,' Value Priorities, and the Quality of Life in Britain"

Arthur H. Miller, Ohio State University, "Political Issues and Trust in Government: 1864–1970"

Thomas C. Nowak and Kay A Snyder, Michigan State University, "Clientelist Politics in the Philippines: Integration or Instability?"

Stanton Peele, Harvard University and Stanley J. Morse, Pontifica Universidade Catolica de Sao Paulo, "Ethnic Voting and Political Change in South Africa".

N. Patrick Peritore, University of Missouri, "Some Problems in Alfred Schutz's Phenomenological Methodology"

David E. Price, Duke University, "Community Control: Critical Democratic Theory in the Progressive Period"

Adam Przeworski, University of Chicago, "Institutionalization of Voting Patterns or Is Mobilization the Source of Decay?"

Douglas Rae, Yale University, "The Limits of Consensual Decision"

Joseph A. Schlesinger, Michigan State University, "The Primary Goals of Political Parties: A Clarification of Positive Theory"

Brian D. Silver, Florida State University, "Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis"

Peter G. Stillman, Vassar College, "Hegel's Critique of Liberal Rights"

C. Neal Tate, North Texas State University, "Individual and Contextual Variables in British Voting Behavior: An Exploratory Note"

Kent L. Tedin, College of William and Mary, "The Influence of Parents on the Political Attitudes of New Voters"

John Wanat, University of Kentucky, "Bases of Budgetary Incrementalism"

Meredith W. Watts, University of Wisconsin, Milwaukee, "B. F. Skinner and the Language of Technological Control"

Herbert Weisberg, University of Michigan, "Models of Statistical Relationship"

Louis P. Westefield, Southern Illinois University, Edwardsville, "Majority Party Leadership and the Committee System in the House of Representatives"

Diplomacy for the 70's: An Afterview and Appraisal*

WILLIAM I. BACCHUS

Commission on the Organization of the Government for the Conduct of Foreign Policy

More than twenty years ago in an earlier review article in this journal, Arthur Macmahon directed attention to the "structural problem" of post-World War II foreign affairs management in the United States.[1] This problem had—and has—two basic elements: (1) to what extent should activities that have "a primary bearing on foreign affairs" be located within the Department of State, or if not located there, where should they be located; and (2) how should those activities which are placed outside State be coordinated, and by whom?[2] Diverse answers have been proposed in the intervening years to these questions of State's appropriate relationship to the rest of government, and to two others which logically depend upon them—how the State Department and the other foreign affairs agencies should be organized and managed, and how they should be staffed. Proposals and recommendations have come from many individuals,[3] and from numerous official and quasi-official bodies,[4] but there has been a general failure to find lasting solutions. In view of an increasingly evident need for improvement, however, these failures have served only to generate still other reform programs.

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* I would like to express my appreciation to James W. Fesler, Laurin Henry, Frederick C. Mosher, and Frederick D. Elfers, as well as to the anonymous readers solicited by this Review, for their helpful comments on earlier drafts. I would also like to thank those officials of the Department of State who so frankly shared their opinions with me. Except for editorial changes and minor updating, this review was completed prior to my joining the staff of the Commission on the Organization of the Government for the Conduct of Foreign Policy. The opinions reflected herein are entirely my own.

[1] Arthur W. Macmahon, "The Administration of Foreign Affairs," *American Political Science Review*, 45 (September, 1951), 836–866.

[2] *Ibid.*, p. 838.

[3] Among the many individual proposals and analyses, some of the most prominent have been: James L. McCamy, *The Administration of American Foreign Affairs* (New York: Knopf, 1952); Arthur W. Macmahon, *Administration in Foreign Affairs* (Birmingham: University of Alabama Press, 1953); Robert E. Elder, *The Policy Machine: The Department of State and American Foreign Policy* (Syracuse: Syracuse University Press, 1960); Ellis Briggs, *Farewell to Foggy Bottom* (New York: McKay, 1964); James L. McCamy, *The Conduct of the New Diplomacy* (New York: Harper and Row, 1964); Burton M. Sapin, *The Making of U.S. Foreign Policy* (Washington, D.C.; The Brookings Institution, 1966); Smith Simpson, *Anatomy of the State Department* (New York: Houghton-Mifflin, 1967); Ellis Briggs, *Anatomy of Diplomacy: The Origin and Execution of American Foreign Policy* (New York: McKay, 1968); and James C. Thomson, Jr., "How Could Vietnam Happen? An Autopsy," *Atlantic Monthly*, 221 (April, 1968), 47–53. More recently, see Lannon Walker, "Our Foreign Affairs Machinery: Time for an Overhaul," *Foreign Affairs*, 47 (January, 1969), 309–320; John Franklin Campbell, *The Foreign Affairs Fudge Factory* (New York: Basic Books, 1971); and I. M. Drestler, *Presidents, Bureaucrats, and Foreign Policy* (Princeton: Princeton University Press, 1972). See also Dean Acheson, "The Eclipse of the State Department," *Foreign Affairs*, 49 (July, 1971), 593–606.

[4] Official and quasi-official studies and proposals have been prepared by:

[a] The Bureau of the Budget at the request of Secretary of State James F. Byrnes. U.S. Executive Office of the President, Bureau of the Budget, "The Organization and Administration of the Department of State," August 15, 1945 (Unpublished memorandum).

[b] A group of Foreign Service Officers within State, headed by Selden Chapin. Two unpublished memoranda were produced: Andrew B. Foster, "Examination of the Proposal to Combine the Foreign Service and the Departmental Service," Memorandum to Selden Chapin, Office for the Foreign Service, September 14, 1945; and Selden Chapin, "A Plan for a Single Service Under the Secretary of State," Memorandum to Donald S. Russell, Assistant Secretary of State for Administration, October 31, 1945.

[c] Chapin and his associates on one hand, and the House Committee on Foreign Affairs on the other, leading to the very important Foreign Service Act of 1946. See U.S. Congress, House Committee on Foreign Affairs, *Reorganization of the Foreign Service* (Washington, D.C.: GPO, July 12, 1946), and for an excellent case study, Harold Stein, *The Foreign Service Act of 1946* (New York: Committee on Public Administration Cases, 1949), which is also contained in *Public Administration and Policy Development*, ed. Harold Stein (New York: Harcourt, Brace and World, 1952), pp. 661–737.

[d] Assistant Secretary of State for Administration John E. Peurifoy, who prepared a comprehensive program in 1948. It was transmitted as "Reorganization of the State Department," Memordandum to Secretary of State George C. Marshall, May 6, 1948. Discussion of this, of the other early postwar proposals and studies, and of some prewar reform programs is contained in U.S. Department of State, *Facts and Issues Relating to the Amalgamation of the Department of State and the Foreign Service*, A study prepared for the Secretary's Advisory Group on Amalgamation (Washington, D.C.: Department of State, December, 1949).

[e] The first Hoover Commission, appointed jointly by the Congress and the President. See *Foreign Affairs: A Report to the Congress by the Commission on Organization of the Executive Branch of the Government* (Washington, D.C.: GPO, 1949), and particularly the Task Force on Foreign Affairs Report, prepared by a working group headed by former Assistant Secretary of State Harvey H. Bundy, which is included as Appendix H. A 1949 reorganization of State placed some of the Hoover Commission recommendations into effect.

[f] The Rowe Committee (The Secretary's Advisory Committee on Personnel), appointed by Secretary of State Acheson and consisting of both "outsiders" and "insiders," appointed to investigate whether sweeping personnel changes urged by the Hoover Commission were necessary. See *An Improved Personnel System for the Conduct of Foreign Affairs,* A report to the Secretary of State by the Secretary's Advisory Committee on Personnel (Washington, D.C.: Department of State, August, 1950). A directive partially following the Rowe Committee proposals was later issued but had limited impact. See *Directive to Improve the Personnel Program of the Department of State and the Unified Foreign Service of the United States* (Washington, D.C.: Department of State, March, 1951).

[g] The Brookings Institution, under contract from the Bureau of the Budget. See Brookings Institution, *The Administration of Foreign Affairs and Overseas Operations,* A Report prepared for the Bureau of the Budget, Executive Office of the President (Washington, D.C.: GPO, 1951). There is no indication this report had much operational impact.

[h] A study group of the Woodrow Wilson Foundation. See *United States Foreign Policy: Its Organization and Control,* Report of a Study Group for the Woodrow Wilson Foundation, William Yandell Elliott, Chairman (New York: Columbia University Press, 1952).

[i] The Wriston Committee (The Public Committee on Personnel), a blue-ribbon group of "outsiders" chaired by Henry M. Wriston, President of Brown University and appointed in March, 1954, by Acting Secretary of State Walter Bedell Smith. The report is *Toward A Stronger Foreign Service,* Report of the Secretary of State's Public Committee on Personnel, Department of State Publication 5458 (Washington, D.C.: GPO, June, 1954). Substantial action was taken on these recommendations, particularly with respect to integration of Foreign Service Departmental personnel into one system. Those who entered the Foreign Service under this program have been designated "Wristonees" ever since.

[j] A small group of personnel specialists drawn from the foreign affairs agencies under the direction of the President's Advisor on Personnel Management, Philip Young, and headed by Henry DuFlon, Young's deputy. This White House task force report entitled "A Foreign Affairs Personnel System," of October 28, 1954, was not published and did not receive wide circulation in government. It was apparently bypassed in part by the action already being taken on the Wriston Report, although it was concerned with all civilian personnel overseas, and not just those in State, as was the Wriston Report.

[k] A group from Syracuse University, at the request of the Senate Foreign Relations Committee, which dealt primarily with overseas operational aspects of foreign affairs. See *The Operational Aspects of United States Foreign Policy,* Study prepared at the request of the Senate Committee on Foreign Relations, by the Maxwell Graduate School of Citizenship and Public Affairs, Syracuse University (Washington, D.C.: GPO, November 11, 1959).

[l] A second Brookings Institution group, also at the request of the Foreign Relations Committee. See H. Field Haviland, Jr. et al., *The Formulation and Administration of United States Foreign Policy,* A Report for the Committee on Foreign Relations of the United States Senate (Washington, D.C.: Brookings, 1960).

[m] The "Jackson Subcommittee" named for its Chairman, Senator Henry M. Jackson, and functioning under various names as a Subcommittee of the Senate Government Operations Committee. An exceptional series of hearings, reform proposals, and studies. Their work up to 1961 is collected in U.S. Subcommittee on National Policy Machinery, Committee on Government Operations, *Organizing for National Security* (Washington, D.C.: GPO, 1961), 3 volumes; through 1964 in U.S. Senate Subcommittee on National Security Staffing and Operations, Government Operations Committee, *Administration of National Security* (Washington, D.C.: GPO, 1965); and since that time in a series of hearings and studies, as the Subcommittee on National Security and International Operations, on such topics as the applicability of programming in foreign affairs, coordination, and specific details of operating programs. Included in Vol. I (pp. 942–1001) of *Organizing for National Security* is Nelson Rockefeller's proposal that a central position, "A First Secretary of Government" outside the line of command, be established to bring order to the multitude of programs and agencies. The Subcommittee's staff report, "Basic Issues," in *Administration of National Security, pp. 7–26,* is an excellent summary of many of the basic themes and issues which had developed through all the previous studies.

[n] The "Herter Committee" (Committee on Foreign Affairs Personnel), composed of experienced "outsiders," and established at the request of Secretary of State Dean Rusk, under the auspices of the Carnegie Endowment for International Peace, with foundation financial support. See *Personnel for the New Diplomacy,* Report of the Committee on Foreign Affairs Personnel (Washington, D.C.: Carnegie Endowment for International Peace, December, 1962), and the series of six Foreign Affairs Personnel Studies supplementing the basic report which grew out of the work of the Committee Staff under the direction of Frederick C. Mosher: Arthur G. Jones, *The Evolution of Personnel Systems for U.S. Foreign Affairs: A History of Reform Efforts* (No. 1); Robert E. Elder, *Overseas Representation and Services for Federal Domestic Agencies* (No. 2); John E. Harr, *The Development of Careers in the Foreign Service* (No. 3); John E. Harr, *The Anatomy of the Foreign Service—A Statistical Profile* (No. 4); Regis Walther, *Orientations and Behavior Styles of Foreign Service Officers* (No. 5); and Frances Fielder and Godfrey Harris, *The Quest for Foreign Affairs Officers—Their Recruitment and Selection* (No. 6). All except No. 6, which is available only in multilith, were published by the Carnegie Endowment in 1965. The very comprehensive Herter Committee Report was an important source of later proposals, but relatively little that it recommended actually became effective.

[o] Assistant Secretary, later Deputy Under Secretary of State for Administration, William J. Crockett, who during his time at State between 1961 and 1967 initiated a whole range of studies and management reforms which remain the subject of continuing contro-

A number of responses are possible to the obvious question of why rationalization of foreign affairs organization has been so difficult to achieve, and why with few exceptions reform attempts have not had much success. Some can be ascertained by examining one recent reform program, the history of which underscores many of the difficulties common to such efforts. Unfortunately, this program, summarized in *Diplomacy for the 70's: A Program of Management Reform for the Department of State,*[5] never satisfactorily confronted the "structural problem" cited by Macmahon, as to a degree it could not. But even granting its more restricted aims, and rather bright initial prospects, it had only limited success.

One hopeful sign was the blunt approach taken by Deputy Under Secretary of State for Administration (later Management) William B. Macomber in introducing the program on January 14, 1970. He pulled few punches about the "Sad State of State."[6] Speaking of its attempts to deal with its postwar tasks, he argued that

> the key fact is that as an institution, despite many brilliant performances along the way, we have not met the challenge of foreign affairs leadership as successfully as we might have. Our failure to do so has caused frustration. And it has raised a clear prospect: either we produce the improvements necessary to meet this challenge, or . . . this will be done for us.[7]

Macomber's statement seems to have rested on the important assumptions that internal reforms within the department would help it regain foreign affairs leadership, that reform was a viable possibility, and that State could reform itself by its own efforts. Many observers would dispute each of these foundations of the program. In the first place, the whole postwar conduct of American foreign relations has been characterized by a proliferation of activities and agencies, and by an increasing tendency toward centralization of the really important decisions at the White House. All presidents have, at least implicitly, encouraged these trends, and even the subsequent appointment of Henry Kissinger as Secretary of State gave scant evidence that devolution back to State as an institution was likely, even if the department's own performance improved. Moreover, the increasingly technical and operational nature of many programs runs counter to the generalist, essentially politically oriented mentality of many in State, who have been at best ambivalent about (and ill-equipped for) exercising real control over such matters as foreign assistance, information programs, clandestine intelligence functions, and monetary and economic policy, not to mention

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versy. The best official summary is *A Management Program for the Department of State, Office of the Deputy Under Secretary for Administration* (Washington, D.C.: Department of State, August, 1966), and a good short commentary on the premises underlying the Crockett programs is Alfred J. Marrow, "Managerial Revolution in the State Department," *Department of State News Letter,* No. 68 (December, 1966), 34–37.

[p] General Maxwell Taylor and four interagency task forces, who on assignment from President Johnson, developed a State Department-centered government-wide coordination system, commonly identified as the NSAM 341 system, after the National Security Action Memorandum which established it on March 4, 1966. The texts of the task force reports have not been published, but the essential text of NSAM 341, which is classified, can be found in Jackson Subcommittee (Subcommittee on National Security and Operations), *The Secretary of State and the Problem of Coordination: New Duties and Procedures of March 4, 1966* (Washington, D.C.: GPO, 1966). Taylor, however, explained some of the thinking behind the new arrangements in a speech to the American Foreign Service Association on March 31, 1966, later reprinted as "New System for Coping With Our Overseas Problems," *Foreign Service Journal,* 43 (May, 1966), 34–36. Cf. my "Obstacles to Reform in Foreign Affairs: The Case of NSAM 341," *Orbis,* 18 (Spring, 1947).

[q] The Heineman Task Force (President's Task Force on Government Organization), which advocated a strong role for the Secretary of State, in an unpublished report submitted on October 1, 1967.

[r] A Committee of the American Foreign Service Association chaired by Ambassador Graham Martin. This detailed study apparently had some limited influence on the developments discussed in this review. The report is published as *Toward A Modern Diplomacy,* A Report to the American Foreign Service Association by its Committee on Career Principles (Washington: AFSA, 1968), and also appears in *Foreign Service Journal,* 45 (November, 1968, Part II).

[s] A working group of the Institute for Defense Analysis, which in 1968 prepared a report with foundation financing for the use of the next president. It was later published as *The President and the Management of National Security,* ed. Keith C. Clark and Laurence J. Legere (New York: Praeger, 1969).

In developing this listing, Arthur G. Jones, *The Evolution of Personnel Systems,* I. M. Destler, *Presidents, Bureaucrats, and Foreign Policy,* and Harris H. Ball, Jr., "An Examination of the Major Efforts for Organizational Effectiveness in the Department of State from 1924 to 1971" (M.A. Thesis, School of Government and Business Administration, George Washington University, September, 1971), have been particularly helpful.

[5] U.S. Department of State, *Diplomacy for the 70's: A Program of Management Reform for the Department of State,* Department of State Publication 8593, Department and Foreign Service Series, 1430 (Washington, D.C.: GPO, 1970). Cited hereafter as *D 70.*

[6] The phrase in quotations is Stewart Alsop's, used as a chapter title in his *The Center: People and Power in Political Washington* (New York: Harper and Row, 1968).

[7] William B. Macomber, Address, Washington, D.C., January 14, 1970. Reprinted in *D 70,* pp. 587–605. Quoted passage from p. 589.

the international elements of the programs of domestic agencies and departments. This preference for confining State to "policy" matters while others conducted "operations" paralleled the wishes of many in Congress and suited Secretaries of State Acheson and Dulles, but it inevitably limited and continues to limit State's potential for leadership.[8] It is conceivable that coordination mechanisms could be developed which would give the department policy leadership under these circumstances, if so willed by a president, but they have not emerged to date. Thus Macomber's plea for leadership was based more on hopes than on hard evidence that what he desired was possible.

With respect to the assumption that the State Department could reform itself, circumstances seemed at best mixed. Some support had at last been developing within the department for coming to grips with the problems Macomber acknowledged, which could perhaps be mobilized to support *Diplomacy for the 70's*. At the same time, the past record hardly allowed great confidence that the predominant mood in State would be particularly receptive to attempts to overcome some of the organizational and personnel performance weaknesses of the department. The "informal culture" of the Foreign Service has frequently been cited as one of "the causes of organizational ineffectiveness" in State,[9] and the rather dismal record of many of the previous reform efforts cited earlier can be ascribed partially to internal opposition. While this opposition might be overcome, it almost certainly would not be an easy task. In sum, severe obstacles threatened the major assumptions of the Macomber program.

Procedurally, the development of the program was straightforward. Macomber and then Under Secretary Elliot Richardson, passively supported by Secretary William Rogers, created thirteen task forces to consider problems outlined in Macomber's speech.[10] Altogether, more than 250 foreign affairs professionals drawn from State and related agencies developed some 500 recommendations before concluding their work in the summer of 1970. Their reports were then discussed throughout State, collated and assigned priorities by a staff working under Macomber. In December, the task force reports were published in *Diplomacy for the 70's* and, shortly thereafter, the "action stage" began.[11] By September 1972, 36 Management Reform Bulletins had announced steps taken to place some 166 of the 505 recommendations into effect, and earlier in the year Macomber asserted that some action had been taken on more than 400 of them.[12]

But neither this brief chronology nor the numerical "results" demonstrate the nature of the reform process or its true impact. Both are essential for understanding problems besetting would-be innovators in the foreign affairs bureaucracy.

The Process of Reform

The major previous foreign affairs reform studies were undertaken by outsiders or internal management staffs, and approved suggestions imposed upon the organization. Macomber acknowledged the value of some of these earlier exercises, but chose not to act by fiat:

> . . . our success will be more significant and lasting if those most directly affected are involved in the creative and implementing process and are convinced of the wisdom of what is proposed.[13]

This sounds remarkably similar to the "participation hypothesis" advanced by the human relations movement more than fifty years ago.[14] Subordinate participation can serve leadership by demonstrating organizational democracy, allowing wider consideration of possible changes, and indicating what is feasible. It can facilitate delegation of decision making, educate and inform subordinates, and help gain their understanding and perhaps, enthusiasm and support. Finally, it can identify talented individuals qualified for greater responsibilities. But there are also dangers. Participatory reform may be more time-consuming, and controversial initiatives may be more easily thwarted. The executive may appear weak because he does not use his full prerogatives. The technical nature of problems may make participation frustrating, and it may leave leadership tactically weak if it is unwilling or unable to carry through on proposals developed through open processes.[15]

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[8] The Kennedy Administration initially attempted to break down this policy-operations dichotomy and to restore coordinating authority to State, but the effort can hardly be termed successful. See on this point the letter from McGeorge Bundy to Senator Henry M. Jackson, September 4, 1961, in *Administration of National Security* (See item m, note 4) and in *The National Security Council*, ed. Henry M. Jackson (New York: Praeger, 1965), p. 278.

[9] For a typical discussion of State's "informal culture," see Andrew M. Scott, "The Department of State: Formal Organization and Informal Culture," *International Studies Quarterly*, 13 (March, 1969), 1–18. The organizational ineffectiveness theme is presented most cogently in Chris Argyris, *Some Causes of Organizational Ineffectiveness Within the Department of State*, Center for International Systems Research, Department of State, Occasional Paper No. 2 (Washington, D.C.: GPO, 1967). Both themes are also treated in Robert L. Rothstein, *Planning Prediction, and Policymaking in Foreign Affairs* (Boston: Little, Brown and Co., 1972), chap. 2. See also Donald P. Warwick, "Bureaucratization in the Government Agency: The Case of the U.S. State Department," *Sociological Inquiry*, 44 (Spring, 1974).

[10] The task forces and their assignments were: I—Career Management and Assignment Policies Under Functional Specialization; II—Performance Appraisal and Promotion Policies; III—Personnel Requirements and Resources; IV—Personnel Training for the Department of State; V—Personnel Perquisites: Nonsalary Compensations and Allowances; VI—Recruitment and Employment; VII—Stimulation of Creativity; VIII—Role of the Country Director; IX—Openness in the Foreign Affairs Community; X—Reorganization of the Foreign Service Institute; XI—Roles and Functions of Diplomatic Missions; XII—Management Evaluation System; XIII—Management Tools. The task force reports are reprinted in *Democracy for the 70's*.

[11] Announced in "A Schedule for Implementing the Recommendations of the Department of State Task Forces on Management Reform," *Department of State News Letter*, No. 117 (January, 1971), 20–43.

[12] William B. Macomber, Address, Washington, D.C., January 26, 1972. Reprinted in Department of State Press Release No. 22 of 1972.

[13] Macomber, Address, January 14, 1970. Quoted passage from *D 70*, p. 588.

For State's leaders, faced with pressure for reform from Congress and the administration, the advantages of participation were enticing. The previous record made them aware of the difficulty of imposing change on a skeptical Foreign Service. Outside studies and programs had not accomplished much, and incorporating the practical experience of foreign affairs professionals might make reforms more viable. Also, the growing ferment in parts of the Foreign Service could perhaps be tapped. This was most notably seen in the activities of the American Foreign Service Association (AFSA), which after its capture by a group of "Young Turks" in 1967 had produced its own substantial reform program in 1968.[16] Participation might turn such dissent into an asset.[17] In addition, State's declining prestige and a growing realization that action was needed made it seem plausible that most opposition could be neutralized. Finally, a participatory program would be more visible than one developed by closed means, and might help signal that State was at last prepared for a more important role. Some in the Department apparently thought the program, if successful, would help them resist the incursions of Henry Kissinger's National Security Council operation, while others, more skeptical, hoped it might at least encourage a post-Kissinger president to give State a chance to become more important.

Yet there may have been more desire for the illusion of participation than for its reality. Critics have always argued that inclusion of subordinates in organizational decision making can actually facilitate autocratic rule by coopting potential critics and manipulating opinion to leadership's advantage. Participants may believe they have an important role, while in fact they have no influence on outcomes. *Diplomacy for the 70's*, motivated in part by a sincere desire for additional input, can be criticized as embodying "pseudo-participation"[18] on several grounds.

First, Macomber's speech made it clear that change had already been decided upon. The role of task force members was to develop details within the framework they were given. Second, even this degree of participation effectively stopped once the task force reports were completed, although they were subjected to continuing discussion. The Organization and Management Staff, after consultation with sixth- and seventh-floor officials (these shorthand terms are used in State to refer to the Assistant Secretaries; and to the Secretary, Deputy Secretary, and Under Secretaries respectively, based on the location of their offices in the State Department building), selected the proposals to be acted upon and assigned priorities to them. Third, for most of those affected, the impact of the program was the same as if it had been imposed, since only a few were involved or consulted. Finally, even this limited participation ran counter to the substance of many recommendations chosen for action. A major feature of the program was an attempt to centralize decision making and control, reversing the trend of some previous reforms. There were cogent reasons for strengthening top officials—the Assistant Secretaries and the Department's principals clearly needed help—but it hardly encouraged participatory conduct of State's business.

It would be unfair to leave the development of *Diplomacy for the 70's* without acknowledging the efforts of many of those involved. The receptiveness of task force members to varied ideas and opinions was noteworthy. In addition to their own studies and discussions, all task forces called upon a variety of witnesses and apparently seriously considered all material presented. If their reports sometimes suffered from the disease of lowest common denominator consensus, collectively they help refute the frequent assertion that nothing good can come from committees. Similarly, the enthusiasm and drive of the small group charged with making the program work was impressive, and their accomplishments were considerable in the face of limited resources, vacillating support, and obstacles beyond their control.

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[14] See Frederick C. Mosher, *Government Reorganizations: Cases and Commentary*, ed. Mosher (Indianapolis: Bobbs-Merrill, 1967), introduction p. xvii, for discussion of this hypothesis.

[15] *Ibid.*, part II, chap. 3, "Participation and Reorganization," pp. 515–537.

[16] *Toward a Modern Diplomacy.* Walker, "Our Foreign Affairs Machinery: Time for an Overhaul," gives the general outlines of the new AFSA leaders' thinking.

[17] AFSA's board chairman, Charles W. Bray III, was quoted as welcoming the Macomber Program when it was announced, calling it "a major step forward." A. D. Horne, "Foreign Service Revamping Unveiled," *Washington Post*, January 15, 1970, p. A27.

[18] The term in quotations is Mosher's, from his "Introduction," in *Governmental Reorganizations*, p. xvii.

Recommendations for Change

The proposals themselves can be grouped into categories of openness and creativity, management improvement, and personnel system reform. Some task forces dealt peripherally with the questions of interagency coordination and governmentwide leadership, but virtually everything later approved for action was directed inward. Thus at the outset there was a failure to address the primary cause of much of State's difficulty: the confusion existing ever since World War II about what State is really supposed to do.[19] Unless and until the department's relationships with other agencies and departments and with actual and potential competitors for foreign affairs leadership below the president, such as the NSC Staff, the Council on International Economic Policy (CIEP), and the International and National Security Programs Divisions of the Office of Management and Budget (OMB), can be clarified, including whether it is any longer possible for State to "lead," the questions of how State should be organized and the type of personnel system it requires will remain unanswerable. The reluctance to confront this larger problem undoubtedly reflected a feeling of helplessness in this area, and no mandate to do so. But, unable to deal with the prior question of its own role, yet faced with strong reform pressures, the department embarked on its management and personnel program anyhow. Its leaders spoke of the need to reform in order to be able to reassert leadership, but what they advocated seemed to ignore substantial evidence that rationalization of procedures and analytic techniques are not enough, if the objective is to gain influence in the larger foreign affairs community.[20]

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[19] The themes of State's organization, personnel system reforms, and overall foreign affairs relationships have dominated the reform programs and proposals cited in note 4. While most have devoted some attention to all three, this is not completely true, as the following summary indicates:

Topics Treated in Previous Foreign Affairs Reform Studies and Programs

Study	State Department Organization	Overall Relationships	Personnel System Modifications
(a) Bureau of Budget [1945]	x	–	x
(b) Chapin-Foster [1945]	–	–	x
(c) Foreign Service Act [1946]	–	–	x
(d) Peurifoy Proposals [1948]	x	–	x
(e) Hoover Commission [1949]	x	x	x
(f) Rowe Committee [1950]	–	–	x
(g) 1st Brookings Study [1951]	x	x	x
(h) Woodrow Wilson [1952]	x	x	–
(i) Wriston Committee [1954]	–	–	x
(j) DuFlon Task Force [1954]	–	–	x
(k) Syracuse (Maxwell) [1959]	–	x	x
(l) 2nd Brookings Study [1959]	x	x	x
(m) Jackson Subcommittee [1958–]	x	x	x
(n) Herter Committee [1962]	x	x	x
(o) Crockett Programs [1961–67]	x	–	x
(p) Taylor (NSAM 341) [1966]	–	x	–
(q) Heineman [1967]	–	x	–
(r) AFSA [1968]	x	x	x
(s) IDA [1968]	x	x	–
– Democracy for the 70's (Macomber) [1970–72]	x	–	x

[20] See, for example, Destler, *Presidents, Bureaucrats, and Foreign Policy;* Roger Hilsman, *The Politics of Policy Making in Defense and Foreign Affairs* (New York: Harper and Row, 1971); Graham T. Allison, "Conceptual Models and the Cuban Missile Crisis," *American Political Science Review,* 63 (September, 1969), 689–718; Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown, 1971); Graham T. Allison and Morton Halperin, "Bureaucratic Politics: A Paradigm and Some Policy Implications," *World Politics* 24 (Supplement,

Turning to specifics, the labors of the two task forces charged with investigating obstacles to openness and creativity in State and the Foreign Service perhaps were most interesting. Their assignments indicated a new top-level awareness of the department's deficiencies, and represented a marked change in opinion. Chris Argyris's widely read and controversial discussion of causes of ineffectiveness in State had focused attention on these topics in 1967,[21] but his negative conclusions for the most part had been condemned or ignored. Openness had been a key aim of ASFA's 1968 reform proposals, but Macomber's speech was the first explicit official acknowledgment of such difficulties.

Unfortunately, positively valuing the linked abstract notions of creativity ("the ability to produce new ideas"[22]) and openness ("a state of mind" leading to open channels of communication, the opposite of "parochial insularity," "narrowness," and "particularism"[23]) did not prove to be the same thing as instilling them in practice. Creative thinking was seen as partially the by-product of openness. Thus both task forces favored greater use of *ad hoc* bodies staffed by personnel of all ranks, hoping this would loosen constraints of rank and hierarchy and result in more independent expression. Similarly, easing access to top levels for new ideas was strongly supported. Each task force recommended that State diversify the Foreign Service and that it emphasize creativity in training. Each also urged that performance evaluations give more weight to demonstrated creativity, and that competitive promotion pressures, held to breed conformity and caution, be lessened. Finally, both wanted State to develop better adversary procedures to force closer examination of existing and proposed policies and actions. More directly seeking to enhance openness, Task Force IX suggested several ways of bringing State into closer touch with other parts of government and society.

The second group of recommendations concerned management improvement. Underlying them were assumptions that State needed to find better ways of assessing U.S. interests and setting priorities among them, of linking such assessments to planning and decision making, and of evaluating how well decisions were carried out and how suitable current policies and procedures were. Primarily, the course chosen was to strengthen the offices of the Secretary and Assistant Secretaries. These "two key decision-making levels in the Department" were to become "Management Centers," unifying previously separated responsibilities for analysis and decision making, and for resource allocation. This unification was to be advanced by a policy analysis and resource allocation system (PARA). Based on the Country Analysis and Strategy Paper (CASP) in use since the mid-'sixties for Latin America, PARA was a programming device designed to help select policy goals and relate them to the means necessary to achieve them, first regionally and then globally. It was seen as a technique by which necessary information and analysis developed by those with country, functional, and regional expertise could be made available in a standardized format, which the sixth and seventh floors could use to keep tighter reign over policy and programs. These senior levels were also to be buttressed by improving staff support. Another proposal was to upgrade the existing Country Director system, and to insure that these relatively junior officials were not cut out of country-related policy making. Finally, a Management Evaluation Group was recommended as the institutional means for evaluating policies and program efforts, incorporating units such as the Inspection Corps and the Management Staff and creating a Policy and Program Evaluation Staff.

Most of the specific recommendations (345 of 505), however, were devoted to personnel questions. The seven task forces concentrating on these problems emphasized the need for creating a single personnel system, centralizing control of assignments for greater flexibility in using personnel resources, developing further functional specialization, adopting semi-automatic promotion at middle grades and "mid-career tenure," rating achievement rather than personality in performance evaluations, improving compensation and allowances, bringing individuals oriented to new tasks and possessing different backgrounds into the Foreign Service, and providing traditionally oriented FSOs with better training and broader career experience.

These recommendations, while primarily technical, did focus attention on a wide variety of ills which had long plagued State. Even if they all had become effective, they would probably have had only minimal impact in building State's government-wide role, but this internal

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Spring, 1972), 40–79; Halperin, "Why Bureaucrats Play Games," *Foreign Policy*, 2 (Spring, 1971), 70–90; Halperin, "Sources of Power in the Foreign Affairs Bureaucracy," paper delivered at the 1972 Annual Meeting, American Political Science Association, Washington, September, 1972; and Richard Neustadt, *Alliance Politics* (New York: Columbia University Press, 1970).

[21] Argyris, *Some Causes of Organizational Ineffectiveness Within the Department of State.*

[22] Task Force VII, *Democracy in the 70's*, p. 291.

[23] Task Force IX, *Democracy in the 70's*, p. 377.

Table I. Diplomacy for the Seventies: Management Reform Bulletins by Subject and Nature of Action

	Major Action	Minor Action	Cosmetic/Hortative Action
Personnel System	5. Centralization and Reorganization of Personnel System	2. Broadening the Foreign Service Recruitment Base 3. Modification of Examination Process 4. Increased Lateral Entry into the FSO Corps 6. Improved Perquisites and Allowances 8. A Unified Personnel System: The Foreign Affairs Specialist Corps 14. Improved Nonsalary Compensations and Allowances 18. Followup to 8. Maximum time-in-class for FAS officers 19. Modification of Performance Evaluation to Reflect Goal Achievement 21. Matching Opportunities and Needs (Training) 23. Restructuring the Foreign Service Institute's Program 27. Program Reform: Threshold Review and Mid-Career Tenure 28. The "Mustang" Program: Developing Talent Within 29. Determining Training Requirements 30. Training Opportunities for Staff Support Personnel 35., 36. Requirements for Area Specialists	17. Better Integration of Training with Onward Assignments
Management Reform	24. Strengthening the Seventh Floor 25. Creation of Management Evaluation Group—Office of the Inspector General 26. Creation of Policy Analysis and Resource Allocation System (PARA)	10. Improving Information Management 15. Automated Handling of Substantive and Administrative Information 22. New Approaches in Budget and Finance	11. Partial Proposals for Improvements in Regional Bureaus (African Affairs) 16. Intention to Seek Supports for Legislation to Reduce Consular Workloads
Creativity and Openness		23. Guidelines for Representational responsibilities of Wives (with additional action later) 31. Increased use of External Contract Research and Outside Consultants	7. Security Practices and Free Expression of Personal Views 9. Openness at Missions and Creative Dissent 12. Domestic Public Opinion and Informing the Policymakers 13. Public Affairs in the Decision-making Process 32. Stimulation of Creativity—one technique (Europolicy) 34. Role of the Open Forum Panel

NOTES: 1. MRB #1 not listed (establishes MRB format).
2. Classification based both on importance of proposals and impact in practice.
3. Descriptions used reflect contents and are not necessarily the formal titles of the MRBs.

strengthening would have increased the potential for broader future gains. But like most reform programs, this one was only partially carried out; the benefits were correspondingly diminished.

Accomplishments and Failures

By late 1972, most of the impact of *Diplomacy for the 70's* had already been felt,[24] and a clear end to the program was obvious with Macomber's departure to become Ambassador to Turkey in April, 1973. One officer with partial responsibility for the program estimated that perhaps half of what had been recommended and approved had been acted upon. Since most major items were reported in Management Reform Bulletins (MRBs) issued by Macomber's staff, analysis of these documents gives a reasonably accurate picture of what was attempted. Table 1 summarizes this survey. The categorization of the MRBs is based on whether each called for specific action or for less tangible support for concepts advanced in the program, whether the intended actions represented major departures or merely reinforced previous reforms or trends, and whether or not they actually became effective.

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[24] Macomber was reported to feel that most of what it would be possible to do had been accomplished, and that the reformers were at the point of stock taking and trying to determine what else might be done. Interviews in State, August and September, 1972.

The record of the program now emerges more clearly. On the personnel side, *Diplomacy for the 70's* is primarily the extension of a philosophy long prominent, and the proposals reflect much that was found earlier, particularly in the Foreign Service Act of 1946, the Rowe, Wriston, and Herter Committee reports, and the AFSA report of 1968. If placed fully into effect, these proposals would result in a broader Foreign Service incorporating a wider range of professional skills, personality types, and backgrounds. Progress has not come easily. The unification of the personnel systems began with the announcement of the creation of a Foreign Affairs Specialist Corps (FAS), into which some Foreign Service Staff Officers (FSSOs), Reserve Officers (FSRs), and a limited number of FSOs and Civil Servants in State were to be encouraged to transfer (MRB #8). However, this unification was stymied by legal action initiated on the grounds that civil service employees would lose some protection by becoming members of the Foreign Service system.[25] Similarly, MRBs #4 and #28, both directed to nonexamination means of recruitment, have foundered because of financial and numerical restrictions. Since State has been forced in this period to carry out a policy of attrition in the numbers of FSOs (from more than 3,500 to about 3,000 in five years) there have not been enough positions available for these lateral entry programs to have significant impact. Even when staffing pressures become less intense, future progress is not assured, for many FSOs continue to resist the idea of newcomers entering the service "on the cheap."

Other personnel proposals also encountered rocky going. An integrated career pattern was envisioned, beginning with a probationary period of about five years in grades 8, 7, and 6, followed by a "threshold review" which if surmounted would lead to roughly twenty years with "mid-career tenure" and semi-automatic promotions through grade 3, after which another "threshold review" would determine whether an officer would retire or would go on to executive responsibility at grades 2 and above. Presumably these procedures would help insure that only well-suited junior officers were given permanent status, and prevent deadwood from accumulating at the top. The individual officer would be freed from severe competitive pressures during most of his career and would have additional security after passing the initial threshold. The junior threshold review was initiated (MRB #27), but relatively few were rejected, partially because some board members argued that the candidates already had so much time invested in the service, and partially because some believed that entrance procedures were so rigorous that few weak individuals entered it in the first place.[26] Midcareer tenure has begun, but by extending the time allowed in class, rather than semi-automatic promotion. The senior threshold review has not been started, and substantial resistance is expected. Thus only limited portions of the program have been initiated, with little prospect the rest will follow.

Other obstacles to personnel reform have been wide dissatisfaction with selection-out procedures (under which officers rated at the bottom of their "classes" are forced to leave the Foreign Service), and severe doubts about the grievance practices of the department. Selection out was suspended at the end of 1971, and although it was resumed in July 1973, some in State believe it will never again be employed energetically, and may well be discarded entirely in the near future. Only in early 1973, after the American Foreign Service Association had been designated as employee representative for departmental personnel in an NLRB supervised election, was it possible to begin to develop permanent reforms in grievance procedures.[27] Whatever the need for increased fairness and tolerance, both obviously related to achieving a more open environment, restrictions on the use of selection out does undercut the program devised under *Diplomacy for the 70's*.

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[25] Technically, members of the FAS Corps were to be appointed as Foreign Service Reserve Officers with unlimited tenure (FSRU) under the authority of Public Law 90-494. See Management Reform Bulletin No. 8, February 16, 1971. The suit, filed by the American Federation of Government Employees, was based in part on the fact that civil servants are not subject to dismissal except for cause, while members of the unified Foreign Service system would come under selection out procedures if their performance was rated at the bottom of their respective cohorts for two successive years. Civil servants transferring to the FAS Corps would thus be somewhat less secure in their careers than previously. Until this question is resolved, only those already in the Foreign Service System (e.g., FSOs, FSRs, FSSOs) can be transferred to the FAS Corps, and the program is proceeding on this limited basis, with a terminal date of December 31, 1974.

[26] Given the small number of new FSOs in recent years and the personal decisions of many to leave the service after a brief period, it can be argued with some justification that those who remained long enough to be subject to junior threshold review were likely in fact to be qualified for full acceptance, and that the review might function differently if and when more junior FSOs enter the service and a large percentage of them desire to remain for a full career.

[27] For details on this election, see "AFSA Recognized as Exclusive Representative for Foreign Service Employees," *Department of State News Letter*, No. 142 (February, 1973), 15. State's grievance procedures have also been the subject of congressional interest, and hearings were held by the House Foreign Affairs Subcommittee on State Department Organization and Foreign Operations during 1972. Details on the interim grievance procedures in effect pending development

There have been a number of other personnel actions. The recentralization of the personnel system (MRB #5) is an accomplished fact. State had been operating five regionally based assignment systems, which was avowed to be inefficient and to reinforce the geographic bureaus' tendency toward separatism. There was also a need, however, because of tight budgets and personnel reductions, to improve overall personnel utilization.[28] Centralization also paralleled a trend toward de-emphasizing the importance of the geographic bureaus (see below). Some progress has been made in revamping training programs (MRBs #17, #21, #23, #29, #30), in reanalyzing the personnel skills actually needed (MRBs #6, #14), and in modifying the examination process (MRB #3) and evaluation guidelines (MRB #19) to reflect new needs and circumstances. None of these, however, will have a major impact on State's immediate capabilities.

Generally, management reforms have been more visible, although even here the results have been mixed. Perhaps most promising was the creation of a Management Evaluation Group in the form of an enlarged Office of the Inspector General (MRB #25). It showed signs of effectiveness in appraising the administrative performance of missions abroad and units in Washington, and several innovations in the inspection process were encouraging.[29] Its ability "to evaluate the effectiveness of existing policies and programs,"[30] is more problematic, since it is not clear that anyone has been or will be able to develop requisite policy evaluation (as opposed to administrative) criteria. At the same time, both Inspectors General who have served since MRB #25 was issued seemed to have been chosen for their ability to tie evaluation into the policy process, which suggests some desire to upgrade their office in importance.

An effort was also made to strengthen top leadership through development of the "Seventh Floor Collegium" or "Senior Management Group,"[31] which was served for the first time by common functional staffs and support mechanisms (MRB #24).[32] These officials attempted to work as a unit, and there was a conscious effort to link administrative and substantive concerns by including the Deputy Under Secretary for Management. Some well-placed observers maintain that Macomber's prime (although perhaps short-lived) accomplishment may have been to force top policy makers to become somewhat interested in overall management, reducing the traditional split between substance and administration. But this was at best a tentative change, since four of the top seven positions, including Macomber's, were vacated in early 1973, and it was unclear whether these procedures would endure with new members. Henry Kissinger's appointment as Secretary of State and additional changes in the top positions made the collegium's viability even more doubtful.

The picture is even less promising at the next levels. Little was done to give the Assistant Secretaries better staff support, although this was held to be a vital adjunct to seventh-floor improvements. Regional Assistant Secretaries were even in danger of finding their powers reduced through growing emphasis on the functional bureaus, and much the same was true for Country Directors. Their freedom of action was likely to be further reduced by recent tendencies to make more decisions at the top and to move away from making policy on a country-by-country basis. Moreover, recommendations to strengthen the Country Directors were all but ignored, especially when they conflicted with others designed to build up higher levels.

The PARA system was installed, and made some limited inroads into traditional State Department skepticism about programming sys-

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of permanent ones can be found in John A. Warnock, "The Foreign Service Grievance Board: How it Handles Its Cases," *Department of State News Letter*, No. 142, (February, 1973), 12–14; and William E. Simkin, "Foreign Service Grievance Board—Remedies Available and Needed," *Department of State News Letter*, No. 149 (October, 1973), 23–27.

[28] Although estimates vary slightly, there are about 20 per cent fewer State employees in 1972 than there were five years earlier. The centralization may also be cyclical, since it has occurred before only to be reversed later.

[29] For details, see "S/IG Introduces Far-Reaching Changes in Inspection Process," *Department of State News Letter*, No. 148 (August-September, 1973), 29.

[30] MRB # 25, July 6, 1971.

[31] These seven officials are the Secretary, Deputy (formerly Under) Secretary, Under Secretary for Political Affairs, Under Secretary for Security Assistance, Counselor, Under Secretary for Economic Affairs, and Deputy Under Secretary for Management.

[32] These common staffs and support units were the Executive Secretariat, the Planning and Coordination Staff, Press Relations, Protocol, Legal Advisor, Congressional Relations, Inspector General of Foreign Assistance, and Inspector General of the Foreign Service. Before this change, these units were tied more closely to just one of the Seventh Floor officials, usually the Secretary or Under Secretary. In early 1974, the Planning and Coordination Staff was renamed the Policy Planning Staff.

tems.[33] Whether it would long survive Macomber was in doubt, as was its ability to achieve its stated purposes. Information resources and staffs, especially at the Assistant Secretary level, are not yet adequate to support it fully. It is not clear that PARA has significantly influenced decision making, although in a period of resource limitations it helped set priorities among operating programs. It also facilitated, or made possible for the first time, collection in one place of most relevant information related to given countries or problems. Some officials initially asserted that PARA would increase participation in decision making, since working-level officials would have face-to-face contact and input with seventh-floor principals during the biennial PARA reviews, but it also seemed possible that once this was done, decisions formerly made in the geographic bureaus would be made higher up. Thus PARA may have been intended as one of those bureaucratic routines developed, as Francis Rourke has written, "essentially to curb the ability of individual bureaucrats to influence policy through the exercise of personal discretion."[34] Whatever was intended, PARA did little either to provide seventh-floor officials with greater information or to facilitate contact. As time went by, some senior level PARA reviews were omitted. Finally, in the fall of 1973, new guidelines were issued which made PARA an exercise almost entirely conducted between the field posts and the bureaus, with regular seventh-floor reviews being omitted. This may have been realistic, especially after Kissinger redesigned seventh-floor procedures and modes of operation, but it undercut some purposes the system was initially designed to serve. It was also possible that it would become an optional exercise, which would further reduce its impact.

Diplomacy for the 70's also brought the tentative beginnings of modern information management and processing. An Information Management Steering Group was established (MRB #10), and shortly announced the first steps toward automatic data handling (MRB #15). While a basic indexing and retrieval capacity now is being installed in the department, it is not particularly sophisticated and has not yet been widely used.

Critics would generally agree with the desirability of most of these management changes, and would urge further attention to those still incomplete. At the same time, the view of the policy process underlying them is rather mechanistic. If "one may say with considerable persuasiveness that all the *important* problems of administration are really problems of politics,"[35] and if the policy process is in large measure political rather than analytic,[36] then managerial improvement, however, essential, is not by itself enough to strengthen the State Department's role. Such changes may help strong actors to be even stronger; they seem unlikely to make the weak powerful. Centralization of decision making without the addition of political clout did not help State resist NSC staff control. It also may have made it even more difficult to employ State's bureaucracy to full capacity, leaving the department even less equipped to play a major role.

State's reformers have faced the roughest going in establishing an open milieu that would make creative performance more likely. Unlike management and to a degree the personnel area, where concrete steps to obtain desired goals are sometimes possible, it has proved very difficult to engineer changes of mood and approach. Most of the "action" in this area consisted of urging various elements of the department to encourage diversity and broader contact with the outside (MRBs #7, #9, #12, #13, #31, #33). Nonetheless, State has found it necessary to curtail public expressions of dissent.[37] A draft MRB allowing FSOs to differ publicly from official policy subject to security limitations, as long as it was clear they were speaking as individuals, was shelved after the Anderson Papers episode in late 1971. Some internal channels of dissent have been routinized (MRBs #32, #34), but many officers remain skeptical. There is admittedly a fine line to be walked between free expression which will stimulate internal creativity, overcome bureaucratic stodginess and excessive caution, and help build public support for State, and the requirements of presenting a united front in diplomatic encounters, safeguarding information, and supporting presidential policies as part of the executive. Yet overconcern for the latter undercuts all the good words about openness, particularly when many younger FSOs believe that some of their peers have been selected out for their outspokenness.[38]

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[33] For the problems of previous programming efforts, see Frederick C. Mosher and John E. Harr, *Programming Systems and Foreign Affairs Leadership: An Attempted Innovation* (New York: Oxford University Press, 1970); and U.S. Senate Subcommittee on National Security and International Operations, Committee on Government Operations, *Planning-Programming-Budgeting* (Washington, D.C.: GPO, 1968).

[34] Francis E. Rourke, *Bureaucracy and Foreign Policy* (Baltimore: Johns Hopkins University Press, 1972), p. 49.

[35] James W. Fesler, "Administrative Literature and the Second Hoover Commission Reports," *American Political Science Review*, 51 (March, 1957), 140. Emphasis in original.

[36] This argument underlies most of the works cited in note 20, and is made more explicitly in Charles E. Lindblom, *The Policy Making Process* (Englewood Cliffs, N.J.: Prentice-Hall, 1968) and Lindblom, *Intelligence of Democracy: Decision Making Through Mutual Adjustment* (New York: Free Press, 1965).

[37] One such episode is reported in Benjamin Welles, "State Department Tells Its Missions Not to Air Dissent," *New York Times* November 22, 1971, p. 11.

On balance, taking *Diplomacy for the 70's* on its own terms, one must be more impressed with the willingness of some in State to attempt reform again than with its accomplishments. Some needed changes have been made, and some current actions may pay later dividends, but the program's impact is at the margin of the policy process.

The Larger Problems of Reform in Foreign Affairs

Diplomacy for the 70's, in short, suffered the same fate as its predecessors. At best it helped improve State's capabilities for carrying out decisions made elsewhere. No answer was provided to the fundamental question of institutionalizing foreign affairs leadership. Its limitations are more comprehensible, however, in light of obstacles faced by the reformers. They could hardly expect to reassert State's primacy without support of the President, but he was more than satisfied with the "White House-centered System"[39] led so flamboyantly by Henry Kissinger. Although Nixon had stated he wanted State to be more effective in coordinating interagency activities,[40] it was clear that he did not intend that it displace Kissinger (even when Kissinger later became Secretary, his personal strength hardly insured enhancement of the Department's role as an institution). Under these realities, reform had to be confined to the Department.

One must look further, however, to understand why *Diplomacy for the 70's* was not more successful within its circumscribed limits. Financial and personnel restrictions made some desired changes impractical. One estimate was that a one-third larger budget would have made it possible to do virtually everything so deferred or dropped. Some items were derailed by the need for legislation which proved difficult to obtain in the face of congressional or OMB opposition. Action was also delayed on some proposals that depended upon prior completion of others. The absorptive capacity of State was another problem, and the reformers tried to strike a balance between attempting too much and doing so little that the program expired from boredom. Finally, reform efforts in State seem to follow a cyclical pattern, with major initiatives being followed by consolidation and reversal. In part, time ran out before everything desired could be done: By October, 1973, it was virtually impossible to obtain even an up-to-date status report on uncompleted portions of the Macomber program.

All of this blunted the thrust of *Diplomacy for the 70's,* but the major handicap was more basic. Mosher has argued that to succeed, significant changes in State seem to require the support of the leadership of the Foreign Services and/or "aggressive, even ruthless, follow-through by the very top political officers."[41] Participation and the backing of AFSA leaders were helpful, but innovations such as the PARA system, increased lateral entry, and upgrading of nonpolitical functions went against the grain of many FSOs. But what seemed most lacking was the will to push the program from the top. One initial bright spot was the strong leadership of Elliot Richardson, who was then influential at the White House and who, alone among recent Under Secretaries, maintained an engaged interest in organizational matters. With his departure while the task forces were still meeting, *Diplomacy for the 70's* suffered a blow from which it never fully recovered.[42] Richardson's successor lacked the influence and perhaps the desire to force tough decisions about the program, and the secretary's role was confined to benign approval. After a strong beginning, the reformers found themselves fighting a distinctly uphill battle. Their situation became similar to that facing former Deputy Under Secretary for Administration William Crockett during the last major (and generally unsuccessful) reform effort in the mid-1960's.[43] Without strong support, the reforms assumed less importance in the minds of FSOs and political appointees alike.

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[38] The reported attitudes of younger FSOs are based on discussions with some of them and on the reactions of others quoted in the press with respect to individual cases during the period 1970–72.

[39] The term is used by Keith C. Clark and Laurence J. Legere, Chap. 2, "The Main Issues," in *The President and the Management of National Security,* ed. Clark and Legere, p. 17.

[40] See the White House Announcement on "The Structure, Role and Staff of the National Security Council," February 7, 1969. Reprinted in *Weekly Compilation of Presidential Documents,* 5 (February 10, 1969). See also William P. Rogers, "Message on the Department of State's Responsibilities," February 7, 1969, reprinted in U.S. Senate Subcommittee on National Security and International Operations, Committee on Government Operations, "The National Security Council: New Role and Structure" (Washington, D.C.: GPO, 1969), pp. 3–4.

[41] Mosher, "Some Observations About Foreign Service Reform: Famous First Words," *Public Administration Review,* 29 (November-December, 1969), 602.

[42] Richardson left State in June, 1970, to become Secretary of Health, Education, and Welfare, and then briefly, Secretary of Defense, and Attorney General.

Planned organizational change is at best an uncertain business. Outright failures and unintended consequences abound. Its dynamics are not well understood, and as Bennis has written, "no viable theory of social change has been established.[44] Nonetheless, some tentative conclusions about the failings of *Diplomacy for the 70's* seem likely to have wider application. First, participation alone is no guarantee of success. If the human relations school is right about the difficulties of reform by decree from the top down, its advocates are too sanguine about what is possible from the bottom up, especially when interests in the middle must be disturbed. They underestimate the importance of top leadership's willingness to give continuing emphasis to reform in an environment where forcing attention may be as important as overcoming resistance.

Second, announcing a program is one thing; making it work another. Foreign policy leaders have often been guilty of assuming that making a decision or announcing a policy is tantamount to carrying it out, and the same has been true of attempts to change the structures and procedures which generate them. Failure to follow through has plagued most reform programs, and lack of any continuing means of expediting reform other than a small and overworked staff was a serious shortcoming of *Diplomacy for the 70's*.

Third, the evidence suggests that rationalization of management procedures is at best an aid which can buttress senior players in the bureaucratic/political process from which policy emerges. Reform programs which fail to take this into account are unlikely to have major impact. Political weight is needed both to be effective in policy making and to force process reforms. This reality presents a dilemma to leadership, since there are political costs involved: those with surplus political capital to expend on reforming their own organizations may least need the added support that better organizational performance can provide; those whose organizations are handicaps rather than assets may already be so disdvantaged in competition with others that they cannot afford the costs of undertaking major reforms.

Fourth, reforms designed to strengthen particular organizational levels or positions, the seventh floor in this case, may do so at the cost of weakening the overall organization. Subordinate levels must have latitude for discretion if they are to be effective, and if the entire organization is to perform capably.

Finally and most obviously, programs must be directed to basic weaknesses, and not just to what it seems most possible to do something about, if they are to have any reasonable ameliorative impact. If the record of the past reform programs show any one underlying theme, it is that the real problems have seldom been faced up to directly.

But while *Diplomacy for the 70's* reached the end of its run without substantial impact, one should not conclude that foreign affairs reforms are impossible. Rather, a realism about what can be accomplished requires that the mechanistic or "rational" model of organizations be supplemented by awareness of their "natural" characteristics.[45] This may help direct reform efforts of the future to those areas which can be significantly affected. A test of what has been learned is in the offing, for the new Commission on the Organization of the Government for the Conduct of Foreign Policy began its work in mid-1973, charged with finding means to provide "a more effective system for the formulation and implementation of the Nation's foreign policy."[46] If this commission is able to meet the minimum requirements for successful foreign affairs reform, if it considers the whole constellation of relevant forces and takes steps to crystallize the relationships among them, and if the lessons of *Diplomacy for the 70's* and its many predecessors are not ignored, then progress toward resolving Macmahan's "structural problem" may at last be possible.

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[43] Much of Crockett's program is summarized in "A Management Program for the Department of State." See also John P. Leacacos, *Fires in the In-Basket: The ABC's of the State Department* (Cleveland: World, 1968), pp. 317–78, 358–64, 456–57.

[44] Warren G. Bennis, "Theory and Method in Applying Behavioral Science to Planning Organizational Change," *Journal of Applied Behavioral Science,* 7 (October-December, 1965), 339.

[45] See Alvin W. Gouldner, "Organizational Analysis," in *Sociology Today: Problems and Prospects,* ed. Robert K. Merton, L. Broom, and L. S. Cottrell, Jr. (New York: Basic Books, 1959), p. 400–428.

[46] Provisions of the Foreign Relations Authorization Act of 1972 relating to this commission, which is chaired by Robert D. Murphy, are contained in Title VI. See Congressional Record 118 (June 15, 1972), H 5716 for the final text.

BOOK REVIEWS

Towards a New Theory of Distributive Justice. By Norman E. Bowie. (Amherst: The University of Massachusetts Press, 1971. Pp. 148. $9.00.)

Professor Bowie is an ambitious philosopher. He has undertaken, in one slim volume, the task of delineating the outlines of "a new theory of distributive justice," one that is applicable not only to the family and the nation-state, but one that "can be applied to the world as a whole" (p. 134). Modern philosophy, according to Bowie, has abandoned the study of justice in favor of epistemology. This is the result of an almost universal agreement among twentieth-century philosophers that problems of values are not properly the subjects of scientific disciplines. This scientific agreement, however, has not been persuasive to political man, who stubbornly insists on describing "the actions, purposes, and characters of his fellow human beings as just or unjust" (p. 1). It is this stubborn insistence which leads Bowie to confront normative ethical problems in a more "traditional" sense which assumes that "reasonable value judgments" can be made (p. 3).

Bowie's methodology is to suspend the second-order meta-ethical problems and concentrate on the first-order ethical problems. "This book . . . temporarily places the higher-order questions in philosophical limbo," otherwise "the discussion of distributive justice would never get started" (p. 9). This, in my opinion, is a sensible view which is animated by a concern for politics and justice. It does, however, put the philosopher or the social scientist in an awkward position. If the metaethical questions are suspended out of concern for justice, what can be used for standards? How does the philosopher know whether he has found the right solution to the problem or, indeed, that he has even asked the right questions? Bowie answers this question by an appeal to the *status quo:* "I do feel constrained by our clearest and most basic notions about the nature of justice. If a theory sanctions as just any distribution which seems self-evidently unjust, then the theory is inadequate" (pp. 7–8). This may be a political perspective, but it is certainly not a theoretical one (cf. p. 6).

Bowie rightly notes that problems of distributive justice are coincident with political society itself. "Disputes about justice result when values are in conflict" (p. 8; cf. pp. 6, 85, 94, 104, 105). It is these disputes and the activities that accompany them that constitute the phenomena of politics. An adequate theory of politics, therefore, would be one in which the "problems of distributive justice are resolved by applying and ordering the relevant values" (p. 117; cf. pp. 104, 105, 108, 122, vii). Bowie takes a moderate, nonideological position with respect to the possibilities of applying and ordering the relevant values. In contrast to many utopian ideologists, he sees distributive justice as remaining essentially problematic. The best anyone can hope for is a workable compromise among the competing claims.

Bowie analyzes the strongest traditional views of distributive justice, utilitarianism (both philosophic and economic), egalitarianism, and socialism. This analysis, which recalls Robert Dahl's seminal work, *A Preface to Democratic Theory,* reveals that none of the traditional theories of distributive justice are adequate. They not only lack conceptual or analytical clarity, but they also attempt to present a principled choice of one or a few values at the expense of other values. Bowie considers this "too restrictive"; he maintains that principled choices that attempt to maximize specific or limited values cannot be made in noncontradictory ways. For example, the utilitarians' preoccupation with "the greatest happiness for the greatest number," whether considered philosophically or economically, is too restrictive; it cannot accommodate such competing values as "equality" and merit." Egalitarianism and socialism present essentially the same principled "exclusivity." One suspects, however, that the main fault of the traditional theories, in Bowie's opinion, is simply that they are "pre-analytic" (cf. pp. 10, 83, 84).

The solution, according to Bowie, is a pluralistic model which, much like Dahl's polyarchy, fosters a "multiplicity of values." The ordering of these values will depend largely upon a society's capacity to produce goods and services. In order to supply analytic clarity to his theory, Bowie formulates a language model whereby he constructs various "justice sentences" composed of "justice words." The rules of spelling and syntax as well as the application of "contextuality" are derived in a dialectical fashion from his analyses of the traditional theories of distributive justice.

The result is a model which allows for the application of the relevant values according to the economic condition of a society. A society of affluence, for example, would be able to foster more values than a society at either the scarcity or subsistence level. Values other than merely production values, it seems, are a luxury (cf. pp. vii, 118, 119, esp. 128, 132–133). Once a society has achieved "a minimum standard of decency," which can be known with some degree of specificity, then the pluralist model can be utilized. In this way, "no relevant values are ruled out" (pp. 128, 136). This, of course, means that, practically speaking, a wealthy (commercial) liberal democracy with a "multiplicity of interests" is the most just regime since it is the one that can tolerate the greatest "multiplicity" of values. In treating the noneconomic values (the noncognitive values?) in this way, Bowie doesn't have to specify the moral content of such values nor make moral decisions among the various competing noneconomic values. This is a surprising result in light of his earlier professed willingness to meet moral decisions head on. Professor Bowie has, nevertheless, produced a remarkable contribution to the study of distributive justice.

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Content Analysis: A Technique for Systematic Inference from Communications. By Thomas F. Carney. (Winnipeg, Canada: University of Manitoba Press, 1972. Pp. 343. $11.00)

It is somewhat doubtful whether students of political science will find this book of much use. Written by a professor of history, it is mainly directed at applications of content analysis in the arts and humanities. While I enjoyed watching Professor Carney fight what are apparently the methodological battles in the humanities, the methodological innovations he suggests are considerably dated for most political scientists. For example, two chapters devoted to "multivariate analysis" are essentially discussions of how to set up and use n x m-fold classification matrices.

Other chapters cover the definition of content analysis, its evolution, the relationship between content analysis and general semantics, the relationship of content analysis to areas of psychology, the sampling problem and finally a discussion of the actual rules and procedures of content analysis. The first two chapters are interesting particularly in the illustrative examples given of the applications of content analysis to solve certain historical problems. The third chapter is intriguing and provides what is probably an important but overlooked adjunct of the content analytic methodology, namely general semantics. The fourth chapter on the "new look" in psychology (personality-and-perception studies) is puzzling, and its relationship to the rest of the book is unclear. The fifth chapter, covering the sampling problem, is poor. The definition of sampling, types and determination of possible samples, and the implications of the sampling issue for the content analytic methodology are only briefly and superficially explored. The sixth chapter represents the guts of the book in attempting to explain the details of the content analysis procedure, step by step. Most of the significant points are covered, but I found the discussion to be fuzzy and the development of the points not always logical. Since Professor Carney indicates in his introduction that his main purpose for producing yet another text on content analysis was to provide a very readable, self-evident how-to-do-it text, this key chapter could have been written more carefully. Interestingly, the three remaining chapters which principally cover "multivariate analysis" are the best written. Professor Carney's style of leading the reader by the hand from example to example has its greatest payoff in these chapters. However, as noted above, "multivariate analysis" for this author means only the consideration of two or more variables simultaneously and is not a discussion of multivariate statistics.

Those who would like an additional source on the content analytic methodology or would like to see applications of content analysis in the humanities and some of the attendant problems might enjoy perusing parts of this volume. Beyond this, however, the volume probably will not be particularly useful to political scientists.

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A Treatise on Political Economy. By Antoine Louis Claude Destutt de Tracy. (New York: A. M. Kelley, 1970. Pp. xxviii, 344. $15.00.)

Psychology of Political Science With Special Consideration for the Political Acumen of Destutt de Tracy. By John M. Dorsey. (Detroit: Center for Health Education, 1973. Pp. xxxvi, 516. $15.00.)

These works are significant contributions to a revival of interest in the history of American public administration. Both reprint Destutt de Tracy's classic 1817 *Treatise on Political Economy.* This treatise was translated by Thomas Jefferson, who said of it, "It goes forth therefore with my hearty prayers, that while the *Review of Montesquieu,* by the same author, is made with us the elementary book of instruction in the principles of civil government, so the present work may be in the particular branch of Political Economy" (p. i).

Given Jefferson's endorsement of Destutt de Tracy's works as the basic texts in the field with which our discipline is concerned, students of administrative history, illustrated by Leonard White's *The Jeffersonians: A Study in Administrative History* (1951), have curiously neglected him. The philosophic kinship of Jefferson and Tracy has been analyzed in Adrienne Koch's prize-winning essay on *The Philosophy of Thomas Jefferson* (1943). While much of the correspondence between Jefferson and Tracy is available in French in Chinard's *Jefferson et Les Idéologues* (1925), the newly-reprinted original works are more revealing. (The *Review of Montesquieu's Spirit of Laws,* orig. 1811, has been reprinted as B. Franklin Research and Source Works Series 290, 1969.)

Thus, while consideration of Tracy's philosophic influence on Jefferson has received at least some attention, the economic aspects of his writings have been neglected. Without belittling Tracy's political philosophy, rooted in emphasis of the "psychology" of sensations and desires expressed through "the faculty of will" (and leading to a concept of property as an expression of personality or self), political scientists will find more striking the views on public finance which found expression in Tracy's work and in Jefferson's administration, leaving a heritage which can still be perceived today.

"Government," Tracy wrote, "cannot be ranked amongst the consumers of the industrious class. The expenditure it makes does not return into its hands with an increase of value" (pp. 196–7). This attack on the prevalent belief in the utility of government expenditures in animating circulation in a manner "very favourable to the public prosperity" corresponds to Jeffersonian belief in the virtue of economy in government and gave a rational underpinning to it.

Some have seen the Louisiana Purchase as contravening Jeffersonian republican principles. In a not unrelated passage, Tracy states, "Many politicians do not approve of government having landed estates . . . I, on the contrary, see many advantages in the government's having possessions of this kind" (pp. 197–8). Likewise, Tracy's opposition to a tax on persons finds some counterpart in Jefferson's repeal of the internal revenue system in spite of the attendant loss of a quarter of the federal patronage. "The consumption of industrious men," Tracy wrote, "being the only one that reproduces what it destroys, and being therefore the only source of riches, it is that above all which we ought to endeavor not to derange [i.e., through taxation]" (pp. 228–9).

Republican principles regarding the national debt are also reflected in Tracy's belief that "every time a government borrows it takes a step towards its ruin" (p. 244). Here, too, one may find the economic rationale for the Republican policy toward banking. Tracy wrote, "All these bankers, exchangers, agents, lenders, discounters, at least the richest and most accredited among them, have a strong tendency to unite themselves into large companies" (p. 103). The result, illustrated by the evil effects of the Bank of London, is "radically vicious" (p. 105). Though not viewed as consumers and destroyers of wealth, as was government, banks were viewed as being of "very inconsiderable" advantage and no small danger.

Corresponding to Jefferson's views on small-scale farming, Tracy wrote, "The great details into which I have gone to analyse the operation of all the springs of agricultural industry, prove sufficiently the importance I attach to it; and certainly to show clearly that a profession is at the same time very necessary, and very unprofitable, is the best method of proving that it should be favored" (p. 66). Of great farms he said, "I think I have proved that the proprietors of lands, who do not work them themselves, have absolutely nothing in common with agriculture, nor with the laws which govern it" (p. 59).

This is not to say that Tracy's political economy and Jefferson's public finance do not at times stand in tension. They do, but the correspondence was sufficiently striking for Tracy and Jefferson to find in each other support for their own perspectives on public economic policy. As Jefferson wrote to the publisher of the *Treatise,* "For perspicuity, precision, and demonstrative logic, we have had nothing to equal it." If contemporary readers would not share this generosity it is at least useful to have reprinted after one hundred and fifty years of scholarly oversight an articulate formulation of political economy associated with the Republicanism of Jefferson. Its perspectives were implicit in much of the public policy of the day, and it may still be read with interest. Theoretically, it represents a world-view which justifies the role of the small entrepreneur, whether in agriculture, commerce, or industry, against the tendency of government and financial combinations to constrain that role in ways deemed not desirable, at a time when this could plausibly be portrayed as a representation of the public interest in general prosperity.

It may be noted that in the Dorsey edition a lengthy commentary on Tracy is printed along with the original work. Dorsey, an M.D., discusses Tracy as an individualist and hails him for articulating a political theory in which law is premised on "protecting and furthering the nature and needs of perfect human individuality" (p. 1). The term "psychology" in the title of this work refers to the Hobbesian rather than the modern usage. Dorsey's treatment may be viewed charitably as a personal, literary one not conducted at a level likely to be of interest to students of political philosophy or administrative history.

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Elite and Specialized Interviewing. By Lewis Anthony Dexter. (Evanston: Northwestern University Press, 1970. Pp. 205. $6.50.)

Out of his wealth of interviewing experience with congressmen, business leaders, lobbyists, and others, Lewis Anthony Dexter has assembled a useful manual for interviewers. It is a practical guide—a cut above the "how to do it" primer—replete with tactical advice and reflective suggestion.

Dexter draws a careful distinction between survey interviewing and the subject matter of his book. As with others who have employed similar research styles (he cites David Riesman on this point), Dexter is not happy with the term "elite." He indicates, however, that he has found no other term that is preferable for the idea he wants to convey, "—namely that people in important or exposed positions may require VIP interviewing treatment on the topics which relate to their importance or exposure" (p. 5). The other terms frequently used in the literature are more confusing for Dexter—"nonstandardized," "exploratory," "journalistic." An elite interview is therefore, "an interview with any interviewee—and stress should be placed on the word 'any'—who in terms of the current purposes of the interviewer is given special, non-standardized treatment" (p. 5). Such treatment involves a considerable measure of latitude for the respondent in defining the situation, giving his own account of it, and introducing his own notions of what is relevant.

To the unregenerate survey interviewer this may sound suspiciously unsystematic and uncontrolled and, indeed, Dexter eschews the standardized interview in which the investigator defines the question and problem in "Proscrustean interview schedules." Dexter argues that the investigator should be willing—even eager—to let the interviewee indicate what the problem, the question, and the situation is, within the bounds—of course—of the broad topic being examined and the interviewer's ability to perceive relationships to his own basic research problem. Dexter is frank to acknowledge that his approach has not always been accepted by colleagues or sponsors. In introducing a 1959 "Working Paper on Interviewing Procedures for a Law and Psychiatry Project," reproduced as Chapter 3 of the book, Dexter indicates that the paper was so unsatisfactory to the project director by whom it was commissioned that he refused to accept it! The lines of disagreement were clearly drawn.

Many would be inclined to agree with Dexter because of the persuasiveness of the argument for flexibility and the frequent necessity of adopting this approach with the influential, prominent, and well-informed. As the book suggests, a good many well-informed and influential people are unwilling to accept the assumptions with which the investigator starts. Moreover, the interviewer may not be able to insist upon their keeping to a standardized line of discussion.

Dexter counsels against extreme latitude in setting the terms of an interview. In general, he feels that one should accept any *time* the interviewee proposes but seek to avoid undesirable *locations* (restaurants, homes, and other places where distractions are present). A large number of other suggestions are made for obtaining, conducting, and recording the interview. There is attention to the way in which one introduces oneself, starts the interview, uses leading questions, and takes notes. Protecting the confidentiality of the interview (where discretion is called for) and safeguarding interview reports are explored.

In addition to the material by Dexter himself, this volume contains two additional papers. "On Oral History Interviewing" by Charles Morrissey, Director of the Vermont Historical Society, discusses oral history interviewing of persons acquainted with Presidents Kennedy (90 interviews) and Truman (15 interviews). Open-ended questions were employed allowing the interviewee to set his own pace; documents were often used to refresh the respondent's memory (e.g., photographs of persons present on a particular occasion; itineraries of Truman's speaking engagements during the 1948 campaign).

"How Do You Know If the Informant Is Telling the Truth?" by John P. Dean and William Foote Whyte deals with strategies for determining the quality of various kinds of subjective data elicited by questions. The advice:

determine plausibility, reliability of the informant, and discrepancies with reports of others; look for ulterior motives, bars to spontaneity, desire to please, and idiosyncratic factors.

A final chapter by Dexter—somewhat inappropriately entitled, "Toward A Transactional Theory of Interviewing: Self-Assessment in the Interview Process"—deals with some familiar problems of interviewer effect, distortion, bias, perceptual differences, and face-to-face interaction. The chapter does not provide an original theoretical statement within the usual realms of Social Psychology, Information Theory, or Exchange Theory.

Researchers engaged in elite and specialized interviewing as defined here are finding this book an important and refreshing contribution. Its impressionistic approach makes possible the kind of helpful guidance to which younger investigators often are not sufficiently exposed. Additionally, Dexter offers 25 pages of references to other useful sources.

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Social Radicalism and the Arts: Western Europe. By Donald Drew Egbert. (New York: Alfred A. Knopf, 1970. Pp. xxxiii, 821. $15.00.)

Political scientists willing to take a busman's holiday will be enchanted with Professor Egbert's excursion into the realm of art and politics. They can expect little or no respite from doctrine and jargon, but in the land of aesthetics the grass seems somehow greener, the perspectives more elevated, and the inhabitants (David, Courbet, Goya, Matisse, Picasso, Gropius, the Dadaists, to name but a few) a piquant and diverting lot. Best of all, here is the chance to inspect part of that "cultural superstructure" which, as Engels acknowledged, not only springs from its economic base but in turn reacts upon it.

Professor Egbert is not a political scientist (who in our profession would dare beard the artist in his studio?), but his all-round credentials are sound and he is an old master in his specialty: Professor of Art History at Princeton, a practicing architect, author of *Socialism and American Art* (1967) and co-editor (with Stow Persons) of the two-volume symposium *Socialism and American Life* (1952). He is currently completing two further studies in ideology and art, one on the Soviet Union, the other on the United States.

The present work focuses on Western Europe, though inevitably spilling over both eastward and across the Atlantic. It is encyclopedic in scope and detail, intended, as the author says, to serve as "a source book for the cultural history of modern radicalism as seen through the history of art." The period surveyed ranges from the French Revolution to the present, with flashbacks to the theoretical contributions of the Enlightenment. Egbert properly includes in "the arts" not only the classic triad of painting, sculpture and architecture, but the more "functional" crafts as well—ceramics, woodworking, town planning, even music, film and theater, as these fall within the ambit of radical purpose. The author's intent is to seek out answers to a cluster of related questions, *inter alia:* How does the artist interact with his social environment? When artists become social radicals, what are the sources of their discontent? How do they transmute their radicalism into aesthetic protest? With what effect upon their art and the social milieu? How does changing technology impinge upon artistic styles and standards? Is good art necessarily "progressive" in its development, or are its norms universal and timeless?

Not surprisingly, Egbert's findings disclose some striking parallels between artistic and social history. Artists, it seems, behave much like the rest of us—at least, like other intellectuals—only more so. They are more likely than others to become estranged from their society, in part because of temperament and economic privation, but more acutely because of a heightened "sense of injustice" coupled with a utopian vision for man. Like others of lesser gifts, radical artists of the nineteenth and early twentieth centuries were outraged by the oppression of the poor, the arrogance of power, the inequities of the law, the hypocrisies of conventional morality. Yet only a minority of artists rebelled politically against their condition; most did not, and of those who did rebel, some put their faith in reform, others in revolution; and they either found their place in a sympathetic movement (chiefly Marxist, though also St.-Simonian, Anarchist, Fabian, and others), or preferred to go it alone. Few, if any, had more than a sloganistic acquaintance with the social doctrines they embraced—a defect the author readily forgives since, in his judgment, the most effective radical works of art have expressed a profound social mood in universally human, rather than in explicitly ideological, terms.

Perhaps the most intriguing aspect of Egbert's study is his treatment of the avant-garde, that small band of artistic movers and shakers who not only interacted with, but literally revolutionized, their cultural milieu. The avant-garde is, by definition, the vanguard party of the arts, a militant, essentially anarchistic minority committed to no particular doctrine or regime but rather to a permanent revolution aimed at liberating the artist from all traditional restraints. Throughout the nineteenth century the common enemy was clearly bourgeois philistinism, and just as clearly Marxism pointed the way of liberation; political and artistic radicalism were thereby fused. But with the triumph of the social revolution, which occurred briefly under the Paris Commune and more durably under the Bolsheviks, the breach between artist and commissar became evident: as the Chinese saying goes, "they lay in the same bed but with different dreams." Lenin vacillated, but Stalin definitively established the role of art under Communism as that of a weapon in the class struggle. Socialist Realism—the apprehension by the masses of that which was concrete and familiar, therefore edifying to the spirit—became the single approved style. The result was an exodus of nonobjective artists from the Soviet Union.

The paradox, as Egbert notes, is that avant-garde movements flourish best in a bourgeois-democratic culture; philistinism (to paraphrase Jefferson) is their natural manure. There is the further paradox that, even as the avant-garde condemns industrial technology, it borrows the new technological methods and materials for its own experiment. Avant-garde art in the West, originally created in defiance of bourgeois tradition, has now become not only accepted but fashionable, its products fought over by bourgeois collectors and galleries, its producers apotheosized as culture heroes. Even the more grotesque modern styles, those characterized by Egbert as "deliberate artistic outrage" (perhaps analogous to political terrorism) are quickly co-opted and merchandised, consistent with Marcuse's doctrine of repressive tolerance.

Half a century ago Trotsky astutely observed that "Art means prophecy . . . works of art are the embodiment of presentiments." Whatever the future may bring, the present embodiments are Socialist Realism in the East and Op-Pop-NeoDadaism in the West. Clearly, each society deserves the art it gets.

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Psychoanalytic Jurisprudence: On Ethics, Aesthetics, and Law—On Crime, Tort, and Procedure. By Albert A. Ehrenzweig. (Leiden: A. W. Sijthoff; Dobbs Ferry, N.Y.: Oceana Press, 1971. Pp. 395. $19. 40.)

As a native Austrian and a former scholar in Vienna, Professor Albert Ehrenzweig is the heir to a rich and varied intellectual heritage, which has included men such as Freud, Jung, Konrad Lorenz, and Erik Erikson. The Viennese tradition has combined a sense of the interconnectedness of ideas and disciplines with a humanistic outlook on the world: Ehrenzweig's *Psychoanalytic Jurisprudence* reflects both these perspectives.

The core of *Psychoanalytic Jurisprudence* consists of an argument that the application of behavior of individuals to various areas of law can serve to reconcile intellectual disputes and clarify thinking about policy questions. The argument is complex and is not always fully or clearly presented in Ehrenzweig's text; consequently I have undertaken to summarize it as I understand it. The principal assumptions Ehrenzweig makes are that individuals search for absolutes, such as "truth," "reason," and "justice," as buffers against anxiety; that in the course of their search they regularly perceive its futility; that they resolve the consequent gaps between their wishes and their perceptions through the creation of constructs, such as faith, power, and utility, that they use as principles around which their lives can be organized; and that they reflect the use of these principles in their social institutions. The organizing principles (Ehrenzweig calls them "meta-norms") constitute trade-offs between a collective yearning for what "ought to be" and a collective acceptance of what "is."

In applying these assumptions to four areas of the law—jurisprudence, criminal law, torts, and procedure—Ehrenzweig makes several imaginative generalizations. First, he maintains that the violent disputes perpetrated by scholars in jurisprudence, symbolized in the continuing controversy between adherents of the "positivist school" and disciples of the "natural law school," are the end products of different psychological responses on the part of members of the two schools, and when seen from that perspective can be reconciled. Both positivists and natural law advocates, Ehrenzweig argues, concede the existence of compromises between the potentially opposing ideals of order and justice; they merely choose to explain those compromises differently. Believers in natural law stress the "inner morality" of laws that are obeyed; positivists emphasize the ability of persons in power to impose their views on others. The choice of one explanation or another depends upon the organizing principle which gives an individual scholar optimum relief from anxiety. If jurisprudential opponents could fully appreciate the psychological basis for their positions, Ehrenzweig believes, semantic snarls could be unraveled and compromise achieved.

Ehrenzweig devotes more than half of his text to jurisprudence; he then turns, in shorter segments, to criminal law, torts, and procedure, again using his governing assumptions as critical tools, here directed at social policies rather than trends in scholarship. He finds that the criminal law in America has suffered from a clumsy use of its sanctioning powers. The clumsiness derives, he believes, from an inadequate appreciation of the different psychological character of various crimes. Certain crimes, such as violent murders, can only be committed if the offender overcomes strongly implanted repressions; these crimes (termed "oedipal") cannot easily be deterred, and are still sufficiently offensive to engender feelings of retribution among society at large. Other crimes ("post-oedipal") do not involve surmounting such deep repressions; they can better be deterred by legal sanctions. Hence retribution, but not deterrence, is a perfectly justifiable purpose of the criminal law in the oedipal area, while in the post-oedipal area policies of deterrence and rehabilitation can be simultaneously fostered through balancing protection for society against help for offenders.

Behind the confused application of criminal law sanctions, Ehrenzweig believes, lie deep-seated irrational attitudes about punishment; behind the equally confused application of the fault standard in the law of torts lie similarly irrational attitudes about moral blame. Again the insights of psychoanalysis lead to reform. Ehrenzweig advocates the abolition of the fault standard for all torts save those which demonstrate "moral fault" (p. 258), such as reckless driving or frivolous surgery. In all other cases, efficient distribution of the costs of risks should be the social goal; moral blame should be irrelevant.

Finally, Ehrenzweig finds a long-standing need in Anglo-American cultures to see judges as impartial, detached functionaries. This need manifests itself in a series of procedures, such as stringent, manipulable rules of evidence and decision making by juries, and these procedures in turn prevent judges from making effective use of their powers and penalize litigants who cannot afford skilled legal advice. A less rigid fixation with judicial detachment could produce, in Ehrenzweig's view, more informal procedures and more enlightened decisions.

As his argument unfolds, Ehrenzweig's perspective is consistently original and his insights provocative. But neither the structure nor the style of *Psychoanalytic Jurisprudence* does justice to the ideas it contains. Ehrenzweig says in his preface that he "had to write [the book] now or not at all" (p. 12), and his haste shows. Part One, on jurisprudence, is a general history of jurisprudential thought presented in so attenuated a fashion as to resemble an outline. Part Two, which contains chapters on crimes, torts, and procedure, is awkwardly related to Part One, both stylistically and analytically. The result is a book that vacillates between being a text and being a monograph, and ends up in an unfortunate no man's land. There are stylistic difficulties as well. Ehrenzweig continually substitutes neologisms for analysis, as if he believes he must coin a word ("beautness," "Quasi-Is," "psychosophy") to convey his meaning. Such coinages increase the already cryptic quality of Ehrenzweig's presentation.

In sum, the impression too often generated by this book is that of a series of penetrating but scattered insights, few of which are developed, interrelated, or reflected upon sufficiently to pierce the reader's consciousness. The impatience one feels is the kind that has consigned many a scholarly book to oblivion. I hope that will not happen in this case, for Ehrenzweig's observations are sufficiently original and interesting to deserve attention.

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Thomas Hobbes und der Puritanismus. By Winfried Förster. (Berlin: Duncker & Humblot, 1969. Pp. 243. DM 49,80.)

Hobbes-Forschungen. Edited by Reinhart Koselleck and Roman Schnur. (Berlin: Duncker & Humblot, 1969. Pp. 300. DM 18,60.)

The volume entitled *Hobbes-Forschungen* contains the contributions (some in German, some in French, some in English) to an international Hobbes-Kolloquium held in 1967 at the Institut für Politische Wissenschaft of the Ruhr University at Bochum. Though the dominant interest of the colloquium was the problem of Hobbes's significance for modern political science, the papers and discussions also emphasize the importance of a modern "ontology" (Riedel) and of a modern conception of creation (natural and civil) as a basis for Hobbes's ideas on contract, law, equity, and legitimacy. Even Hobbes's geometry and optics were attempts to explain the contrast between natural and civil types of "generation." The papers contain considerable discussion of the differences between mechanical generation, vital (organic) generation, and moral (artificial) generation by covenant, the generation of a public person and a "body politick." Several papers (especially Cattaneo, Villey, Polin) emphasize the importance of Hobbes's conception of science for the Enlightenment. Maurer's comments on the problems raised by the colloquium are especially worth careful reading.

One of the chief problems is the interpretation of Hobbes's "absolutism," for this idea is ambiguous. One must take seriously Hobbes's remark to Charles II, made shortly after the Restoration, when the monarch refused to grant publication rights for Hobbes's *Behemoth* (his history of the civil war) on the grounds that it was "incendiary." Hobbes took this occasion to say to him: I hope you will excuse me for defending you and your father by a double-edged sword. Evidently both Charles II and Hobbes were aware of what was implied in this remark. And any reader of *Behemoth* will note that it pays its respects not only to the cause of the Stuarts but also to the Commonwealth and to Cromwell as General and as Lord-Protector.

In this connection several members of the colloquium call attention to Hobbes's last work, the *Dialogue* against the common-law, but they fail to note that Hobbes's plea for the Royal Courts of Equity was a revival of the policy instituted by Francis Bacon as Lord Chancellor. No doubt this theme had been discussed on their "walks in the gardens" of Bacon's estate during the time Hobbes was Bacon's secretary. And the colloquium fails to note that the *Dialogue* closes with the theme that ever since ancient times English kings have exercised their sovereignty best "in Parliament" through the Royal Courts of Equity. There are other evidences in Hobbes's post-Restoration writings and sayings that indicate his disappointment when Charles II did not accept his crown as coming from the people (who had welcomed his return) in the spirit of commonwealth. Hobbes anticipated the Revolution of 1688.

The volume on Hobbes and Puritanism by Winfried Förster expands in detail and more precision the theme of his paper at the colloquium. He contrasts the differences between the theory of "covenant" as found in the writings of the early Separatist leader, Henry Jacob, (1616) and the theory of covenant in Hobbes. This raises a complicated issue concerning the meaning of "Puritanism." Hobbes was explicitly not a Separatist, as Milton was. Though for him the Covenant which binds citizens of a commonwealth to their government has the solemnity and sacredness of a "New Testament," Hobbes expected any genuinely "public" religion to be a function of the commonwealth. Therefore, he held that even and especially bishops must be responsible citizens. Hobbes had some sympathy for the Presbyterians in Scotland, where they had been established, but when Presbyterian preachers became itinerants in England, preaching to English workers during working hours, Hobbes despised them as agents for a Puritan establishment and a treasonous Long Parliament. Like the majority of Puritans in England, who were less Calvinist than those in Scotland, and who, both in the early days of Jacob and later when sects of independents multiplied, were in favor of reforms (parish by parish) *within* the Church of England, Hobbes was a loyal Anglican despite his open criticism of bishops. But intellectually Hobbes was a "latitudinarian" and refused to interpret the "church-covenant" as a covenant with "the spiritual body of Christ." For him, the membership in the Church of England was an aspect of his covenant as a citizen of the Commonwealth or the "body-politick," and he declared, "There are no spiritual bodies on earth." He held a very Protestant and Puritan theory of the New Testament "under God." To the bishops this was atheism; to Hobbes it was the political science of a Christian Commonwealth.

The volume on *Hobbes-Forschungen* contains a comprehensive bibliography of publications in German on Hobbes. Förster appends to his volume an international bibliography of recent works on Hobbes, with the exception of some important Italian contributions.

Both volumes, in their preoccupation with "absolutism," ignore what Hobbes himself regarded as his greatest achievement, his radical revision of the principles of natural law as formulated in Chapters XIV and XV of *Leviathan*. Here he formulates these norms as empirical, scientific generalizations of those modes of conduct whose *natural* consequences are "peace and security" rather than conflict. These "principles of natural justice" are related on the one hand to Puritan ethics, and on the other to later political science of the Utilitarians.

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The Mature Society. By Dennis Gabor. (New York: Praeger Publishers, 1972. Pp. 208. $7.50.)

The Mature Society is a difficult book for a political scientist writing for this journal either to praise or condemn. The gaps in the text occur primarily in areas where the political scientist imagines he has something to say: political institutions, constituencies for change and responsibility and power. Few political scientists are quoted and even they are not central to the argument.

At the same time, political theorists will find the diagnosis of the author, Nobel Prize winner in Applied Physics, who describes himself as a "gadgeteer," central to their concerns with man, human nature, political economy, freedom, education, and utopia. Professor Gabor contends that man, whose historic enemy has been nature, struggles today with the remaining enemy, man himself. His talents for the conquest of nature turn against him: aggressiveness, search for novelty, love of progeny, and loyalty to tribe and nation. Man is aware that consumerism and growth addiction—a stage in the rise and decline of capitalism foreseen by Schumpeter—bring little happiness. Yet except for four spheres of silent or open protest (the revolt of university youth, voluntary absenteeism, drug addiction, and crime), society's answer is that of St. Augustine: "Lord, make me good, but not yet." Nevertheless, Gabor predicts that the end is in sight and that the transition period we face will be neither stable nor happy. He sees the private sector reduced to one-quarter of the economy by the year 2,000 and confesses that "gadgeteers" such as himself must turn their efforts from hardware to social inventions if the autonomy of technology is to be defeated.

Despite the despair of his diagnosis, he is not without hope, in part because of his doctrine of man "wonderful in adversity, weak in comfort, affluence and security" (p. 47). Science can serve man as it has already demonstrated in "driving out superstitions, eliminating physical pain, relieving hormonal weaknesses, defeating poverty," though its advances intermingle good and evil as with heroin derived from morphine. In providing the requisites for a mature society, Gabor would add to the "intelligence quotient" an "ethical quotient" and link the two together in an IQ-EQ histogram for testing both intelligence and virtue. Since history is largely a tale of the misdeeds of power addicts, they would be excluded from power in the mature society. "Experienced educators will have no difficulty in recognizing the incipient power addict and warning society against him"—through forwarding his records. According to Gabor, education should be a lifelong process—permissive and loving to age six, based on a hard apprenticeship to age eighteen (and using three-dimensional, olfactory films to make students "viscerally conscious" of their history). Higher education would go on in two-track universities: one for the gifted minority, and the other the less gifted majority preparing, not for exacting professions, but for lifelong self-improvement. Everyone will receive education in parenthood including those who are to be discouraged as unworthy of or emotionally unprepared for families.

The mature society, then, is a modern utopia—a society stabilized in numbers and material production, ecological equilibrium, and responsible freedom. Its slogans will be, "Excellence instead of quantitative growth, possession instead of consumption" (p. 172). In an era of disutopias and doomsday books, it is heartening to have this word of hope. Standing as utopia, *The Mature Society* can throw the spotlight on neglected areas of truth. Yet, the author sees it as more than that—and at this point, the political scientist is plunged back into his sense of unreality and void.

How is this mature society to come into being, through what processes, interest groups and structures? A scant two and one-half concluding pages give us some clues, but little more than that. It will be Fabian and gradualist, based on compromise without hurting anybody and with citizens enlightening themselves as does a jury instructed by a learned judge. Its carriers will be not "technocratic mandarins" but an elite, nonetheless, of Gabor's "fellow futurists and other experts" (p. 173) designing projects for governments, parliaments, and public opinion who will back them as they did the Apollo projects. But will they? If, as Gabor states, "all the present trends of our world are against us: overpopulation, nationalism, economic group inertia and general aimlessness" (p. 174), who will provide the political power? Will it be the new generation? Apparently not, for "Our best potential ally, youth, is deeply confused." The one banner that continues to fly is that of creative intellects and futurologists—sufficient for a utopia but hardly for legislation.

Thus, not only is *The Mature Society* vulnerable to the criticism that it neglects politics; it also ignores the essential relation between intellectuals and power, a dilemma as old as history. Someone must fight for an idea within the decision-making structure. It will not do to write a memorandum and withdraw from the field. For within the democratic process, there are legitimate questions raised. Who will control the futurologists and planners? Can we be sure that this form or that of education will usher in the utopia? What do we know about the wellsprings of human behavior that lead us to think couples will resolve to have or not to have progeny because of their alleged worthiness, or emotional maturity? What basis is there for accepting the projections of the futurologists and basing policy on it?

To raise such questions is not to disparage a serious intellectual endeavor. It is merely to say that, from the standpoint both of psychology and of political science, Dennis Gabor has raised as many questions as he has answered. Fortunately, his vigor and intellectual vitality, even at seventy-three, suggest he can turn his attention to these issues in another characteristically challenging and original book.

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Behavioural Worlds: The Study of Single Cases. By P. G. Herbst. (London: Tavistock Publications, 1970. 248 pp. $8.00.)

The author deals with an issue of central importance to researchers in the social (behavioral) sciences: how to study individual cases in a scientific fashion. He proposes techniques for studying such cases so that principles emerge which are applicable to other single cases and yet do not eliminate the uniqueness of each (person or group).

The author argues that the social sciences have emulated the physical sciences in developing methods and theories with which to understand and study large numbers of cases, not single ones. Further, he claims that in the social sciences, both the relationships between behavioral variables and the measurement scales differ from case to case. Thus, his generalized behavior theory views each person and each group as a behavioral universe with its own laws and its own measurement scales. In contrast, in the physical sciences, the same functional relationships and measures are universally applicable to all cases.

Although I think Herbst overstates his case here—there must be at least one area of one social science where the measurement scales are the same across individuals—there is a ring of plausibility to his contention that the social scientist compared to the physical scientist has a greater interest in and need for theories and experimental procedures relevant to the study of single cases. Few techniques are currently available, even though, potentially, they could be of great benefit to the investigator studying individual psychotherapy or the scientist examining a given political system.

The book provides techniques developed in the process of performing four research projects within the social-psychological sciences. These dealt with a diverse range of topics: the work output of a group of coal-miners, subjective attitudes of high school students toward their courses, verbal communications of a courting couple, and labour turnover in two factories. The work on the coal-miners and the students is quite extensive, the hypotheses growing out of the theoretical constructs and dictating the data pertinent to their own evaluation. The other two techniques are more limited in scope. The study of verbal communications offers a data-collecting device, but no theoretical framework for the observations. The industrial research provides a mathematical model of a narrowly defined process within a factory but does not yield any methodological or conceptual innovations.

Two additional chapters contain theoretical ideas with no empirical underpinnings and are only remotely connected to single-case research. Better that these had been left out altogether and that fuller, more understandable exposition had been given to the techniques, particularly those dealing with the coalminers and the students. In describing the research on verbal communication, the author points out that for purposes of clarity, redundant, self-evident phrases are often dropped from written documents. I believe Herbst goes too far in removing redundancy from his written document. Little in the book is self-evident. The rationale for choosing one type of analysis or one set of definitions is never provided and would have been welcome. Although Herbst worked on the coalminers project for five (!) years, the reader sees only an inkling of the process of discovery, of trial and check. Rather, one is given the finished product, a *fait accompli,* or *post hoc* answers.

Herbst's two main techniques grow out of his Generalized Behavior Theory. This theory does not provide a universal set of behavioral principles, for the author claims none exist. Rather, for a given situation, the theory yields a way of generating all possible behavioral universes that can exist and of excluding those which cannot. Implicit here is the idea that a theory which can handle all outcomes is useless and unscientific, and that a theory becomes more valuable the more restrictive and specific it is. His theory consists of a set of postulates (which specify the dimensions and possible scale-transformations of variables in a behavioral system), of some definitions (e.g., internal variables are related to the internal functioning of a behavior system, transactional variables deal with the relationship between the behavior system and its environment), and of methodological implications (e.g., all measures must refer to the same observational time period during which steady-state conditions obtain for all parameters). All of these restrict the range and kind of theoretical predictions and the types of method to be used in collecting and analyzing data.

In my judgment, the restrictions are too extensive. An example: as a result of the postulates, all functional relationships must express the value of one behavior variable as a function of two others (no more, no less), no matter how many may be operative in a situation. I think a number of interesting possibilities, applicable to any number of situations in which a scientist would study single cases, are thereby ruled out.

Turning to the research for which the theory and associated techniques were initially developed: In the coal-miner study, one working group was considered as the single case. Six variables pertinent to the work task were chosen, and the measurement scales appropriate to each were selected. The possible functional relationships were derived using the definitions of the variables and the postulates of Generalized Behavior Theory. Four of the six basic findings of correlational analysis were compatible with one or the other of the functional relationships. That is, most, but not all, of the data could be fit by some theoretical prediction.

The variables appear to have been selected and defined after careful examination of all the data. The value of such a post hoc analysis can only be demonstrated if the derived functional relationships also accounted for the behavior of another independent group of coal-miners. No such replication is reported.

In the research on student attitudes, a comparison was undertaken of the results found in one case with those found in other cases. The analyses indicated that a restricted number of functional relationships accounted reasonably well for the data of three students, who all performed quite differently. One must add here, however, that seven variables were omitted from the detailed analysis, some because they demonstrated no simple relationship with other variables, which I take to mean no simple relationship that could be derived from Generalized Behavior Theory. Also, data from only 10 per cent of the students were found to be suitable for analysis, even though no explicit reasons were given for the exclusions. At the very least, the elimination of half of the variables and 90 per cent of the subjects severely limits the generality of the methods used and the conclusions reached.

I do not think that generalized behavior theory yields techniques which adequately deal with the data for which it was developed, not to mention other single-case data. The other two techniques in the book have more narrow and specialized foci. Whether they can be applied to other research situations is open to debate.

In general, Herbst does not manage to avoid the difficulties that lie in the way of performing research on single cases. Inadvertently, he highlights the potential pitfalls, and this may well be the book's major contribution.

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Man and Citizen. By Thomas Hobbes. Edited with an introduction by Bernard Gert. (Garden City, New York: Anchor Books, Doubleday & Co. Inc., 1972. Pp. 386. $2.95, paper.)

Disguised by an uninformative title, which makes it sound like Son of *Body, Man and Citizen,* the paperback selection of extracts edited by Richard Peters (New York: Collier, 1962), this volume is in fact a complete reprint of *De Cive* in Hobbes's own English translation, plus the first English translation of the last third of *De Homine,* made by the editor and two colleagues. As a bonus, the front cover has a nice color reproduction of a portrait of an obviously balding but still quite hairy Hobbes which ties in closely with Aubrey's description, including the yellowish-reddish whiskers naturally turned-up ("a sign of a brisque witt") and the beard under the lip, reduced to a "little tip." ("He desired not the reputation of his wisdome to be taken from the cutt of his beard, but from his reason.")

Stocks in Hobbes, virtually unmarketable during his lifetime and for another century and a half after his death, continue their spectacular post-1950 rise. With three paperback editions of *Leviathan* in print now joined by this edition of *De Cive* it is to be hoped that other publishers will at last recognize the goldmine of pungent prose and lucid argument that is the eleven volumes of Hobbes's *English Works* in Molesworth's early Victorian edition. Can we look forward at least to paperbacks of *Behemoth* and of *Liberty and Necessity* in the near future?

These two would indeed seem to me to make better propositions for paperback publication than the present one, since they are of great interest in their own right. *De Cive* is mainly of interest to the specialist or at any rate the enthusiast: treated as a first (or more precisely second) draft of *Leviathan,* it lets us see where Hobbes changed his mind as his thoughts developed between 1642 and 1651. (In particular, he becomes more consistent in sticking to his definition of "obligation" in terms of the logical consequence of a covenant instead of also having a category of "natural obligation" as the effect of irresistible power; and he introduces into *Leviathan* the subtle and important discussion of authorization and representation which has no counterpart in *De Cive.*) *De Homine* is of rather marginal interest, even to Hobbesian scholars. It is supposed to form the first part of a trilogy, of which *De Cive* is the last. As Hobbes explains in the Preface to *De Cive,* this third part of the planned trilogy was "ripened and plucked" by the disputes which led to the civil war: "Therefore it happens, that what was last in order, is yet come forth first in time" (p. 103). The other two parts had to wait a long time, for *De Cive* (1642) was elaborated into *Leviathan* (1651) before Hobbes produced the first part *De Corpore* (1655) and the second part *De Homine* (1658). Peters says of it that it "contained little that Hobbes had not published before" (*Body, Man and Citizen,* page 17) and as far as I can see this is correct. Moreover, the lapse of years inevitably meant that *De Homine* does not fit neatly on to the front of *De Cive.* The last chapter of *De Homine* is "On Artificial Man" and gives a highly condensed account of the *Leviathan* doctrine of authorization and representation, but (as I have already mentioned) this is not picked up in *De Cive.* "[To] fasten *De Homine* and *De Cive* cleverly together requires a little more knocking and hammering" said one of Hobbes's earliest and wittiest critics, John Eachard, in his *Mr. Hobbs's State of Nature Reconsidered* (1672).

Unfortunately, Doubleday chose to present the volume in rather different terms. The back cover says:

> This collection of Thomas Hobbes's writing on *Man and Citizen* offers the best representation of his moral and political thought available in English. It includes the important second section of *De Homine,* translated here from the Latin for the first time, in which Hobbes expresses his most mature thoughts on human nature and morality. And it includes the only complete and readily available English translation of *De Cive,* which is superior to the popularly studied *Leviathan* as a statement of Hobbes's political philosophy.

Professor Gert says much the same in the first paragraph of his Introduction, and adds in support of the view that *De Cive* is "superior to [*Leviathan*] as philosophy" the following:

> For example, in Chapter 14 of *Leviathan,* definitions of The Right of Nature and A Law of Nature are abruptly offered with no explanation of their aptness. However, in Chapter 1, Sections 7-10 of *De Cive,* Hobbes provides reasons for his definition of The Right of Nature and in Chapter 2, Section 1, he provides reasons for his definition of A Law of Nature. Thus, *De Cive* provides a much clearer account of these crucial concepts than *Leviathan* (Page 3).

Even if these examples were well taken they would hardly suffice to meet the point that *Leviathan* contains many extensions and improvements on *De Cive,* but it is not even clear that they do show superiority for *De Cive* in its definitions. Hobbes stated in *De Corpore* his view of definitions as follows: "Definition of a Name. A NAME is a word taken at pleasure to serve for a mark, which may raise in our mind a thought like to some thought we had before, and which being pronounced to others, may be to them a sign of what thought the speaker had, or had not before in his mind" (Chapter 2, Section 4). Thus, insofar as Hobbes may give the impression in *De Cive* that he is *giving reasons* for his definitions this is merely an indication that he had not yet worked his way through to the uncompromising nominalism of *De Corpore* which is exhibited in the treatment of definitions in *Leviathan.*

David Gauthier, in his study *The Logic of Leviathan* (London: Oxford University Press, 1969) says about A. E. Taylor's view that *De Corpore Politico* (the first draft) and *De Cive* (the second) are superior to *Leviathan* as statements of Hobbes's political theory that it is "one of the more curious aberrations of Hobbesian scholarship" (p. 120). Unless Professor Gert can produce much better arguments for the superiority of *De Cive,* and also explain why the major changes between it and *Leviathan* are not *philosophically* improvements, I think that Gauthier's judgment must stand. These arguments for the superiority of *De Cive* would presumably be put before professional peers in a recognized scholarly forum, and this question of context is important. An article by a literary critic arguing without qualification for the proposition that Dostoevsky never again reached the literary heights of his first novel, *The Double,* would be a stimulating exercise; but in writing an introduction to a paperback edition of *The Double* he would surely have some responsibility to make it clear that, although he believed *The Double* was the best thing Dostoevsky ever did, most competent critics thought otherwise.

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Foundations of Political Sociology. By Irving Louis Horowitz. (Harper & Row, New York, 1972. Pp. 590. $12.00.)

Foundations of Political Sociology "is at its core a statement about the transformation of a past-oriented series of reflections into a goal-oriented series of prescriptions" (p. xv). This "fundamental" transformation reflects the shift from the nineteenth-century Political Economy approach to social problems based on history to a twentieth-century political sociology approach based on policy demands. Accordingly, the "economic" factor has given way to the "political" factor in the construction of explanations of social phenomena. Specifically, "the political sociology of growth is the central organizing pivot for any modern discussion of social systems" (p. xviii).

Describing the emergence of political sociology, Horowitz argues that political science cannot be defined according to a set of theoretical propositions, but rather is unified by the traditionaly elitist response of political scientists to the needs of those who rule. Sociology, however, regards the masses, large-scale events, and impersonal forces in formulating its account of political behavior. The marriage of sociology and political science gives birth to political sociology, which explains social action in terms of the interdependency of elites and masses, personal factors and impersonal forces, etc. Historically, Horowitz maintains that political sociology was spawned by the failure of elites to provide political solutions to the social problems brought to consciousness by the activity of the masses in the French Revolution. The author's commentary on these social forces and political ideologies as they affect contemporary American society constitutes the major portion of *Foundations.*

Horowitz observes that "the conflict of interests from a practical point of view is now a struggle between liberalism and socialism" (p. 107). With respect to liberalism, he finds "little historic correspondence" between that ideology and "its presumed capitalist economic base," i.e., the bourgeoisie. Instead, "the rise of liberalism corresponds with the rise of the political enfranchisement of the masses" (p. 132). Liberalism is "essentially linked to the belief that each person should count as one irrespective of his property holdings . . . that in the political arena one citizen is as important as any other citizen" (p. 134). Except for Bentham, it would be difficult to uncover a theorist who approximates this ideal typical description of liberalism. The author mentions J. S. Mill, but Mill never held any such view, and in Mill's case, there is a fairly clear relationship between the political ideology of liberalism as he defined it and the "economic base" of the middle class.

Liberalism was from its inception elitist-oriented. That this elitism assumed the form of opposition to a declining aristocracy on the one hand, and opposition to the threat of a politically active working class on the other, provides the historical basis for understanding the essential *practical* expression of liberalism: "fragmented planning" (p. 107). Given its ideological commitment to progress and evolution, the fate of liberalism in America depends upon "assumptions about the long-range viability" of the American political system (p. 144) and the ability of technocrats to sustain an apolitical bureaucracy. Thus, the depoliticalization of politics—absence of ideology plus rule of the expert—constitutes the essence of twentieth-century liberalism.

Horowitz identifies a two-pronged challenge to the hegemony of contemporary liberalism. Ideologically, the most serious threat stems from Marxism. Politically, fascism has proven itself an organizational alternative to liberalism capable of functioning within a capitalistic economy.

In opposition to nineteenth-century liberalism, Marxist theory made the masses the prime mover of politics, and, at the same time, it replaced the economic determinism of classical political economy with an emphasis upon the necessity for political organization. In other words, Marxism provided an ideological framework for political sociology by redefining politics in terms of the social needs of the masses. Nevertheless, Horowitz maintains that Marxism collapsed as an ideological alternative to liberalism precisely because it succeeded politically in the Soviet Union. Russian Marxists, he asserts, resurrected the elitist conception of politics (the party), reduced ideology to the rote memorization of Marxist-Leninist scripture, and sacrificed the social needs of the masses to the imperatives of industrial development. The result is bureaucratic socialism and the depoliticalization of Soviet society.

Fascism, Horowitz argues, represents "an advanced stage of industrial capitalism" (p. 246), and must, therefore, be considered a fact of contemporary political life. This argument is premised upon the priority fascism grants to the state as the determinant of social development, its success as a twentieth-century mass movement, and its reliance upon a militaristic bureaucracy. Its specific doctrines vary considerably, but "the idea that classes can be managed without a revolution is the essence of fascism" (p. 247). Bureaucratic regulation of class conflict by the state in the interests of a military-industrial complex supported by appeals to nationalism and racism is the hallmark of fascism, a definition uncomfortably close to some current descriptions of American political society.

Horowitz concludes that a pronounced drift toward fascism rather than to the left is more likely in the event of an economic crisis. Moreover, the Left can no longer depend upon the proletariat to lead the socialist revolution. Does this mean that liberalism is in fact endowed with long-range stability? No, Horowtiz argues, because the line between social deviance and political marginality is fading. This means that "radical changes in the structure of American social and political life are imminent" (p. 363). This "fusion between deviant behavior and political processes" is "a major event" because it makes "each man a potential agent of revolutionary change" (p. 574–5).

Thus, despite the marginal impact of Marxist ideology, the absence of a mass-based social movement with effective political leadership, and the general restraint of the New Left as regards the use of violence to achieve political goals, Professor Horowitz nevertheless insists that "the possibility of insurgency must be taken seriously" (p. 293). Indeed, we are already in the "first stage" of insurrection. Marginal men may have had their viewpoints overlooked by historians, as Horowitz believes, but it is also true that marginal men—especially intellectuals—tend to mistake rhetoric for performance.

It is in this context that I read the author's proclamation that political sociology "must be ranked a foremost innovation of social science in our age" (p. 17). Yet, as Horowitz admits, most social scientists serve to legitimize the status quo, and the instances in which political sociology has successfully broken through the ideological barrier erected by traditional social science are few. Political sociology is still as marginal to social science as the social actors it studies are to the political system. Put another way, political sociologists have not yet done enough of the in-depth substantive investigation and analysis of social phenomena necessary in order to make an effective assault upon the abstract empiricism of social science.

Foundations of Political Sociology, while a useful compendium of interesting insights, is, ultimately, a disappointing book. Instead of "foundations," we are presented with an atomistic approach to sociological ideas, a superficial analysis of contemporary social problems, and dramatic prognostications by the author. The success of political sociology and radical social change require a great deal more than this.

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The Unknown Dimension: European Marxism Since Lenin. Edited by Dick Howard and Karl Klare. (New York: Basic Books, Inc., 1972. Pp. 418. $12.50.)

The Unknown Dimension is a large and expensive reader devoted to the explication of the ideas of lesser-known 20th century Marxists. It is edited by two American academicians, and most of the articles are written by Americans or by Europeans living in the United States. The sixteen chapters are not reproductions of the works of these lesser-known Marxists, but are for the most part recent articles written about them for this volume—articles extensively documented, occasionally critical, and often sympathetic.

The first two chapters differ in that the first introduces the subject from a "New Left" or neo-Marxist point of view, and the second seeks to provide the historical context in which the subjects of the book fit. These two chapters amplify the stated purpose in the Preface: "This book was conceived both as a contribution to intellectual history and to the study of the development of Marxism, and even more as a political project." The project mentioned is providing leftist intellectuals with a greater variety of "Old Left" thinking, so that a new left might build on a richer Marxian heritage.

This is a laudable undertaking. University teaching in this field, either because of time pressures or laziness, often only hits highlights that leave an impression in the student's mind either of a canonized Marxism or a Marxism entirely chanelled into the Communist Party of the Soviet Union and later branches, such as the Communist Party of China. This book shows both the superficiality of that approach, and that, although less potent, the European 20th century was rich in alternatives to Bolshevism. Some of these alternatives are better known than others; quite possibly this will differ with each reader. The list includes Georg Lukács; Ernst Bloch; Karl Korsch; Antonio Gramsci; the Council Communists, such as Anton Pannekoek and Hermann Gorter; Wilhelm Reich; The Frankfurt School of Herbert Marcuse, Eric Fromm, Leo Lowenthal, and Theodore Adorno; Walter Benjamin, Jurgen Havermas; Jean-Paul Sartre; Henri Lefebvre;

Galvano Della Volpe, Louis Althusser; Serge Mallet; and André Gorz.

Some of these individuals represent sharper alternatives to Bolshevism than others, as for example Gramsci as opposed to Lukács; and some present alternatives that might carry little appeal—such as Ernst Bloch's mixture of Marxism with Christian eschatology, Jewish mysticism, and neo-Hegelianism. (If what we are about is the redemption of the earth, we have not progressed much beyond the ancient Essenes and Sons of Light.) Throughout the volume, however, a fairly consistent theme is that a proletarian revolution made without a revolutionary-conscious proletariat is doomed as a source of hope for the worker or for people in general. This principle is extremely important. Why has this consciousness not yet developed sufficiently? *The Unknown Dimension* suggests answers—e.g., Reich's notion of sexual repression, or Marcuse's thoughts on one-dimensionality; but these answers are not as pragmatically satisfying as Lenin's perception of the necessity of a disciplined Party, or his argument from imperialism that capitalism had received a temporary extension of life.

This sort of discussion is very useful because it demonstrates quite effectively that Marxism is not a closed canon—a dogma to be accepted or rejected. There are some problems in Marxist thought that have as yet resisted solution. New interpretations, new ideas, and new applications are needed to vitalize Marxism as a stimulus to discussion and a spur to critical analyses of contemporary society. This does not mean, of course, that older ideas were always wrong. Unfortunately, *The Unknown Dimension* suggests this, and implies a dogmatism to which the New Left as well as the Old is not immune:

> . . . What is known and transmitted to us as "Marxism"—by academic writers and the orthodox leftist parties alike—is an institutionalized ideological artifact emanating from the historical trajectory of Marxist movements, one that bears little resemblance to the assumptions and inherent potentials of Marx's own intellectual project (p. 4).

Parts of the book I found particularly instructive, including the section in which the Council Communists very clearly described the suppression of the workers' movement in Russia as a bourgeois effort, and blasted the Comintern as the servant of the ruling Soviet elite. This would have been a good place to discuss the practicality of the worker council notion, citing perhaps Yugoslav data, but such was not done. Another instructive example was provided by Della Volpe's reminder that descriptive notions such as the dialectic can become abstractions that have a life of their own, hypostasizing the very reality they purport to describe. Della Volpe calls for a scientific revolutionary theory, experimental, and verifiable by practice. The closeness of this view to Mao's position could have been mentioned, but was not. In fairness to the editors, the omission of Yugoslav and Chinese material is deliberate. They are describing the *unknown* dimension of Marxist thought, and neither Yugoslavia nor China fit that category.

The final chapter dealing with the recent work of Mallet and Gorz is provocative. Just as one's view of economic determinism can be too rigid, so also can one's analysis of the working class be woefully out of date. Is the working class of 1973 the same as it was in 1850? Mallet answers that it is not, that the working class has undergone a triadic development which demands new analyses in order for revolutionary theory to have concrete applicability. Is a Party still necessary? André Gorz replies that it is, but adds that it may well be a necessary evil. This is an heuristic notion because similar words were used by Engels to describe the state power which the proletariat would inherit.

Professor Howard interestingly contends that it is not enough to demonstrate socialism's superiority over capitalism by showing that it can do the same things as capitalism although quantitatively better; it must be shown that socialism is qualitatively superior. This is applicable to theory, but it is also something to recall when describing allegedy socialist societies. We find it easy to compare GNPs and rates of growth between, for example, China and the United States. A much more difficult question is the possible qualitative difference which is much more important to ascertain.

The book is long and somewhat easy to put down, but the format—self-contained chapters of about 25 pages—makes such piecemeal reading justifiable. By whom will it be read? Certainly by professors and graduate students, but unless they have substantial preparation, undergraduates will find it heavy going. In part this is due to the specialized character of the book, but it is also due to the specialized language often used. We very much need synonyms for such words as reification, hypostasization, and objectification; and we need to use them if the purpose of this book, i.e., reaching a wider audience, is to be realized.

The Unknown Dimension is one of those

readers that is not simply a "scissors and paste job." It is informative, heuristic, and challenging. It deserves to be read and used.

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Social Science and the Idea of Process: The Ambiguous Legacy of Arthur F. Bentley. By Paul F. Kress. (Urbana: University of Illinois Press, 1970. Pp. xiv, 260. $8.95.)

Professor Kress evaluates the intellectual contribution of Arthur F. Bentley and relates it to intellectual developments in the social sciences of the past century. The subtitle forecasts one element in his conclusion, but it indicates only modestly the complexity of the issues. Professor Kress thinks of his essay as ". . . an inquiry into that shadowy area which lies between the philosophical and methodological realms . . ." (p. xii). Contemporary philosophical literature provides the materials for this part, but the "methodological realm" that he explores is more esoteric than that covered in the usual books on methodology in political science. The methdological problems are among those that have torn traditional philosophy to shreds, and it was these problems that disturbed Bentley for all of his intellectual life.

Professor Kress makes it clear that while *The Process of Government* of 1908 is usually cited as the formulation of one branch of what may generally be labeled *group theory,* concepts articulated there went deep. Bentley's premises were not clearly specified in 1908, and his statement of pressures led many writers into superficial and misleading propositions. Kress makes it clear that the criticisms Bentley leveled at contemporary social science in Part I of the *Process* challenged the fundamental philosophical and logical bases on which most social science proceeded. His criticism challenged the implicit or explicit reifications of social analysis and appealed to a deeper or underlying body of observable activity. Bentley suggested that this underlying body of human activity was the "stuff" of politics and government and that it should be the substance of social science. This more fundamental body altered the significance of the activities of government as revealed in the usual legislative, executive and judicial structures. Part II of *Process* is a tentative formulation for utilizing the traditional governmental institutions to search out these underlying activities. These activities were pressures seeking and often finding expression through the formal institutions. Bentley attempted some studies of this kind that are similar to some contemporary pressure groups studies and are what Kress designates as the minimum program of political research within the Bentleyan analysis.

Political scientists seldom explore the later writings of Bentley, although many have examined *Relativity in Man and Society.* These later writings are primarily devoted to the problems related to the study of the characteristics of process, the adequacy of existing language to describe process, and the sociological, scientific and philosophical requirements of a satisfactory means of describing social activity. Process is the central theme, for it is the concept by which Bentley hoped to unify "endurance and change," a problem that ". . . has perplexed the western mind from its earliest ventures into the mystery of reality" (p. 3). Most of his writings are geared to that effort. Kress provides a detailed account of that effort, as well as the efforts of sociologists, psychologists, and a few philosophers to deal with the issues.

A critical issue is how to state the data of social activity in a descriptive universe that is only process—in short, how to overcome the dualism that permeates most western philosophy. The verbal forms of the dualism are many: endurance-change, form-substance, stability-dynamism, and others. The intellectual effort that has gone into the search for a solution has persisted since the 5th century B.C., but as Kress's discussion demonstrates, the effort is still with us. If change is admitted to occur, in what does the change occur?

Kress states in his preface that . . . "Process can be considered on several levels" and "has almost infinite dimensionality and an inexhaustible literature." In order to recount the role of process in social science, Kress needed a "focus relevant to theoretical social science" (p. xii–xiii). Bentley's writings became the vehicle to carry the story of the idea of process in the social sciences. The selection is apt, for Bentley devoted his full attention to the matter. As to his accomplishment, Kress observes that

> Arthur Bentley's transactional analysis dissolved history, nature, and man in the universal process and posed a profound challenge to theoretical reconstruction. The conclusion of this analysis is that his own attempts failed, but political scientists as a whole have not yet confronted the demands of the process idea, and perhaps with good reason. It may well be that such demands, at least those of the maximum program of full transactional statement, are beyond human capability, and even if realizable, incur the prohibitive cost of impoverishing our experience (p. 253).

Professor Kress provides for his readers an accurate presentation of the theme in *Process* and a commentary on later studies by political scientists that purport to follow the Bentleyan proposals. These studies, Kress remarks, approximate the minimum research program deriving from the process analysis.

After preparing the way, the author recounts the uses of process in American sociology, and he concludes, "Insofar as American sociologists who have employed the process concept believed the term to be something more than a metaphor, they adopted either the evolutionary or analytical version . . . not surprisingly, Bentley did not find these sociological formulations helpful" (p. 129).

A lengthy chapter, "Process as Transaction," is devoted to expounding this important idea in the later Bentleyan intellectual scheme. A transaction is distinguished from self-action by an individual and from interaction between independent entities. Interaction can refer to action between selves, but sometimes it may have as its referent selves adjusting their behaviors to the existence of others. The latter notion is involved in transaction: with it Bentley sought to solve the fundamental problems suggested earlier. It is a complex concept, and the chapter contributes greatly to grasping its importance in transcending dualism. Kress concludes that Bentley was not successful.

A chapter on "Process and System" examines the capacity of equilibrium and system to meet the questions that Bentley had leveled at social or political research structures or both. Neither of the concepts survives his critical analysis, and he suggests that "field" or "complementary" theories may be more useful in dealing with the fundamental methodological problems in the social sciences.

Professor Kress has examined carefully a large body of contemporary social science literature vis-à-vis claims to be found in Bentley's writings. In his examination he has been guided by many of the propositions of contemporary philosophy. His conclusion is that the social science literature has not been able to withstand the methodological critiques. His book is a substantial accomplishment.

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Medieval Political Philosophy. Edited by Ralph Lerner and Muhsin Mahdi. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 532. $4.95.)

The volume presented by Professors Lerner and Mahdi contains translations of twenty-five selections from seventeen different authors who lived between the ninth and fifteenth centuries and wrote on political philosophy. Several of the selections have never before been translated into English or any other modern language and some have never before been edited. Although many of the authors are unknown in the West today, most of them extensively influenced the formation of modern Western political philosophy. Machiavelli, Descartes, Spinoza, Hobbes, and Locke were familiar with many of these authors or with the writings of the schools that arose to defend or to combat their teaching. This book is such an excellent introduction to those medieval political philosophers so important to our own understanding of contemporary political thought that we are fortunate it has been reissued in a paperback edition.

In addition to presenting a very important book, Professors Lerner and Mahdi have presented a very scholarly book. Some of the most competent scholars in Arabic, Hebrew, and Latin studies have contributed to the volume and have thus brought their deep understanding of the different authors to bear on their translations. All of the selections are edited carefully and accompanied by judicious footnotes which identify obscure references or indicate alternative textual readings. Moreover, there is no appearance in the book that numerous hands participated in its composition. To the contrary, there is a uniformity of editing and translation throughout.

The two editors have also tried to make their book readily accessible to those not familiar with medieval political philosophy. Intelligent prefaces to each selection provide basic information about the author, identify the context of the selection, and explain some of the questions treated in the selection. Moreover, Professors Lerner and Mahdi have preceded the selections by an excellent introduction to the subject matter. They explain what political philosophy is and what is characteristic of medieval political philosophy. With gentleness, they alert the reader to the peculiarly modern preconceptions which might prevent an appreciation of medieval political philosophy. They then indicate the fallacies in those preconceptions and suggest how the selections can be read with the greatest profit.

Such an introduction is essential, because many features of medieval political philosophy make it almost incomprehensible to the modern reader. For example, these writings are not presented in the same way as most modern writings in political philosophy. Thus, superficially, Ibn Tufayl's delightful tale, *Hayy the* *son of Yaqzan,* appears to be a precursor of the romantic call for a return to the solitary life. Only after much probing does that veil fall away to reveal a tenacious inquiry into the relationship of revelation to unassisted human wisdom, into the problems of how to rule people of varying intellectual capacities, and into the dilemma between a wise man's duty to his fellow men and his desire to pursue his own thoughts. In a similar way, Maimonides' *Letter on Astrology* first appears to be a very pious warning against taking astrological predictions seriously. On closer examination, the reader notices that Maimonides' argument leads to intriguing reflections about the relationship between human freedom and divine guidance of the universe.

Another reason that medieval political philosophy tends to be incomprehensible to the modern reader is that it appears to be too staid and to lack originality. For example, we have heard so much today about the slavish character of scholastic commentary that we are poorly disposed to St. Thomas Aquinas' *Commentary on the Ethics* or *Commentary on the Politics* and are likely to prejudge either work as simply repeating Aristotle's thoughts. Thomas spoke so respectfully of Aristotle that a first reading of the text does little to change that prejudice. Because much of Thomas's interpretation is directed to drawing out the implications of some of Aristotle's remarks or ordering Aristotle's arguments, it is not easy to avoid the impression that he has added nothing new. When the reader takes the time to read Aristotle and Aquinas together, however, the deeper value of the commentary becomes evident. Some of Aristotle's ideas are altered and others pushed to stronger conclusions than is consonant with the explicit character of Aristotle's text. More important, one begins to understand more about what both Aristotle and Thomas were trying to do; one begins to think about these very difficult issues with them and to argue with them. At that point, it becomes evident that our current praise of originality is based on an extremely superficial understanding of philosophic thought.

Yet another feature of medieval political philosophy that sometimes makes it incomprehensible to modern readers is its intense involvement with revelation and religious law. Professors Lerner and Mahdi have underlined the importance of religion in medieval political philosophy by arranging the authors according to their particular religious community: Islam, Judaism, or Christianity. But their acknowledgment of the importance of religion is not an affirmation that these authors simply reflected the doctrines accepted in their particular communities. The problem is infinitely more complex. Medieval political philosophers recognized the political advantages of presenting most citizens with clearly defined laws whose wisdom could be vouchsafed by appeal to supernatural authority. At the same time they sought to investigate the merits of those laws, to resolve the problems posed by the doctrine of prophecy or divine guidance, and to understand the meaning of revelation in general. Since they did acknowledge the merit of appealing to divine laws in political rule, they did not express their thoughts in ways that would threaten those laws. Averroës, for example, justified philosophy before the tribunal of Islamic law, all the while suggesting that philosophy could provide a fuller explanation of the very issues the law defined. Still, if we are to appreciate the spirit of inquiry which prompted Averroës and other medieval political philosophers, we must make the issue our own and place most of our emphasis on understanding why they arrived at their individual positions. Like the medieval political philosophers, we must be willing to take the claims of religion as seriously as we take the claims of reason or science.

In sum, the ultimate value of studying medieval political philosophy is that it forces us back to a reconsideration of our own opinions by reminding us of issues that we no longer investigate. It comes from a period remote enough in time and sometimes from distant enough places that we perceive no immediate threat. Yet because medieval political philosophy continues the pursuit of classical political philosophy within a world dominated by the revealed religions, it is close enough to speak to our most pressing concerns. Professors Lerner and Mahdi have made it possible for us to examine those concerns anew by presenting the writings of some medieval political philosophers in a scholarly and philosophic manner.

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The Nihilistic Egoist: Max Stirner. By R. W. K. Paterson. (London, New York, Toronto: Oxford University Press, for the University of Hull, 1971. Pp. xiii, 322. $14.75.)

Max Stirner, (the pen-name of Johann Caspar Schmidt [1806–1856]), won a fleeting fame in Germany as a philosophical writer in the 1840s, only to disappear into oblivion a few years later. *Der Einzige und sein Eigenthum,* his best-known work and the source of his brief renown, appeared at the end of 1844 and was published in the United States in 1907 in an English translation under the clumsy title *The Ego and His Own*. The passing interest in Stirner among the English-speaking readers was mainly due to the circumstances of the times rather than the inherent merits of the work. As some philosophers and historians of philosophy (such as F. A. Lange and Wilhelm Windelband) emphasized, Stirner marked the beginnings of a growing trend toward a boundless individualism in modern philosophy which culminated at the turn of this century in the enormously influential works of Schopenhauer and Nietzsche. The re-edition of the English translation in 1963 (New York, Libertarian Book Club) also seems to be attributable more to the interest in other writers than in Stirner. Martin Buber (*Between Man and Man,* Part II) and Karl Löwith (*Von Hegel zu Nietzsche*) tried to establish certain points of contact and repulsion between Stirner and Kierkegaard (1813–1855), and later some other contemporary writers pointed out that Stirner had been not only the last link in the dissolution of Hegel's philosophy in the 19th century but also the first link in a new chain of intellectual debates that eventually led to the rise of atheistic existentialism in the 20th century. Mr. Paterson, whose book is the first comprehensive presentation in English of Stirner's life and philosophy, subscribes to this school of thought. As he writes in the introduction (p. viii), his main objective is to reappraise Stirner's contribution to the development of European thought, made possible only by the rise of existentialist philosophies in Europe over the last forty years.

Mr. Paterson's purpose accounts for the outline and content of *The Nihilistic Egoist*. Of the three parts into which the book is divided, only the first deals with Stirner's philosophy in its historical setting and philosophical context. The remaining two are concerned with the alleged or genuine legacy Stirner passed on to the 19th century, and the detailed examination of his philosophy within the perspective of existentialism. The author seems to assign greatest importance to the last part of his work, treating the two preceding parts as a preparation for the accomplishment of his ultimate task.

Considered in the context of its time, *The Ego and His Own* is by no means an outstanding philosophical treatise. It is very much a polemical tract of transitory significance, excessively long and repetitive. It lacks philosophical sophistication, indulges in obscure pronouncements, and fails to produce arguments in support of its extreme claims. Its historical importance lies in its anthropological rebellion: it defends the concrete human person against Hegel's philosophy. In this respect Stirner both continued and extended the work of other Young Hegelians, by asserting the unconditional primacy of the concrete individual (*der Einzige:* the Unique One). Stirner consistently contrasted him not only with Hegel's vertiginous abstractions but also with Feuerbach's Man and Bauer's thinking subject or self-consciousness. He rejected everything that was placed above the individual and that limited the will of the Ego. "Egoism" in Stirner's sense is not the opposite of "unselfishness," but meant the uniqueness and irreducibility of the individual to anything else, and involved the repudiation of all doctrines and all authority. Thus, he aroused equally the hostility of all his contemporaries in Germany, of the followers of Bruno Bauer's critical philosophy, and of Moses Hess, Feuerbach, and Marx.

Historians of social and political thought are inclined to present Stirner as the first exponent of anarchist individualism. It is true that for Stirner the state involves servitude and subjection; every state is a depotism and its sole purpose is to limit the free activity of the individual and make him subject to one generality or another. But anarchism is something more than a repudiation of the state. G. D. H. Cole in his *History of Socialist Thought* divided the Anarchists broadly into two groups—the individualists who are anxious to do without any social organization whatsoever, and the collectivists (or anarcho-communists) who combine the rejection of the State with a strong belief in co-operation based on moral principles. Since practically all Anarchists sympathise with or espouse collectivist anarchism and since Stirner alone seems to belong to the individualistic trend, Mr. Paterson has rightly concluded that it would be futile to put Stirner among anarchist thinkers; the similarities between them and Stirner in their attitude toward the state conceal insurmountable differences and incompatible objectives.

The long third part of *The Nihilistic Egoist* is concerned with the proposition that *The Ego and His Own* acquires new significance when read from the vantage point of existentialism. But Stirner's affinities to existentialism are conceptual rather than substantive; they conceal once again a deep-seated difference of response and concern. Regardless of its merits, this comparative examination adds little to what we know about Stirner's philosophy. Thus, Mr. Paterson's useful, erudite and welcome study seems to confirm, in a lengthy and laborious way, the verdict reached in the 19th century that *Der Einzige und sein Eigenthum*, despite its audacity and historical importance, is a curious piece of eccentricity, a polemical *tour de force* that leads nowhere.

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Political and Legal Obligation, Nomos XII, Yearbook of the American Society for Political and Legal Philosophy. Edited by J. Roland Pennock and John W. Chapman. (New York: Atherton, 1970. Pp. 455. $7.95.)

This volume brings together eighteen assorted essayists from political science, philosophy, law, religion and the English Parliament to address a threefold question: the nature and existence of any "obligation" to obey the rules and commands laid down by the agencies of the State, the empirical conditions that promote and discourage such obedience and, lastly, the nature and justification of civil disobedience. To the extent that the contributors exhibit any general agreement, it would seem to consist chiefly in the rejection of any claim that an individual, insofar as he is a moral agent or, indeed, insofar as he is a citizen of a democratic society, has an unremitting duty to do as the state tells him. The agreement spawns the school of problems that have come to plague us, and the essential heterogeneity of the book. What sort of duty, what sort of obligation, does a man have, if he has any at all? What relations obtain among political, legal, and moral obligation? What properly limits and occasions obedience or disobedience? Which arguments, which kind of logic, support and distinguish the various kinds of obedience and disobedience? What does obedience or disobedience to law mean, given the various senses of "law"? The book will be of minimal use for those who seek a mapping out and ordering of the domain, but the reader who seeks less will find some interesting and curious things.

Part I, entitled "Legal, Moral and Political Obligation" begins with a challenge to traditional political theory by Professor John Ladd, whose endeavor is to prove that (p. 4) "in the ordinary and strict sense . . . the concept of political obligation or an obligation to obey the law is not only inapposite but morally objectionable as well." "If," he argues (pp. 34–35) "we assume that the basis of political obedience is an obligation, then the onus is on an individual who is inclined to disobey a particular legal or judicial prescription to defend his doing so. . . . The Establishment, on the other hand, does not ab initio have to justify its claims . . . the burden of proof devolves upon the challenger to show why it should not be obeyed. . . ." Ladd thinks this is morally objectionable; and so it is, provided one substitutes "Establishment" for "Authority," as Ladd does, and judges the sense of the statement unchanged.

After Ladd's argument receives some rather searching criticism, particularly from Professor Jeffrie Murphy, there follows perhaps the most ambitious effort of the volume. That is Professor Alan Gewirth's attempt to grasp in a carefully articulated logical structure and thirty-three pages multiple aspects of the problem of obligations perceived in relation to one fundamental principle of rationality, the "Principle of Categorial Consistency." According to the Principle, it constitutes a contradiction for a person to say that what is right for him—to "participate voluntarily and purposively" in a "joint transaction" with another—is not "right for a relevantly similar person" (p. 68). It is not necessarily to approve that principle or its uses to deplore that Gewirth's essay, unlike Ladd's, finds neither critics nor supporters. It might have helped provide a somewhat better focus for the volume's proceedings.

Professor Richard Flatham's immediately following article might well also have received some discussion, although for different reasons. Flatham is concerned to establish (p. 115) that ". . . critical awareness, reflection, and evaluation are not less important in the practice of political obligation in a stable society than in any other aspect of politics in such a society." The "practice of political obligation," meant to substitute for a rule of obligation, is a rather intriguing idea, if one could get a better hold on it. The question, however, remains of saying what rules formulate the practice, if it is indeed a practice, and when "critical awareness, reflection, and evaluation" have actually taken place and been successful. Nevertheless, the possibility of a pragmatic analysis has been raised.

Professor Kurt Baier, on the other hand, seeks not to transform the problem but to "respond directly to the more traditional issue," (p. 117). Taking "obligation" broadly as "the logical correlative to the morally ought," he argues that the existence of political obligation depends upon whether there is "a justification . . . for the existence of governments and the coercive apparatus which is an essential feature of . . . political authorities," (p. 139). Plainly, there often is such a justification. If there is, merely residing in a territory will generate at least a prima facie obligation to obey the rules and commands of the State that exercises control over that territory. Unlike Ladd, Baier is committed to the view that ". . . the burden of proof that there have occurred abuses sufficient to reverse the prima facie wrongness of disobeying the government must lie on the person who makes such a claim. . . ." (p. 139).

On the whole, the next section of the book, "Obligation and the Conditions of Obedience," is easier going and of lesser interest. With the exception of Professor David Rapoport's study of *fides* and *obsequium* as expressed in different institutions in ancient Rome, there is not much more to be found, except in detail, than one has already encountered. But Rapoport examines the general structure of the attitudes required by political communities, and the consequences of their alteration where those communities are taken, in effect, as systems. This reader, at any rate, found the results highly suggestive.

"Civil Disobedience," the last section of the volume, is the most internally disparate of all, reaching all the way from attempts to find a definition for "civil disobedience" to Professor Meyer's "Political Change through Disobedience in the USSR and Eastern Europe" and Professors Wayne Leys and Rama Rao's "Gandhi's Synthesis of Indian Spirituality and Western Politics." Professor James Luther Adams opens the section with an attempt to show how "the tests for authentic civil disobedience are similar to those employed in the doctrine of the just war" (p. 303), and proposes what one takes to be a set of necessary and sufficient conditions for "authentic" civil disobedience. But, like most writers on the question of "civil disobedience," Luther Adams does not clearly tell us the point of calling a more or less fully described act of disobedience "civil disobedience," or denying it that title. Why does one need necessary and sufficient conditions? What is the point of the argument, besides classification?

It is the merit of Professor Kent Greenawalt's analysis that he in effect undercuts such questions by focusing instead "on the arguments one would put for and against a particular disobedient act before it is done, and the criteria for deciding if it was correctly undertaken" (p. 335). Even though the concept of obedience is itself, as Professor Greenawalt intimates, far from transparent, especially in a complex legal order such as our own, the tactic produces an extremely flexible article, undistracted by definitional necessities. Let "civil disobedience" be what it may, he is free to describe and evaluate cases in their own terms.

It is interesting to contrast Greenawalt's article with Professor McCallum's "Some Truths and Untruths about Civil Disobedience," which is concerned to show how "over-simple and artificially constrictive accounts . . . bedevil our thinking about . . . civil disobedience" (p. 371) and how "various persons purportedly engaged in dialogue about civil disobedience may not be talking about the same thing at all" (p. 377). Undoubtedly, McCallum achieves his end. Yet insofar as he does, his essay only tends to underscore the value of Greenawalt's approach. So, for that matter, does Professor Walzer's ingenious article, which argues in relation to military service that some young men who are native-born citizens have a legitimate claim to be treated as aliens, since they have never undertaken the obligations of citizenship (p. 413). Whatever the merits of his argument, the great debate about "civil disobedience" is happily left behind.

From the perspective of the present, perhaps the most striking thing about the volume as a whole is the overwhelming concentration on the moral plight of the citizen before the law. The obligations of officials to the law, the problem of the nature and justification of their principled disobedience, do not begin to come to focus. Of course, when these essays were written, sometime before 1970, Watergate was not even a suspicion. Perhaps now we can hope that Messrs. Chapman and Pennock will increase still further our debt to them with a Nomos volume on the issues in justification that the problem of official obligation raises, and most particularly on the connections of those issues with the problems of citizens in defining their obligation. The whole discussion of legal and political obligation is due for a radical expansion.

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Wittgenstein and Justice; On the Significance of Ludwig Wittgenstein for Social and Political Thought. By Hanna Fenichel Pitkin. (Berkeley and Los Angeles: University of California Press, 1972. Pp. 375. $12.50.)

This generous, careful and clearly argued book tackles a paradoxical task, and comes near to being a resounding success. Professor Pitkin concerns herself not with the late Ludwig Wittgenstein's classic, aphoristic *Tractatus Logico-Philosophicus,* written at the end of the First World War, but with the methods of conceptual analysis that he developed in the 1930s and 1940s, on taking up philosophy again after a ten-year break and moving to Cambridge University. Although those methods have recently become influential in psychology, aesthetics, jurisprudence, and even theology, as well as in academic philosophy, up to now they have had much less influence on political science. The reasons are clear. Wittgenstein himself left scarcely any recorded views about politics, still less about political theory: no major philosopher since Plato, indeed, said less about social and politcal issues. Yet it should not be concluded (Professor Pitkin argues) that Wittgenstein's later philosophy has *no* implications for political science. At the very least, his analytical methods could, in her view, add as notably to the discrimination of political thought as they have done to epistemology and the philosophy of mind. Eventually, they might even lead to the development of a characteristically "Wittgensteinian" political theory. How, what, and why this might be, are the subjects of her book.

The first seven chapters are an admirable introduction to contemporary analytical philosophy, and a remarkable feat of condensation and synthesis. They set out an entire corpus of work in all its essentials; including the ideas of Stanley Cavell and Gilbert Ryle, Paul Ziff and J. L. Austin, Friedrich Waismmann and Peter Winch, as well as Wittgenstein himself. The main themes of this corpus—the theory of meaning, the roles of language as performance, the nature of action, the roots of philosophical fallacies in a misunderstanding of grammar, the relations of philosophy to a more general account of the "natural history of man"—are all covered with extraordinary sympathy, accuracy and conciseness. Most importantly, Hanna Pitkin pays close attention to Wittgenstein's central notion of "forms of life" (*Lebensformen*), those constellations of human behavior that define the contexts within which any mode of discourse acquires a meaning. As she recognizes, this basic notion gives rise to some unanswered questions: e.g., "Are the *Lebensformen* to be thought of as universal (perhaps species-specific) features of human conduct? Or are they rather—as the Whorf-Sapir hypothesis would suggest—historical and cultural variables, reflecting radical differences between the modes of human life in different milieux?"

The second part of the book comprises six linked but largely independent essays, applying Wittgenstein's ideas and methods to problems in political theory. The range of topics is large: from Plato, Machiavelli, and Marx to Wolin, Dahl, and Arendt, from "justice" and the "political," by way of "membership," "action," and "legitimacy," to the nature and function of political theory itself. The most striking and effective sections are those that emphasize the practical, "performative" roles of political concepts, or that bring to bear recent insights into (e.g.) the differences between "actions" and "bodily movements," and between "reasons" for action and "causes" of behavior. After reading these essays, one must surely concede Hanna Pitkin's initial claim: that we can do much to clarify our ideas about politics, avoid needless cross-purposes, and deepen our understanding of the roots of political thought in human life, by employing methods of analysis and argument familiar among contemporary analytic philosophers.

This is not to say that everything in Professor Pitkin's argument is entirely plain sailing. She writes perceptively about the features that distinguish political *philosophy*—which should deal in straight analysis, and "leave everything as it is"—from political *theory*, which advocates either a change in our current conceptions, or the adoption of brand new concepts. Yet at times her own philosophical analyses seem to advocate theoretical positions that close off alternative options, without offering any very clear or satisfactory reasons for doing so. In declaring, for instance, that "Socrates is entirely right [*contra* Thrasymachus] about the whole meaning of 'justice'" (187), and apparently identifying "the conception of the political" current in Socratic Athens with "*our own* concept of the political" (pp. 213–14, my italics), she begs some crucial questions about the uniqueness of this "conception" or "concept." But what reason is there for thinking that one-and-only-one conception of "justice" or "the political" has overriding authority in all historical and cultural milieux whatever? It would be equally Wittgensteinian—probably more so—to suppose that such political conceptions display an irreducible variety; that they are linked only by "family resemblance"; and that they do not approximate to any single essentialist ideal, to be called "*the* concept" of justice, the political, or whatever.

These reservations apply with greatest force to Professor Pitkin's final chapter. Here she diagnoses our "modern predicament" in the manner of Hannah Arendt, and clearly hopes to achieve some theoretical convergence between Arendt and Wittgenstein. The results are not convincing. For example: she cites the contemporary spread of cynicism, lying, and manipulation on the part of politicians and governments—though are our current rulers *all* that more devious than their historical forerunners?—as evidence of "dehumanization" and of a new tendency to perceive people "as objects"; and she blames the rise of science and technology for these habits of mind. (The resulting corruption of political and social discourse calls, in her view, for a return to philosophical rationality based on the "truth-revealing function" of language.) Her account of this "dehumanizing world-view of science and technology," however, is curiously *un*-Wittgensteinian. It is based on a misdescription of scientific rationality as being rigorously "objective" and standardized; it reads a (Skinnerian?) preoccupation with behavioral causality into current assumptions about political agency; and it invokes "the total doubt and sense of unreality" supposedly generated by modern science—something Wittgenstein himself would have regarded as a philosophical *misunderstanding* of science—as the central and unique consequence of science for our contemporary view of ourselves.

The resulting argument is incoherent at several points. For the *misuse* of language, quite as much as its *use,* can be effective only with potentially rational concept users. Politicians who *really* perceived the citizenry "as objects" would not even bother to lie to them: they would simply push-and-pull them. What the defense of political language accordingly demands is not the reinstatement of *reasons* as against *causes,* but rather a clearer understanding of the differences between *good and bad reasons* within the sphere of political arguments.

How that distinction is to be drawn is, of course, another story, and one on which Hanna Pitkin's own methods might throw interesting light. For the moment, Wittgenstein's own silence about politics is something we cannot afford to ignore. In his early years, it was associated with an extreme moral individualism, similar to that of Kierkegaard, and in his later years with a view of philosophy that indeed "leaves everything as it is" and which certainly lends no more support to Arendt's psychopolitical diagnoses than to anyone else's.

Still, this final chapter of Professor Pitkin's book is the smallest part of it; and its weaknesses do nothing to take away from the virtues of the previous thirteen. All in all, the book deserves a very wide circulation, and could serve as the prototype for a whole new *genre* of analysis in political theory.

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Measures of Political Attitudes. Edited by John P. Robinson, Jerrold G. Rusk, and Kendra B. Head. (Ann Arbor: Institute for Social Research, 1968. Pp. viii, 702. $12.00, cloth; $6.00, paper.)

Measures of Political Attitudes is an amply annotated compilation of political and social attitude scales. Searching through social science journals, books, dissertations, and manuscripts as far back as the 1930s, the authors found 95 scales that they felt were of sufficient quality to include in this handbook. The book presents a complete listing of the items that comprise each scale, along with documentation on reliability tests, validation, type of samples used, and reference sources.

The authors have not merely listed the scales; they have also included brief evaluative commentaries as well. These commentaries alert the reader to the important methodological limitations of each instrument.

This handbook is indeed a worthy effort, and the fact that it is now in its 6th printing testifies that its usefulness has been widely recognized. If you have ever been frustrated by an author who presented only a tantalizing sample of the items in his scale but failed to publish the complete set of items; if you have ever spent days combing the literature to see if someone has devised an instrument that will measure a construct that you are interested in; or if you have ever tried to devise a scale of your own, the worth of this handbook will be self-evident.

A companion volume, similar in format, entitled *Measures of Social Psychological Attitudes* has also been published by the ISR (John P. Robinson and Phillip R. Shaver, editors.) This volume contains a number of scales that would be of interest to political scientists including measures of alienation, authoritarianism, dogmatism, trust in people, etc. It, too, makes a valuable addition to one's reference library.

As helpful as these compendia of old instruments may be, they should not detract from the need for creative new instruments. The Guttman-type scale is surely not the only method of measuring attitudes, and as the authors point out in their evaluative commentaries, these scales are full of methodological pitfalls. As we go into the 1970s, let us not be bound so much by the instruments of the '30s and '40s. New and better methods of measuring political attitudes can surely be devised.

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Radical Political Economy: Capitalism and Socialism from a Marxist-Humanist Perspective. By Howard Sherman. (New York: Basic Books, 1972. Pp. 431. $12.50.)

In recent years many social scientists have become increasingly dissatisfied with what they consider to be the "sterility" and "apologetic nature" of their respective disciplines. Dissatisfied economists are beginning to pay more attention to social and institutional factors which affect the functioning of the economy, and political scientists to the economic and social setting of political institutions. Many of these scholars make it quite clear that their aim is to help bring about radical changes in the structure of existing society. The approach to economic and political problems which focuses on the interrelationship of economic, social, and political factors has been termed "political economy."

Howard Sherman, Professor of Economics at the Universty of California, Riverside, has attempted in this very ambitious book to show that radical political economy is a scientific paradigm, that it does provide "a systematic and complete framework of analysis" of present-day capitalist and socialist societies (p. 4). He acknowledges that there is little in his book that has not been discussed elsewhere in great detail. What is new is the attempt to summarize many of the recent studies by radical social scientists and to synthesize this material with the work of older radical social scientists, such as Sweezy, Lange, and C. W. Mills, so as to provide a unified framework of analysis.

Professor Sherman describes his perspective as Marxist, though not the dogmatic variety which has characterized Soviet scholarship, especially during the Stalin years. It is a Marxism which takes into account changing political, social, and economic realities and "which tries to incorporate the best tools and findings of non-Marxist social science" (p. vii). But nowhere does Sherman adequately explain what he means by a Marxist approach and how it differs from what he calls the "liberal" approach. The first chapter on method is much too brief and simplistic. Yet the comparisons made throughout the book between a "radical-Marxist" analysis of a particular problem, such as pollution, and the differing "liberal" analysis do provide the reader with some implicit understanding of what he means by a Marxist approach.

The book, divided into three main parts, describes the political economy of capitalism (represented by the United States), the political economy of socialism (represented mainly by the Soviet Union), and the political economy of communism (the humanist society of the future). Each of these parts is divided into sections dealing with particular aspects of political economy, such as poverty and exploitation, growth and waste, the role of the state, imperialism, racism, and sexism. This framework permits Sherman to draw comparisons between the different societies. For example, he tries to show how the problem of economic waste in the capitalist United States is in large part a function of the power of the monopolists to "persuade" people to buy unnecessary products, products which often are made to fall apart after a few years. In the Soviet Union, on the other hand, waste is principally a function of the inefficient operation of the gigantic, highly centralized state bureaucracy. Thus the nature of economic waste in the two societies is determined in large part by the economic structure of the respective societies. As another example, alienation, the feeling of being estranged from one's work and from other people, is in capitalist society largely due to capitalist control of the economy and to the competition and isolation which capitalism forces on people. In the socialist Soviet Union alienation is largely due to the fact that the people themselves have little control over the operation of the huge state bureaucracy.

Sherman concludes from his analysis that for a humane society to exist—one without poverty, waste, alienation, etc.—capitalism must be abolished. For example, after his discussion of how sexism contributes to the functioning of capitalism (e.g., by providing for a source of extra profit) he concludes that sexism probably cannot be abolished as long as capitalism remains. (Sherman acknowledges that sexism continues to exist in the Soviet Union, though, he says, to a lesser degree than in most capitalist societies.) But at the same time he does not favor socialism of the Soviet variety. Soviet history, he says, has demonstrated the need for political democracy and constitutional safeguards. There must also be some degree of economic decentralization, though not to the extent which exists in Yugoslavia.

To a political scientist, Professor Sherman appears to be at his best when discussing economic issues. His comparison, for example, between Marx's approach and that of the neoclassical theorists is illuminating. He convincingly shows that much neoclassical theory is compatible with the approach and theories of Marx. The appendices to the book contain useful summary discussions of some of the more technical aspects of Marx's economics. Sherman also provides ample references for further study of Marxist economic literature. My major criticism is that much of his analysis is too cursory. Some key questions, such as the difficulty in defining economic exploitation, are only noted, not adequately discussed.

Sherman's treatment of social and political issues is less satisfactory. In the chapter on the capitalist state and democracy he summarizes the methods by which dominant economic interests are able to control the state. His tendency to explain state policy in terms of the manipulation of political institutions by strategically placed individuals is not what one normally considers to be Marxist. It is more like the power-elite approach of C. W. Mills and

W. Domhoff. Sherman does not seem to recognize the difference. Throughout the book he tends to equate the terms "Marxist" and "radical." And at one point he refers to C. W. Mills as a "non-dogmatic Marxist" (p. 5)!

The discussion of other topics, such as imperialism, racism, and class structure, is often too sketchy. Particularly inadequate are the sections dealing with the Soviet Union. The wide scope of the book may in part excuse this. Yet it is unfortunate that certain basic theoretical questions are not explored. For example, how should traditional Marxist class analysis be changed to take into account changes in the class structure of advanced capitalist society? What does the continued existence of economic exploitation in the socialist Soviet Union imply about the adequacy of the Marxist approach to exploitation in capitalist societies?

In conclusion, Professor Sherman's book is unsuccessful in providing "a systematic and complete framework" for analyzing capitalist and socialist societies. More basic theoretical work needs to be done. Sherman's book does demonstrate that the theoretical perspective of political economy yields a number of insights and is worthy of further development. The book itself contains summaries of what many of today's radical social scientists are saying and therefore may be useful as a teaching aid.

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Institutions and Social Exchange: The Sociologies of Talcott Parsons and C. Homans. Edited by Herman Turk and Richard L. Simpson. (Indianapolis and New York: Bobbs-Merrill, 1971. Pp. 417. $8.50.)

The majority of the essays making up this volume are reprinted from two special issues of *Sociological Inquiry*: the Winter, 1959 issue dealing with Parsons (the journal was then called *Alpha Kappa Deltan*; two of the three essays in question have been extensively revised) and the Spring, 1964 issue dealing with Homans. To these twelve essays (including Homans's "Commentary" on the latter issue, reprinted here with revisions) the editors have added: a short introductory statement of their own; a well-known pragramatic piece by Homans, "Bringing Men Back In"; seven new essays, mostly dealing with Parsons; finally, by Parsons himself, a previously unpublished essay on higher education, and a "Commentary" which, with the piece by Homans bearing the same title, closes the volume. Among the reprints from the 1964 issue there is also a critical piece on Homans by Parsons himself. There is no corresponding piece by Homans specifically dealing with Parsons; but of course many writings of the former, including the two reprinted here, develop a more or less explicit anti-Parsonian argument. In fact, for decades now, probably since the 'thirties when they both sat at the feet of Lawrence J. Henderson, who was the Pareto prophet at Harvard, Parsons and Homans have been involved in a sustained (though, so far as I know, urbanely carried out) sibling rivalry. Only since the early 'sixties, however, with the publication of Homans's *Social Behavior: Its Elementary Forms,* the feud has spilled over its original turf, i.e., Harvard, and led to a (minor) conflict of schools within American sociology at large.

As usual with volumes of this type, the content is rather diverse, but it can be grossly broken down as follows: Some pieces are primarily expositions and elaborations of this or that aspect of the work of one of the principals: these are the essays by Martel, Boskoff, and Jacobson, all of whom deal exclusively with Parsons. Others are mainly critical of one or the other of the two: Morton Deutsch and Parsons criticize Homans; Sjoberg & Cain and Lopreato criticize Parsons. The essays in a final set, after a more or less perfunctory nod in the direction of Parsons or Homans, or both, develop themes of their own: there are essays by Blau, Josephine Klein, James C. Coleman, Shibutani, and Alexander and Simpson, which nod to Homans; by Simpson, which nods to Parsons; and by Turk, which nods to both. Alone, defying classification, stands Cloyd and Bates's spectacularly inane quantitative study of references to Homans in footnotes to articles published in six leading American sociological journals between 1951 and 1962.

I should like to point out to interested readers those few essays in this collection which seem to warrant attention on the part of social scientists not particularly interested in strictly intramural sociological arguments. Above, I have referred to Martel's essay, "Academentia Praecox: The Aims, Merits and Scope of Parsons' Multisystemic Language Rebellion (1958–1968)" as being primarily expository. It is that, but it is on all counts a first-rate job, well-organized and full of original and penetrating insights. I was particularly struck by its forceful acknowledgment of what has always impressed me as one great virtue of Parsons's work: the fact that Parsons has *so many things* to talk about, that he never contents himself with elaborating a few points, but always moves on to new ones, thus displaying an unequalled awareness of the sheer severalness of social reality. One could wish, of course, that Parsons did not feel obliged to announce with a flourish of trumpets *each* new point he moves on to (the adjective "crucial" occurs with annoyingly high frequency in his writings), but it simply cannot be said of him that he goes on and on about not very much: whatever the merits of what is said, it is being said *about* a great number of diverse things. While Martel points this up, and duly acknowledges the strain this imposes on language in Parsons's writings, he does not point out instead that in seeking to organize this multitude of points, Parsons all-too-often yields to what one might call his tetradic compulsion; that is, he simply assumes that all noteworthy things occur in sets of four —no more, no fewer. The basic tetrad is, of course, the AGIL scheme of system exigencies (adaptation—goal attainment—integration—latency); but Parsons seems to have forgotten that he originally derived that scheme through the simple (and analytically legitimate) device of "crossing" two dichotomized variables. Currently, he does not bother even to substruct his innumerable tetrads, and to display them for what they are, i.e., "property spaces," analytical constructs—and seems to think of them as sheer properties of nature.

Lopreato's essay on the equilibrium concept is, I feel, the second best piece in the collection. It is certainly among the best discussions of the concept in the social science literature, not just a critique of its use by Parsons: I recommend it to all those who are puzzled about the significance of the notion of equilibrium and its relation to the more widely used notions of system and model. Finally, I would particularly recommend to readers of this *Review,* because of its bearing on what I presume to be their more widely shared concerns, Coleman's essay on collective decisions. I also find worthy of mention Shibutani's graceful essay on sentiments and social control and Simpson's on the structural significance of associations (professional ones in particular) in contemporary society.

The rest of the papers are, I am afraid, of rather indifferent quality. There is, unfortunately, a further reason that this volume makes on the whole depressing reading, and this is its astonishing parochialism. To begin with, at most, only some seven or eight contemporary non-American authors get even passing mention (although each essay of course has the usual complement of learned references). Thus, for instance, the reader fails to learn from this book that in the 'sixties significant new departures in sociological functionalism were made by an impressively high-powered German author, Niklas Luhmann, who learned a great deal from Parsons and has a great deal to say on him; or that a somewhat similar relationship exists between some writings (also available in German) of the Polish sociologist Andrzej Malewski and those of Homans; or, finally, that a younger member of the Frankfurt school produced a few years back one of the two books on Parsons known to myself.

Furthermore, even the fact that *within the American sociological scene itself* the relative credit and significance of both Parsons and Homans sharply decreased during the 'sixties with the comeback of symbolic interactionism, the rise of phenomenological sociology and ethnomethodology, the popularity of Goffman, the new wave of "social problem"-oriented empirical research, etc.—even this fact finds practically no echo in this book. Judging from it, a reader could easily assume that in 1971 Parsons and Homans still stood alone on the scene.

Finally, even as a contribution to the Parsons-and-Homans debate this collection strikes me as definitely stale. As Parsons rightly complains in his "Commentary," few of the authors who criticize him show much awareness of his work in the 'sixties. In fact, everybody, critics and supporters, goes on rehearsing the old diatribes of the 'fifties: Can Parsons account for conflict and change?; Does he have a Polly-Anna view of society? And so on and so forth. A Rip van Winkle gone to sleep on the last page of *The Social Theories of Talcott Parsons* (edited by Max Black in 1961, and a much better book than this on all counts) and awakening in the middle of this volume, would have little occasion to realize that he had been asleep for ten years, during which in fact a great deal has happened both in American society and in American sociology. I had long known the truth of Riesman's complaint about sociologists not being enough interested in society; but I had not suspected that some of them pay distressingly little attention to what goes on in sociology itself.

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The Constitutionalist: Notes on the First Amendment. By George Anastaplo. (Dallas: Southern Methodist University Press, 1971. Pp. 826. $20.00.)

In 1950, when anti-Communist hysteria and fear of subversive activity were at a high point, George Anastaplo sacrificed a legal career in order to resist inquiry into his political associations in the course of the review of his application for admission to the Illinois Bar. Although it was widely understood, even at the time, that he had never been a Communist or a member of any suspect organization, he declined to follow the line of least resistance as did many others of less stern stuff, and chose instead resolutely to refuse to answer all questions relating to his beliefs or associations. For his insistence on principle, he was denied admission to the Bar—a decision that was never overturned despite eleven years of litigation, including two appeals to the United States Supreme Court—and he turned instead to the study of political science. His doctoral dissertation, *Notes on the First Amendment,* has now been published, albeit with a new primary title, with the addition of discursive notes that expand the book to three times the length of the basic text, and with a number of appendices, including one outlining the essence of his celebrated case.

In this book, Professor Anastaplo once again demonstrates that, in his mind, principles must be given priority over consequences. He professes to adopt an "absolutist" view of the First Amendment's guarantees that relate to freedom of expression, but his "absolutism" is not that of Justice Black or of Alexander Meiklejohn. Instead, although Professor Anastaplo excoriates Justice Holmes and his successors on the Supreme Court for subverting the meaning of the First Amendment by adopting and applying the "clear and present danger" and "balancing" tests, the "absolutism" he professes is a doctrine that would provide no more protection for freedom of expression than is afforded today under the prevailing tests, and would erode or discard completely constitutional protection in many areas where it has been definitively established.

The Constitution, he points out, expresses its absolutism in language that begins "*Congress* shall make no law," and he notes that this does not forbid executive or judicial abridgments of the freedom of speech, nor, more importantly, does it forbid state prohibitions on speech, a gap which he does not consider to have been closed by the Fourteenth Amendment. Thus, the President, as Commander-in-Chief, can impose restrictions on speech that may be necessary to maintain discipline in the armed forces; the courts can insure fair trials by punishing as contempt any expression that might threaten to interfere with justice; and the states, under their police powers, are free to restrict speech as much as they deem appropriate, except that Congress, exercising the power to guarantee to every state a republican form of government, may prohibit such state restrictions as interfere with the effective implementation of representative democracy.

Moreover, Professor Anastaplo concludes that since the overriding concern of the framers of the First Amendment was with freedom of political or religious expression, only those areas of expression can be said to fall within the scope of the Amendment's guarantee. No matter what its incidental effects, Anastaplo maintains, artistic or literary expression is distinct from political expression and is thus constitutionally unprotected. Perhaps more startlingly, Professor Anastaplo apparently would not extend constitutional protection to radio or television, even when those media are used for political discussion. He bases this arresting distinction largely on the fact that the electronic media are potentially so effective as purveyors of ideas as to be dangerous, and on an unexpressed but ill-concealed disdain for the mass audience who might thus be manipulated.

Recognizing that the consequences flowing from his proposed construction of the First Amendment would appear unpalatable to many, Professor Anastaplo admonishes us that it is the proper duty of "the Constitutionalist" to heed the language of the document and its underlying principles regardless of the immediate results. There is merit in this contention, but there are also dangers in ignoring consequences and heeding only language, particularly where the intent of the framers is far from clear. And the principles that Professor Anastaplo extracts from his examination of the legislative history of the First Amendment are indeed far from clear. For example, the available evidence regarding the adoption of the language, "Congress shall make no law," indicates that it was employed to avoid any inference that the Establishment Clause was intended to affect state religious establishments, and that the term was extended to the speech and press guarantee solely for reasons of style. There is not a shred of evidence that it was intended to permit federal abridgments of expression imposed by the President or by the courts, any more than that it was intended to allow a federal establishment of religion if effected by executive or judicial authority.

Professor Anastaplo would grant the states freedom from constitutional restrictions in the area of expression, and would rely exclusively on them to prohibit abuses attending the freedom of speech. Besides the fact, however, that this formula might permit the frustration of federal policy in states willing to tolerate incitements to resistance against enforcement of that policy, anyone familiar with the history of the adoption and enforcement of sedition, anarchy, and anti-Communist legislation by the states in this century would probably not be willing to substitute for the existing federal constitutional barrier the possibility that Congress might legislate against such laws—as Professor Anastaplo suggests—through the power to guarantee a republican form of government to the states.

The understanding of the Supreme Court regarding the meaning of the First Amendment and the necessity for judicial enforcement of its provisions has grown considerably since the abuses of the "clear and present danger" test perpetrated in *Schenck* and *Dennis*. (The test Holmes advocated in his *Abrams* and *Gitlow* dissents was entirely different from the test he announced in *Schenck*.) If all Professor Anastaplo seeks by his call for "absolutism" is a constitutional rule that no person can be held liable to a congressionally imposed penalty merely for expressing his political or religious beliefs, no matter how heretical, outrageous, or potentially dangerous those beliefs may appear to be, that position is now accepted by every member of the Supreme Court, even in the post-Warren era. Of course, frightened judges may once again find a clear and present danger where none exists. But the same frightened judges could also distort the Anastaplo test by manipulating the concept of political speech. If, as Professor Anastaplo would have it, speech broadcast over television loses its political character, why could it not also be held that it loses its political character when uttered in a situation of grave danger?

Professor Anastaplo states that he knows of no one who agrees with his position on the First Amendment. It is unlikely that he will have to change that assessment as the result of this book.

DEAN ALFANGE, JR.
University of Massachusetts

The New Deal in the Suburbs: A History of the Greenbelt Towns. By Joseph L. Arnold. (Columbus: Ohio State University Press, 1971. Pp. xiii, 272. $10.00.)

At long last a thorough, competent account of one of the most interesting experiments in the nation's history—the federal government's building from scratch of three suburban communities—is available, thanks to Joseph L. Arnold, a historian at the University of Maryland. Although his sympathy is obviously with the experiment, his work is carefully documented, analytical, and far from uncritical.

The book has value for a very wide range of readers including architects, planners, public administrators and managers, utopians, environmentalists, enthusiasts for the cooperative movement, economists, sociologists, and political scientists. The treatment is largely chronological and narrative, replete with large numbers of names, dates, and bibliographic citations. But Arnold manages to provide some conceptual framework and linkage to the practical problems encountered in trying to realize essentially new, far-reaching goals. He interrelates and coordinates the mass of data and objectives so as to focus on numerous implications. He traces the construction and history of the three towns—Greenhills, Ohio; Greendale, Wisconsin; and Greenbelt, Maryland—devoting the fullest treatment to the Maryland experiment.

The subjects of greatest interest to political scientists are probably the pluralist politics involved, tenant selection and regulation, and participatory democracy. The author notes that "favorable reaction to the greenbelt towns was the crucial goal of the entire program. Unless public pressure could be generated, Congress would not expand the program and private builders would continue to construct disjointed little housing developments. . . . It is obvious that the majority of those who wrote and spoke about the towns reacted negatively" (p. 191). Further, it was important for supporters of the programs to make known that the towns were complete communities in themselves, as distinguished from ordinary housing developments. Most of the public never understood this distinction nor accurately perceived the goals and significance of the projects.

From the outset, opponents far outnumbered supporters and kept up a steady drumfire until the towns were sold in 1954. The projects were condemned as unnecessary government competition with private enterprise, that they were unneeded, impractical, and expensive, and were socialistic regimentation disguised as cooperative planning. By raising the specter of suburban areas containing poor as well as affluent families, the program threatened one of the most fundamental, traditional attractions of suburban areas. Despite the great enthusiasm and support given to the greenbelt experiments by Franklin and Eleanor Roosevelt and Rexford

Tugwell (the towns were dubbed "Tugwell towns"), not many allies were found, particularly where they were needed—in Congress.

The most interesting aspect of the greenbelt idea was the building of a face-to-face democracy "to direct socio-economic cooperation" and to provide the normal services of an independent municipality. As the only political scientist living in the town of Greenbelt I moved there (having met the low income guideline!) a few months after it opened and remained until 1942. I can testify that Arnold's account of "the frenetic activity of the townspeople" is not exaggerated. Large numbers of residents attended meetings every night. Considering the size of the town, adult educational opportunity was probably without parallel. But people were overstimulated, and they organized more activities than they had time or money for.

The cooperative democracy atrophied after the first years. Cooperative business requires continuous citizen support. Most newcomers found civic activity a new and exhilarating experience. But, as Arnold notes, "what was a joy to build became a dull burden to administrate" (p. 185). The war years brought heavy turnover of families, and newcomers could not be quickly enough assimilated into the demanding community institutions. The greenbelt experiments underline again the well-recognized problems of sustaining a high level of mass participation over many years.

From the political scientist's point of view the greatest gap in the author's story is that he insufficiently analyzes the origin and development of factionalism and the internal politics of the towns. When a community is suddenly born how do factions arise? From persons who possess a certain charisma? From ideology? From partisanship? One wishes that one of the original residents would try to piece together this story to augment our theory and practice of internecine politics in a government-owned town.

HUGH A. BONE

University of Washington

An American Philosophy of Social Security: Evaluation and Issues. By J. Douglas Brown. (Princeton, N.J.: Princeton University Press, 1972. Pp. 244. $8.50.)

Social Security: Universal or Selective? By Wilbur J. Cohen and Milton Friedman. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1972. Pp. 114. $5.75.)

Launching Social Security: A Capture-and-Record Account, 1935–1937. By Charles McKinley and Robert W. Frase. (Madison: University of Wisconsin Press, 1970. Pp. 519. $12.95.)

There are few domestic issues more demanding of resolution than the reform of the welfare system and through it the alleviation of poverty. A recent congressional report (issued in March, 1973) confirms what many taxpayers have long suspected—there are many inequities and often conflicting provisions in the more than 100 public welfare programs costing over $100 billion annually and administered by 11 federal agencies. Although the three books which provide the basis for this brief essay purport to deal with the subject of social security, the coverage is broad enough to encompass many aspects of a total social welfare system.

They present a detailed administrative history of the social security organization in the first two years of its existence (Launching Social Security); a debate on the validity of social security in its present form (Social Security: Universal or Selective); and an analysis of the roles of public assistance, contributory social insurance, and private mechanisms in preventing or alleviating poverty in America (An American Philosophy of Social Security). The three monographs have at least one thing in common other than the subject itself; most of the authors were closely involved with the emergence of the social security system in some official capacity.

An American Philosophy of Social Security, in the author's own words, is "neither a technical analysis nor a detailed history" (p. v.). After a brief excursion into the genesis of social security in America, Professor Brown asserts that the introduction of a comprehensive public-support system was based more on amorphous concepts than on comprehensive statistical studies. In essence, universal security for the American people is posited as having three major characteristics: public assistance for which a needs test is required; a contributory old-age insurance program which would prevent poverty through the cooperation of the employee, the employer, and the government; and an open-ended layer of protection through private initiative and private mechanisms for those able to afford it.

Self-help and mutual aid societies, says Brown, accounted in large measure for the relief of poverty in the United States up to the advent of social security legislation in the 1930s. What had been a laissez-faire approach to poverty and its problems on the part of government came to an abrupt end as a result of the economic debacle beginning in late 1929. The winter of 1932–33 had been a desperate time for millions of Americans. In 1934, the American people greatly needed a constructive program for the prevention of poverty in old age. Professor Brown seeks in this series of essays to emphasize the broad sweep of evolution of the social security system and the major issues which have been resolved or are still to be decided.

Actuarial soundness of the emerging system was of considerable concern to many at the start and continues to provoke critical analysis from economists today. Although a small contingency reserve was agreed upon almost as soon as the benefit fund was created, the Congress has, in recent years, increased the payroll tax rate as benefits have been increased. In Chapter VII, Brown strongly advocates the sharing of costs of the total system on a three-way basis. With the introduction of Medicare and Hospital Insurance, more government support is a necessity.

An interesting, although brief account of the evolution and general structure of Medicare is provided. As the author notes, however, the program is still unfinished and internally fragmented. He characterizes health care for the American people as the expanding frontier of social security, but is concerned that the costs of a comprehensive system of health protection under social insurance undoubtedly will hamper such a system from developing rapidly.

An American Philosophy of Social Security clarifies many issues which concern those American citizens for whom social security is now basic protection against dependent old age and a host of other ills. There exists some unevenness in the essays (chapters), in that some events or issues are described more fully than others. However, the stated objective of the author was to provide general understanding of the philosophy and policy of social security. In this, he seems to have succeeded.

Social Security: Universal or Selective is based on a debate between Professor Wilbur Cohen, former Secretary of Health, Education, and Welfare, and Dr. Milton Friedman, a leader of the "Chicago School" of economics. It pits Dr. Cohen, one of the principal architects of a selective social security system against Professor Friedman, one of its most articulate critics and exponent of a more universal approach to a comprehensive program of assistance to the indigent. Friedman is concerned about the redistribution effect of "income from relatively low income classes to middle and upper income classes" (p. 48) through the payment of social security benefits and the fact that benefit payments are made out of current tax income. He insists also that the social security program is not an insurance program in which individual payments purchase equivalent actuarial benefits. Instead, Friedman considers the benefit structure as "protecting individuals from severe declines in living standards in retirement whether they want to be protected or not" (p. 37).

There is little question that given Prof. Friedman's economic postulates, the present system seems to be flawed. His claim that the United States "stumbled" into its adoption because of the special problems caused by the economic depression of the 1930s is probably correct. Concluding that the present system cannot be justified, Friedman recommends that our goal should be to eliminate the payroll tax completely, end the present complex structure of benefits, get the government out of the "pseudo-pension" business and combine the valid elements in the benefit structure with a single comprehensive program of assistance to the indigent (p. 41).

Professor Cohen, while acknowledging the regressiveness of social security payroll taxes, insists that actuarial soundness in funding is foolhardy and really unnecessary. According to him, it is doubtful that any group insurance program in the United States would qualify as insurance under such a test. He also does not deny that social security is primarily for the middle class but notes that a majority of Americans are now middle-class in income. The gist of the Cohen position is that in social insurance there are two basic elements; first, an equity concept and a social concept with emphasis on the latter. Nevertheless, the balance which has been achieved between these two elements has made social security in the United States an acceptable system.

The debate as recorded in the monograph is succinct and informative, although it is by no means an exhaustive treatment. Comments raised by members of a panel involved with the debate were hardly illuminating. Statistical information contained in the appendices dealt with a few specialized features. The data were not intended, however, for comprehensiveness of coverage but merely as illustrative of particular points. The debate monograph does highlight familiar positions taken by strong adherents of the social security program (Professor Wilbur J. Cohen) and that of its critics (Professor Milton Friedman).

In many respects *Launching Social Security* by Charles McKinley and Robert W. Frase is the most ambitious of the three works reviewed here. It is not as comprehensive nor as explanatory as Arthur Altmeyer's *The Formative Years of Social Security* (1966) or Edwin Witte's *The Development of the Social Security Act* (1962). The story told by the McKinley-Frase text is often a highly personal one, as seen from the eyes of officials who were a part of the administrative staff. The emphasis is on organization imperatives and on administrative necessities. An intensely personal account, it is unstructured and "scattershot" in nature.

Nevertheless, an informative and significant account emerges for the general reader and specialist alike. Highlighted are such issues as the impetus the Townsend pension movement of the 1930s gave to the enactment of social security legislation; and the importance of other factors immediately preceding the passage of the Social Security Act, such as a severe economic depression, bank "holidays," and similar calamities.

To a considerable extent *Launching Social Security* chronicles the growth pains of a unique experiment in providing for various benefit categories, some based on need (public assistance) and others requiring contributory participation. Massive efforts had to be made by the Social Security Board to develop an administrative organization both central, regional and local to achieve its goals.

The period under discussion in this text, even though examined in depth, is too short to justify definitive conclusions about most of the larger questions of organization and administrative policy. The authors, however, agree that the Social Security administrative structure was inadequate for the operation of its multifaceted program. Personnel practices during the period under study left much to be desired because of the need for hasty recruitment and an inordinate utilization of so-called "experts." The Federal Civil Service Commission was unable apparently to maintain effective and expeditious recruitment programs for Social Security agency personnel.

The reader can certainly agree with the authors, in their observation that "there were certain unmistakable achievements . . . Unemployment Compensation laws were passed by all states; public assistance plans were adopted by a majority of the states; and services were inaugurated for multitudes of disadvantaged" (p. 492). In short, a social security system was born.

It is difficult to envisage three more divergent attempts at dealing with the nature of social security and social security programs. A search for a philosophic concept characterizes one of them; a debate on the validity and efficiency of the Social Security Act and its programs concerns the authors of the second; a third develops a highly personal and detailed historical record of a scant two-year (albeit important) period. One cannot recommend all three to the reader as necessary parts of a whole, for the books range from the general to specialized concerns, and each has its appropriate audience.

LOUIS L. FRIEDLAND

Wayne State University

The Presidency on Trial: Robert Kennedy's 1968 Campaign and Afterwards. By Stuart Gerry Brown. (Honolulu: The University Press of Hawaii, 1972. Pp. 155. $6.95.)

If nothing else, its very title, paralleling as it does the title of James MacGregor Burns's well-known critique of Congress in 1949, seems to make this book exceptionally germane to the concerns of contemporary analysts of American politics. For a variety of reasons, far less of the energies of modern political analysis have been expended on the globally consequential institution of the American presidency than on an institution that often has seemed barely to be holding its own—the Congress. Thus, for example, from the start of the New Deal to the breakdown of Lyndon Johnson's Great Society consensus, the bulk of liberally oriented political scientists (which is to say the bulk of political scientists) have implicitly assumed that, as Rossiter put it, the presidency is a highly successful institution not to be tampered with or questioned. As recently as 1966, Grant McConnell's critique of the shortcomings of American political institutions and patterns (*Private Power and American Democracy*) arrived at the prescription: strengthen the presidency. Moreover, unlike the Congress, the presidency in recent years has not presented itself to scholars, and especially thesis-seeking graduate students, as a neatly divided terrain replete with handy subterritories that could be staked off by diverse investigators.

The present volume is by a professor of American studies (Hawaii), not a political scientist. Brown is described on the jacket as "a campaign associate of Adlai Stevenson," a member of the Civil Rights panel of the long-defunct Democratic Advisory Council (1957–60), and "a campaign consultant to Robert Kennedy's staff in 1968." His book, a rather surprising entry on the list of a university press, is a liberal activist's commentary and lament on the current state of American politics.

Both the intellectual tone and the content of Brown's rather repetitive, thinly documented (*New York Times* articles and occasional popular books), though obviously heartfelt volume can be illustrated by quoting typical sentences from the abstract that accompanies the table of contents of his six-chapter book:

> Presidential system on trial. Alternative not a democratic utopia but an undemocratic repressive state.
>
> President Nixon's avoidance of divisive issues puts 1968 campaign in perspective and explains [sic] the tragedy of RFK's death.
>
> Of the 1968 candidates, only RFK gave all his energies and risked his chances in dealing with [the issues of poverty, race, and crime]. RFK builds a new coalition of the poor, the black, and the young. . . Momentum lost after 1968.
>
> Unbridgeable gap between Nixon and the young.

The nearest thing to an analytic distinction in Brown's paean to the late Robert Kennedy is his assertion of a "vital distinction between presidential politics and the politics of presidential leadership," the former being the proclivity to blur issues, trim, and accommodate, and the latter the willingness to be divisive in order to achieve policies of sufficient rigor to deal with pressing social problems. Consequently, he sets up the old Democratic Advisory Council with its task forces and reports as an example of the sort of issue-stating institution that is needed, not noting that the DAC inevitably stood for only some elements in the Democratic party or that the Kennedy Administration had no interest in the perpetuation of such a competitive source of party leadership and was instrumental in encouraging the demise of the DAC.

Brown's book is a contribution to what promises to be a sizable liberal-left literature reassessing the presidency and presidential leadership. In this literature he occupies an interesting space in the sense that he seems to be interested not in tempering presidential power but rather in deflecting the President's efforts in policy directions Brown considers desirable. From the standpoint of the professional concerns of political science, *The Presidency on Trial* is emphatically not an original analytic contribution. But it *is* a datum illustrating the current ambience of critical thought and discussion about the presidency.

FRED I. GREENSTEIN

Princeton University

The United States and World War II: Military and Diplomatic Documents. Edited by A. Russell Buchanan. (Columbia, S.C.: University of South Carolina Press, 1972. Pp. ix, 303. $9.95.)

From Roosevelt's smug appeal to Hitler on the eve of the Munich Conference ("The Government of the United States has no political involvements in Europe, and will assume no obligations in the conduct of the present negotiations") to Attlee's cheery note to Truman just days before Hiroshima ("Thank you so much for your letter of today about the new weapon to be used on Japan"), Buchanan parades before the reader a varied collection of items, some of which fall only marginally into the category of "military and diplomatic documents." To his great credit, he has avoided both snippets and overediting and has included enough German and Japanese material to indicate "the nature of the enemy." The selections demonstrate, moreover, his familiarity both with the available documentation and with the events, his engagement and sophistication, and his unfailing interest in and eye for the surprising, the picturesque, and the human. The result is a rarity among collections of documents: a volume which is eminently readable and, for long stretches at least, even fascinating.

Unfortunately, however, the book also raises serious problems with regard to method and purpose. Buchanan describes it as a "sampling" designed to be "both illustrative and an inducement to further study" (p. ix). But illustrative of what, and an inducement for whom? Certainly, no balanced picture of the causes and conduct of the war emerges from these pages, in which as much space is devoted to the Pearl Harbor attack as to all of the wartime diplomacy, and more to the farewell letters of Japanese Kamikaze pilots than to the Normandy invasion. Surely 15 pages—and three separate accounts—of General Patton and the slapping incident is more than is needed simply for purposes of illustration, and Buchanan's use of the limited space available to him can be challenged on other counts as well. Do the largely self-serving dispatches of Hans Thomsen, for example, really contribute enough to an understanding of America's entry into the war or of Germany's position to warrant their inclusion? And what is illustrated when General Stillwell's account of a conversation with Roosevelt at his silliest is printed under the heading "Conference on Policy in China" (p. 98)?

Particularly the unknowing and the unwary can be seriously misled in this fashion, yet it is apparently for just such an audience that the book is intended. Serious students would surely want a collection based on more than merely the published sources. They would also expect a bibliography, preferably critical, as well as evaluative annotations, and, of course, an index.

None of these aids are provided.

Even the less serious student, whose appetite might well be whetted by this volume, will need more help than Buchanan offers him. The skillful, but excessively condensed section introductions which sketch the larger picture for the uninitiated are again heavily weighted in the direction of the editor's particular interests, and the absence of even identifying annotations is a frequent source of frustration. Surely the student might be told the name of the "acting Secretary of State" who became excited over the announcement of V-E Day (p. 189), the identity of the man who received the directive which included the phrase "You are designated as the Supreme Commander of the SOUTHWEST PACIFIC AREA . . ." (p. 212), or the whereabouts of "Funafuti" (p. 253).

There are technical problems as well. Buchanan's practice of stringing together separate portions of a single document without indicating omissions, while sometimes effective in making a point, not only distorts the thrust of the document, but frequently leads to confusion. Thus Stimson's account of the news of Pearl Harbor seems to be dated November 25 (p. 33), and Patton, described on p. 150 as preparing in Washington to command the Casablanca expedition, appears on the following page to be pushing forward "to the center of the island," presumably nearly a year later in Sicily. These are not isolated examples, and there are other sources of confusion. The pre-Teheran correspondence of August-November 1943, for example, appears without apparent rhyme or reason in the same section, but ahead of, the United Nations Declaration of January 1942, and the Yalta agreement on Russia's entry into the war against Japan is mistakenly labeled "Teheran Conference: Military Conclusions" (p. 107).

Buchanan, in sum, has produced not documentary history, but rather a collage which can be examined with enjoyment and even profit, but can be used only with difficulty and should be used only with caution.

MANFRED JONAS

Union College

How the Bureaucracy Makes Foreign Policy: An Exchange Analysis. By David Howard Davis. (Lexington, Massachusetts: D. C. Heath and Company, 1972. Pp. 164. $12.50.)

How the Bureaucracy Makes Foreign Policy is an ambitious title for a short book. This effort by Professor Davis of Rutgers University fails to fulfill the promise of the title but does present a model of bureaucratic interaction that may be useful to describe and analyze some aspects of the public policy-making process.

Davis seeks to use terms from microeconomics to clarify the relations between the State Department and three domestic agencies—Labor, Treasury, and NASA. Rather than focus on crisis decision making in foreign policy, Davis chooses to analyze some day-to-day bureaucratic activities that feed into the development of foreign policy. He looks at issues that have occasioned a large degree of cooperatin rather than conflict between the agencies. Among those studied are site selections for a space tracking station and for an international labor conference, and the labor attaché program.

Perhaps the basic starting point for microeconomics and exchange theory is the concept of a commodity, which Davis uses as a "generic term to describe anything that is exchanged," or the "bundle of services" that any agency produces (p. 15). After laboriously creating four categories of commodities based on the relative amount of transformation undergone through the agency's "bureaucratic manufacturing process," Davis fails to treat these categories of commodities in a sophisticated, systematic way in his ensuing discussions of specialization, bargaining, and production. The problem stems from the artificiality of the categories. Is there, for instance, an analytically meaningful difference between "refined" and "invented" commodities?

The section on utility and preference, however, is the strongest and most suggestive portion of the book. Davis sees interagency cooperation as a system of commodity exchanges taking place over time and involving flows of commodities rather than a specific *quid pro quo* relationship. The basis for the exchange is that agencies value the output of other agencies and are willing to give up some of their output in order to get something they value. "An agency puts a low utility on its own product because it has so much and a high utility on the product of other agencies because it has so little. . . . [An agency] was often quite willing, even eager, to reveal its secrets (on which it placed a low utility) in return for the scarce secrets of the other agency (on which it placed high utility)" (p. 49).

Davis's most interesting contribution is his use of indifference-curve analysis to suggest the direction of decisional bargaining between agencies in foreign policy questions where the "issues tend to dichotomize into political versus scientific functions" (p. 51). Although the agencies involved did not use indifference-curve analysis to reach their decisions, the indifference curves did approximate the factors that were considered. The selection of a site for a space tracking station was one such application. Although I question how the indifference curves were generated for the example given, the technique is applicable in this case and seems promising under similar conditions.

In a general way we can see how exchange analysis provides insights concerning the relations between agencies, especially the decline of the State Department's substantive role in foreign affairs:

> In negotiating the terms for coalitions the State Department often appears willing to relinquish a large quanity of substantive commodities providing that it maintains its titular jurisdiction. The State Department values the myth of foreign policy primacy more than the reality. . . . Foggy Bottom doctrine centers on maintaining the department as an umbrella over all activities [sic] in the arena. What transpires under the umbrella is open to compromise. . . . The State Department sometimes appears to be more concerned with deference to its right to lead than with any particular direction in which to lead (p. 75).

Yet there is much that exchange analysis cannot explain except at a very superficial level. For example, Davis retreats to historical analysis to describe the *modus vivendi* worked out between Treasury and State over internal finance, bilateral aid, and a host of problems where bureaucratic jurisdictions overlap.

Professor Davis sees exchange theory as an egalitarian organization theory. "Each actor is valued for the contribution it [sic] can make. The interaction of the various actors works to maximize the total resources" (p. 139). Yet the idealized exchange theory posited by Davis cannot work unless the hierarchy determines that bargaining will take place. Pushing his economic analogy, Davis notes:

> The market for exchange among the agencies is tiered in a fashion analogous to the tiering of the economic market into assorted wholesale and retail levels. Interagency exchange must occur at the right level and bargaining must take place at the right level. When it begins at too low a level it cannot be successfully concluded (p. 80).

Who decides what is the right level for administrative bargaining? The market? An "invisible hand?" Or the hierarchy? I will put my bets on the hierarchy.

The failure to deal with the interpersonal dynamics of large-scale organizations and political control through the hierarchy are the glaring weaknesses of this effort. There is no room for nonrational or irrational actors. There is no room for the personalities of individual actors. There is no room for a Lyndon Johnson, a Maxwell Taylor, or a Henry Kissinger. Still, it would be interesting to see such an analysis applied to the United States involvement in Vietnam, or the National Security Council/State Department relations concerning Bangladesh, or any number of other critical foreign policy issues.

As a heuristic device, Professor Davis's exchange theory is interesting and somewhat helpful in describing the broad outlines of some kinds of bureaucratic interactions. Like most other attempts to force political phenomena into economic models, however, it will probably have limited utility, and only at a suboptimal level.

FRED A. KRAMER
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The Birth of Mass Political Parties: Michigan, 1827–1861. By Ronald P. Formisano. (Princeton, N.J.: Princeton University Press, 1971. Pp. 356. $12.50.)

This is one of a number of recent books emphasizing the role of ethnocultural forces in nineteenth century American politics. It is also one of the best. The book makes two major contributions. First, it demonstrates the primacy of ethnocultural factors—especially religion—in determining party identification and voting behavior, not only during the period of party formation in the 1830s but throughout the antebellum period. Professor Formisano views Michigan politics as an extension of a conflict of religious value systems and a conflicting view of the role of religion in society. The conflict was between "evangelical Protestantism" and "antievangelicalism." Religion was especially important in shaping the anti-Democratic opposition, from Antimasonry to Whiggery, the Know-Nothings and the Fusion movement, and Republicanism. Religious differences determined political configurations largely because of aggression on the part of evangelicals, mostly of Yankee origin, who saw society as a unified whole and refused to separate the secular (politics) from the religious. Formisano asserts that the "Christian politicizing" of the evangelicals, resulting primarily from the "moral ultraism" generated by the revivals, not only shaped the nature of the Antimason, Whig, and Republican parties, but also "helped prepare the electorate for its reception of parties" and thus "should be regarded as a long-run cause of political party development" (p. 104).

Although Michigan achieved statehood in the turbulent Jackson-Van Buren era, it was not the Monster Bank or any other national issue that stimulated party development. What triggered party activity was the battle in 1835 over whether the state constitution should permit aliens to vote. Evangelicals viewed alien suffrage (and political power) as a threat to their Yankee value system and the social order. The result was a Yankeefied Whig party and a party system divided along evangelical-anti-evangelical lines. Defeated in 1835, the Whigs nevertheless persisted in a campaign of "political revivalism" that stressed moral issues that perpetuated ethnocultural cleavages: temperance, Sabbath observance, and anti-Popery. In a particularly interesting chapter, Formisano describes the religious basis of antipartyism among Whig evangelicals that accounted for the party's lack of development relative to the Democrats. "To many evangelicals *party* in politics resembled Romanism in religion, namely submission of individual reason and conscience to a central authority" (p. 79).

The second contribution is Formisano's interpretation of the complex political upheavals and consequent realignment of the mid-1850s that produced a solid Republican hegemony in the state by 1860. Again, it was not national economic or sectional issues but local concerns that destroyed the old party configuration *before* the Kansas-Nebraska Act—namely, Protestant-Catholic strife over schools and evangelical agitation for Maine law prohibition. Nevertheless, other factors were necessary to achieve permanent realignment. The first was Know-Nothingism, particularly virulent in Michigan, which exacerbated the religious strife; the second was the Kansas-Nebraska Act and "bleeding Kansas" which gave the emerging coalition (known variously as Fusion, Independency, American, anti-Nebraska, and Republican) a new, all-powerful issue configuration which Formisano labels "anti-Southernism." Anti-Southernism—used as a plea to resist domination of the North by a Democratic slaveocracy—permitted the nascent party to graft an anti-aristocratic appeal to its evangelicalism. While anti-Southernism was the dominant Republican issue between 1854 and 1860, cultural issues also remained important. Know-Nothings were an active and effective if quiet element in the new party, and the Fusion/Republican legislatures between 1855 and 1859 passed "an iron-clad Maine law," anti-Catholic laws preventing ecclesiastical ownership of church property and regulating nunneries, and a voter registration law to prevent ineligible aliens (solidly Democratic) from voting. "Indeed," Formisano writes, "the Republican party, though perhaps less explicitly, was far more the *Protestant party* than the Whig had ever been" (p. 324). Moral reform to Michigan Republicans meant evangelical Protestantism; other moral reforms—the abolition of slavery and Negro suffrage, both unpopular with a Negrophobic electorate—were minimized or ignored to avoid any "Black Republican" stigma.

Formisano combines extensive use of traditional sources, an impressive array of data, and a solid background in relevant social science literature to produce a model work of scholarship. The book does have its defects, however minor. For example, Formisano's definition of "evangelical" is so framed as to include only Presbyterians, Congregationalists, and Baptists. How does his use of "evangelical" differ from the "pietist" category of other historians? (I get the feeling that Methodists were not included as evangelicals because they tended to vote Democratic.) Aside from this categorization of a few Protestant groups, my other criticisms are mere quibbles about what is a first-rate book.

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The Great Campaigns: Reform and War in America, 1900–1928. By Otis L. Graham, Jr. (Englewood Cliffs, N.J.: Prentice Hall, Inc., 1971. Pp. xiii, 386. $8.95.)

The political and social ferment of the first three decades of the twentieth century form the subject of Otis Graham's *The Great Campaigns.*

Incorporating the latest scholarship on Progressivism, Graham provides a highly readable synthesis of the reform years. Granting an element of altruism in the reform impulse, he sees the movement as primarily motivated by enlightened self-interest. He is careful to point out that reform legislation aided the interests it purported to control, but he still remains somewhat uncritical. For example, after a survey of the origins of railroad legislation and the interests that supported it, he concludes that "when formerly private decisions are politicized, whether aggrieved groups actually improve their position or not, the new procedure reduces the feeling of powerlessness among the unconsulted, which is a clear gain for any democratic society" (p. 39). The illusion of participation may quell discontent, but the gain for democracy is certainly questionable. Similarly, comparing Theodore Roosevelt to John F. Kennedy, Graham notes that the rhetoric of both men far excelled their achievement. Yet he asks, "who can say that such revivals of simple faith . . . , faith that forces are being brought under the control of decent men, are not in certain circumstances worth more than the scornful radical ever imagines " (p. 42). There is, then, a sense of contentment with Progessivism that is rather jarring when juxtaposed with the details of the limitations of the reform achievement that Graham conveys.

In the second section of the book, Graham describes America's move toward involvement in the First World War. The treatment here is subtle, and while Graham sees the nation's plunge into war as inevitable after a certain point, he is critical of Wilson for closing his alternatives in the early years when he had greater freedom of movement. "If it is too much to ask of any mortal political leader that, given the circumstances of Spring, 1917, he choose division and national humiliation over unity and pride, it is not too much to ask for a different line of diplomacy reaching back to 1914, one that would not allow such restricted and self-defeating alternatives to hem him in" (p. 91). Graham wishes that Wilson had used the moral fervor he brought to the war to intensify domestic reform efforts. He does not, however, deal with Wilson's lack of will in that direction even if circumstances had permitted.

The final section of the book brings the story down to 1928. Graham concurs with most historians that "the war was fatal to the humanitarian, liberal component of reform" (p. 102). The underside of the reform impulse remained, however, and manifested itself in prohibition, Klanism, and the antievolution crusade. Following recent scholarship, Graham notes that the most lasting governmental reforms of the decade were administrative. "The 1920s in state government was a time of the integration and rationalization, as well as the expansion, of modern governmental bureaucracies" (p. 133).

The documents following the text are well selected and the bibliographical essay will be useful to students.

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The Political Image Merchants. Edited by Ray Hiebert, Robert Jones, John Lorenz, and Ernest Lotito. (Washington, D.C.: Acropolis Books, Ltd., 1971. Pp. 310. $4.95.)

Why Vote? By William C. Mitchell. (Chicago: Markham Publishing Co., 1971. Pp. 196. $2.50, paper.)

The Election Game and How To Win It. By Joseph Napolitan. (Garden City, N.Y.: Doubleday & Co., 1972. Pp. 300. $6.95.)

The Election Process, Voting Laws and Procedures. By Allen Reitman and Robert D. Davidson. (New York: Oceania Publications, 1972. Pp. 154. $3.25.)

The Election Men. By David Lee Rosenbloom. (New York: Quadrangle Books, 1973. Pp. 182. $6.95.)

Campaign Craftsmanship. By Edward Schwartzman. (New York: University Books, 1973. Pp. 276. $8.50.)

The New How To Win An Election. By Stephen C. Shadegg. (New York: Taplinger Publishing Company, 1972. Pp. 189. $6.50.)

The Necessary Majority. By Robert C. Wood. (New York: Columbia University Press, 1972. Pp. 95. $5.95.)

These books are of unequal merit and importance. By far the most interesting and valuable is Joseph Napolitan's primer, intermingled as it is with biographical anecdotes on how to win elections in modern-day politics. Up to date as it is, however, with references to the revolutionary implications arising from the development of the electronic media, the message of the author has very little to add to the old classic written by Tammany boss Edward Flynn in his *You're The Boss* (Clifton, N.J.: Augustus M. Kelley, publisher, 1947.) It all gets down to the individual voter and how to reach and influence him.

What is new, however, is the professional political constultant. Professor David Rosenbloom, of Hamilton College, in a clear and well-written critique, looks upon this development as a threat to the American party system. The emphasis, he argues, is on winning and not the issues. Although this emphasis does not appear to be a new one, in the sense that winning elections seems to have been traditionally the name of the game, authors Napolitan, Schwartzman, and Shadegg provide effective justifications for their roles as essential for a healthy body politic.

Napolitan sets forth three essential steps to winning an election. It is necessary first of all, he urges, to define the message that the candidate desires to communicate; second, to select the vehicles of communication that he wants and needs to use; and third, to implement the communication process. Although he dwells on the important rôle of emotion, rather than reason, in political campaigns, he also notes the importance of issues. He states as his guiding principle that the intelligence of the voter should never be underestimated but that the amount of information that the voter has should also never be overestimated. Napolitan points out that he only works for Democrats, whereas Shadegg's reputation was made as a result of his work with Barry Goldwater's senatorial campaigns and his identification with conservative candidates. Both, however, as well as Schwartzman, combine their intelligent practical advice with recognition of the importance of issues. Shadegg, for example, believes that political promises should relate to the major concerns of a constituency and be consistent with the character image of the candidate, as well as deal realistically with the anticipations and expectations of the voters. Similarities in these books are plentiful. They are effective primers of practical politics. The authors have had intensive practical experience. They insist that political images of their candidates must be consistent with the real man or they become false and ineffective. The authors emphasize the importance of research, particularly the important use of public opinion polling as research tools. They insist that important as television may be, radio is also vital to an effective political campaign.

Napolitan is not just content with analysis. In a chapter on *Changing the Rules of the Game,* he sets forth a number of recommendations to reform the system, including a proposal for free and equal television and radio time, limiting the time for campaigning, free mailing privileges, the direct nomination of candidates for President on a national primary day, abolition of the electoral college, and the direct election of the President.

The Shadegg book, an updated version of the 1954 original edition, holds that elections are won by appealing primarily to the indifferent voter rather than by emphasizing drives to bring one's own voters to the polls. The book contains many shrewd and candid lessons of value to all office seekers. Like Napolitan, Shadegg believes that the final two weeks of the campaign are crucial and urges that the bulk of television scheduling and direct mailing take place during that period, with primary emphasis in the 48-hour period before Election Day. In an interesting commentary on public opinion polls, he emphasizes the research tool as a way of identifying the major concerns of the constituency, but points out that voters do not require that a candidate agree with them. Voters do insist, however, that the candidates recognize and appreciate their fears and their problems and indicate some understanding of those problems and fears. Interestingly enough, the author has no patience for the new style "image-makers" who use the ten- and twenty-second television spot pattern to sell their candidates.

Schwartzman, a professional political consultant whose book is primarily aimed at the first-time candidate for public or private office, emphasizes the day-to-day details of campaigning. He too notes the problems that arise from growing campaign costs. His theme is that one need not be unethical or rich to run successfully for office. The author's experience has been primarily in the New York area, and his illustrations therefore arise out of that experience, but the immense detailed treatment of how to wage a campaign will be of assistance to all candidates.

The Political Image Merchants was prepared as a campaign workers' handbook. Published in 1971 after a late 1970 conference at the University of Maryland, much of it has already been outdated by the 1972 campaign and the Watergate hearings. Nevertheless, the sections dealing with new political techniques, television, survey research, and the use of computers and data processing represent very good summaries by some of the country's outstanding experts in these fields. A separate section on ethics has particular relevance, and concluding essays by Ithiel de Sola Pool and Robert E. Lane on future political developments provide an excellent conclusion to this volume.

The Mitchell book is a small tract urging 18-year-olds to vote in national elections as a major means of influencing the system. It is aimed primarily at college students, although there is an attempt to present an appeal to noncollege youth as well. It is on the level of a civics book rather than a college textbook, and its efforts at presenting a statistical and scientific base for evaluating campaigns for public office are not successful.

The Reitman and Davidson work is a handbook on current election laws and procedures. It covers such subjects as who has the right to vote, how registration is handled, how the voting system works, as well as a chapter on the election process. Its usefulness is limited only in that it deals with an increasingly changing field.

Professor Wood's book deserves better than to be treated as part of this kaleidoscopic review. Now serving as President of the University of Massachusetts, the author writes as a former Secretary of the Department of Housing and Urban Development. This is a series of lectures on the housing and urban crisis, a top-priority challenge for social action since the 1960s, but one still not effectively met. Wood claims that the urban crisis can only be met effectively and successfully if it is linked to the concerns of Middle America—the suburban and nonurban majority. Basically, the urban crisis is a minority problem and a majoritarian system comes uneasily to the resolution of these kinds of problems. He quotes Seymour Martin Lipset, who states that the majority of Americans are leftist on the issues but politically right on the tactics. He then moves on to propose some simple but revolutionary suggestions for solving the urban crisis. He would provide incentives for directing population and development into those areas that will minimize urban tension and concentration. He would provide governmental regulatory intervention to control land values and comes up with a program for land reform in urban America which would include the possibility of public ownership and public planning. What is not adequately explained is how these rather drastic programs are likely to appeal to the Middle America whose support, he urges, is necessary if our society is successfully to meet social problems.

Max M. Kampelman

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Spending To Save: The Complete Story of Relief. By Harry L. Hopkins. (Seattle: University of Washington Press, 1972. Pp. 197. $7.50.)

When Harry Hopkins became the head of the Federal Emergency Relief Administration, he spent $5 million in the first two hours in his new post, summing up his administrative philosophy in four words: "Hunger is not debatable." This statement becomes the basic theme of the book, but it is not, despite the subtitle, the complete story of relief. The book, which appears in the dull format of a staff production, is a reissue of the original 1936 work. The introduction by Roger Daniels is new, however, and attempts to place the work in a contemporary setting.

Nearly half the book is devoted to describing the Depression under President Hoover, including: the rise of unemployment with the usual confusing statistics; the decline of the states and local governments in relief measures; and the continuous efforts of some members of Congress to provide federal appropriations to the states. The book contains many direct quotes which have become standard and classic statements on relief. For example, from Governor Wilber Brucker of Michigan: "The people of Michigan will take care of their own problems." And from Governor Conley of West Virginia to President Hoover: ". . . I feel that public opinion supports the position you have taken in opposing any step that would bring a dole system into existence in the United States" (p. 50). A great deal of emphasis is placed on the private and voluntary relief efforts of the Red Cross and Community Chest and people's own individual initiative with the hope that "Prosperity was just around the corner" (p. 15).

But as was discovered, private groups as well as individuals were caught in the circle of poverty and could not generate funds. Local relief was inadequate to take care of the millions of unemployed either through voluntary efforts because sources of revenue were drying up or through the states because of reduced tax values, the large amount of uncollectible taxes and bank failures, and the rigid constitutional debt limitations. Even when federal funds were appropriated, such agencies as the Reconstruction Finance Corporation withheld or impounded some of the requested funds.

Although Senators LaFollette, Costigan and Wagner were leaders in the effort to provide some form of federal unemployment relief, nothing really happened until two months after Roosevelt's inauguration when he approved the Federal Emergency Relief Act of 1933 which appropriated $500 million to aid the states in meeting their immediate relief needs. Hopkins describes the Herculean task of New Deal programs and readily admits problems in the administration of such relief efforts. After putting four million men to work in the Civil Works Administration, he told Roosevelt: "Well, they're all at work, but for God's sake don't ask me what they're doing."

This is basically a study of a bygone era which has important significance but also has a great deal to contribute to the understanding of contemporary problems. The emphasis placed on local and state priorities for governmental action, the dread of a large federal bureaucracy, and the fear of overspending through federal appropriation are problems viewed by the current national administration which, under the principle of reorganization to streamline the bureaucracy and reduce federal spending, is proposing to turn back to the states some primary governmental functions and to control excessive spending through the possible impoundment of funds. In this assessment, made thirty-six years ago, Hopkins stated the questions of social equity and justice sought by so many groups today and disrupted the assumption that poverty or failure to function adequately within the system was essentially the fault of the individual rather than the system. National problems still require national solutions.

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The All-Volunteer Army: An Analysis of Demand and Supply. By K. H. Kim, Susan Farrell, and Ewan Clague. (New York: Praeger Publishers, 1971. Pp. 230. $12.50.)

The Case Against a Volunteer Army. By Harry A. Marmion. (Chicago: Quadrangle Books, 1971. Pp. 107. $1.95, paper.)

The only similarities between these two books is that they both are concerned with the all-volunteer army. Any likeness ends at this point. The first is a scienfically approached, well-presented attempt to determine if an all-volunteer army is budgetarily feasible. The second is a polemic, and a very bad one.

The study by Kim, Farrell, and Clague, originally done under contract for the Department of the Army, examines probable manpower requirements and supply responses in an all-volunteer environment. The demand for military manpower is analyzed and projected using an inventory approach. This inventory model simulates changes in the size and structure of the military over time and allows for a determination of the number of recruits needed each year to maintain desired strength. The difficulties encountered in this approach are significant. Since "required" force levels change frequently, as do other important variables. such as the military's definition of eligibility for reenlistment, the forecasting model must be very flexible. The authors recognize this problem and carefully document their methodology so that it can be altered to cope with any change military planners might want to impose on the model.

The supply side of the model is approached by examining the characteristics of the civilian manpower pool and applying elasticity of supply coefficients from the historical experience. Because of the variety in the elasticity of enlistment response reported in other studies, the authors use a range of elasticities and present their results. There are many problems in estimating the supply response due to the fact that such factors as civilian employment rates, levels of hostility, and the status of the military profession are exogenous to the model. Yet the authors are aware of the complexity and difficulty of their project and take care to document their approach. The results of the study indicate that if the assumed conditions hold, a volunteer army will require a 38% military to civilian relative pay increase to attract the needed enlistees.

If the study has a major fault it is that the authors (intentionally) did not examine to what extent women and civilian personnel can be substituted for male military enlistees. Such substitution might permit realization of the desired force structure with a much smaller budgetary outlay.

The book by Marmion is anything but a tightly reasoned essay against the all-voluntary army. In the preface Marmion states: "I hope to be able to present both sides of the argument fairly, but my overall interpretation will be against the all-volunteer concept." Marmion is, however, in no position to present both sides of the argument since at many points in the book it is clear that he does not understand the case for an all-volunteer army. In addition it is apparent from his citations that he has not examined the relevant literature concerning the volunteer army. Due to a space constraint we will present only a *few* illustrations of Marmion's misunderstanding of the basic issues.

"One central question permeates all proposed solutions to meeting our national military manpower requirements: How can we make the best use of the available manpower pool?" (p. 11). This paternalistic view of the allocation of young men might lead to any conceivable result, though one later finds confirmation that Marmion supports some form of national service. Marmion is talking about allocating resources that he or "we" do not own, and in so doing he simply avoids the basic question of individual freedom surrounding the military manpower policy in a democratic society.

"The question of the cost of a volunteer army remains ambiguous at best" (p. 34). We interpret this statement and the surrounding discussion to indicate that Marmion is confused about the basic distinction between opportunity and financial measures of cost.

"The volunteer armed force will become a refuge for the poor, and as such will reduce pressure for reform. After all, why bother with reform when the poor and disadvantaged can simply be shunted off into the military" (p. 41). This statement is logically inconsistent. If a volunteer army is financially costly, as Marmion argues elsewhere, then young men become "unpoor" by joining, if they wish. It is inconsistent to worry about the "cost" of a volunteer army and about shunting off the "poor and disadvantaged" into the military. One could easily take Marmion to mean that rather than offer higher military wages to those, including the poor, who wish to enlist, he would prefer to retain lower military pay levels so as to maintain some mysterious "pressure for reform." On this criterion we prefer reform now to "pressure for reform."

Finally, Marmion simply gets out of bounds, ignoring any rational debate. At this point his book should not be taken as a serious statement against the all-volunteer army:

> "One should respect the stated motives of those who seek an end to compulsive conscription. Yet I cannot help feeling that if they were completely honest about their position they would admit they are opposing the draft because they do not want to serve, thus their position may well be tainted by a very pragmatic self-interest. Too many of those who advocate increasing military pay are people who do not have to worry about such things as military obligations themselves because they have the education and training to get equally well-paid or, indeed, better paid positions. In point of fact, an all-volunteer army would liberate the middle class from the legal necessity of serving, but commit others to compulsory service by economic circumstance. Is this not, in effect, forcing the poor and the less fortunate into the armed forces? Is this truly democratic?" (p. 46).

This sort of personal attack on those who advocate a volunteer army is unprofessional, and it opens the door for a round of recriminations of this sort. One could easily counter by saying that Marmion supports a draft because it lowers the *pro rata* tax cost to him of military manpower. This form of argument is unnecessary and rules out serious scholarly dialogue on the issue. Perversely, though, the attack on the motives of those who advocate a volunteer army can be seen as a capstone to Marmion's little book. Frankly, it is the worst that we have seen on the subject.

In summary, the work by Kim, Farrell, and Clague is the type of study that aids policy makers by reducing the uncertainty surrounding their decision making. The work by Marmion with its illogical statements and inflamatory rhetoric only confuses the issue and renders decision making more difficult.

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Innovation In New Communities. By Brown Miller, Neil J. Pinney, and William S. Saslow. (Cambridge, Mass.: M.I.T. Press, 1972. Pp. 301. $15.00.)

The authors make no attempt to disguise their advocacy planning approach to "new communities" as a means of dealing with current urban problems. Although professing the intent of circulation among a wide and diversified audience, this book is plainly aimed at city-planning types.

"New communities" are defined as "projects involving the creation of an urban environment rather than the development of limited-use projects in predetermined (or existing) urban patterns." The "new communities" approach is distinguished from more traditional large-scale residential development projects by the fact that they attempt to incorporate a "Total concept" of living (schools, industry, business and commerce, parks, etc.) rather than the more restricted "dormitory" uses of traditional residential development projects.

The authors do not attempt to assess the recent innovations either in housing systems proper or in the design and planning processes supporting them. In terms of magnitude alone, they feel that this should constitute a separate study and so devote little space to it even though innovations in housing design constitute a very large part of the "new community" approach.

The central argument of the book entails an assumption of a crystalized national urban growth policy. The basis for that assumption is outlined in Chapter IV, specifically with regard to Title VII of the 1970 Housing and Urban Developing Act. The authors feel that any balanced national growth policy must incorporate the "new communities" approach, because "new communities" can be planned and developed in a manner which adapts to and incorporates changing technologies and their accompanying social change, thereby facilitating orderly growth.

The authors stress that a national urban growth strategy is one which would open up the suburbs to blacks, increase the income of cities and their residents (partly by reducing dependent population densities in the inner city), and promote population migration to lagging regions. New community development could certainly provide a *mechanism* for achieving the first and third elements of that strategy.

The book is very forward-looking in attempting to isolate social trends and in matching them with complementary technological innovations; especially the *product* (e.g., transportation) and *process* (e.g., health and educational systems) innovations now being adopted to many "new communities." Perhaps most important, for the administration of these communities, is the effort (in part B of Chapter IV) to develop a financial model to determine the proper "mix" of government services in these "new communities." The model matches those services which the community is *willing to pay for,* with the *fiscal capability* of the community to provide those services. With further inputs of a more sophisticated nature, the model will be able to predict the degree of state and federal support necessary to provide the desired "mix" of governmental services.

The treatment of service areas for various functional and subfunctional services requested by an urban area is very helpful. It includes an appendix which summarizes the population levels estimated as necessary to sustain and provide economies of scale for clusters of education, recreation, health, employment, transportation, commercial and "institutional" services. This is of course a particularly knotty problem and the authors do not attempt to "prove" any of the estimates they have culled from a variety of sources and authorities. They do present data showing a cultural difference—i.e., that American new communities have substantially lower population densities than those in Europe. However, this factor is omitted from a list of eleven design criteria proposed for establishing service areas.

This book should have had an index.

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Political Economy of Urban Areas. By William B. Neenan. (Chicago: Markham Publishing Company, 1972. Pp. 344. $9.95, cloth; $4.95, paper.)

William B. Neenan, Associate Professor in Social Work and in Economics at the University of Michigan, has written an excellent book which focuses on fiscal relationships, fiscal disparities, and public policy needs in metropolitan areas. In his first chapter, after sketching a pessimistic picture of the condition of metropolitan areas in the United States, Neenan states that "the remainder of this book, however, is based on the premise that it is possible through wise policies and courageous action, to brighten a bit the prospects for metropolitan areas" (p. 43). Many before Neenan, of course, have entertained similar hopes. Unlike others, however, Neenan believes that the basis for policy prescriptions should be developed from analytical models and quantification. Using economics as a base, but drawing upon other disciplines including political science, Neenan proceeds systematically to examine a variety of significant fiscal and public problems confronting our governmentally fragmented metropolitan areas.

Neenan begins his analysis by developing an "isolated city model" as a "pure case" for analytical purposes. Although such a city is not likely to exist, i.e., it assumes no interjurisdictional public sector and relations, the model permits one to "(1) focus exclusively on the problem of the provision of local public service; (2) determine the internally optimal supply of public services for the city; and (3) derive insights regarding the efficient assessment of tax liability across the citizenry" (p. 46). Using this model, Neenan concludes that "Only in the situation when the marginal tax-price equals the marginal evaluation of a public service for all individuals does there exist both individual efficiency as well as the optimal supply of the service" (p. 49). By itself, the isolated city model does not offer specific policy guidelines for metropolitan services because it assumes that tax and benefit externalities are absent and that public services are pure public goods, i.e., goods enjoyed in common in a situation in which one person's consumption does not subtract from another's consumption. Externalities exist among public services in metropolitan areas, however, and public goods are not "pure" since they are spatially limited in impact, involve rivalry in consumption, and can become exclusive in their benefits. Thus, the "isolated city model" suggests the nature of the problems to be explored, and Neenan uses it as a take-off point for much of his analysis.

Neenan proceeds to analyze quantitatively public sector benefit and revenue flows for a major metropolitan area, i.e., Detroit and six of its suburbs. First, by basing his analysis on cost of service, and by examining the actual use of several Detroit public services by suburban residents, Neenan concludes that there is a considerable public-sector benefit flow from Detroit to its suburbs. Second, Neenan analyzes the demand for public services by developing a model for defining citizen welfare maximization, testing two hypotheses derived from the model with voter and survey data, and constructing a willingness-to-pay index for public service benefits. This analysis suggests that when public sector benefit flows from Detroit to the suburbs are adjusted for willingness-to-pay they are likely to be even higher than when measured on a cost-of-service basis. Third, Neenan examines state revenue grants to the seven municipal governments under study, the contributions to and benefits received from state revenue sharing funds and municipal nonresident income tax contributions to Detroit for the purpose of determining whether or not revenue flows from the suburbs to Detroit provide adequate compensation for the benefits actually received. Neenan finds that compensation is inadequate and that each of the suburban municipalities enjoys a welfare gain from Detroit's public services.

To correct this imbalance, Neenan constructs a state revenue sharing plan which is designed to compensate urban areas for the spillout benefits of their public sector service. In developing this plan for 89 urban areas in Michigan, Neenan (1) estimates the state revenue needed to finance it for a four-year period, (2) estimates the total and per-capita grants which would be received by selected cities, (3) constructs a variable which would determine the annual size of the grant, i.e., a grant ratio which is "a constant fraction of a city's current operating expenditures" (p. 160), and (4) estimates the state personal income-tax rate required to raise the needed revenue.

Neenan also proposes a normative benchmark for evaluating public sector interaction in metropolitan areas. A core concept of the benchmark is the fiscal residuum, i.e., the difference between an individual's perceived benefits and perceived tax liability. After an analysis of the problems of equity and efficiency in public sector interactions, Neenan concludes that "efficiency in metropolitan public sector activities will typically require intergovernmental grants to equalize individual fiscal residua across income classes, under the constraint that the fiscal residuum be maximized for the whole metropolitan area" (p. 192).

Neenan then examines intergovernmental transfer programs and makes some policy recommendations which are designed to move toward the "efficiency criteria'" of "full compensation for net spillover benefits" and horizontal equality of fiscal residua across a metropolitan area . . ." (p. 235).

Finally, Neenan examines local tax instruments and concludes that the income tax should become the primary local general tax.

In his concluding chapter, Neenan states that the proposals which flow out of his analysis (p. 335) add up to substantial fiscal reform. Such reform is needed, however, because metropolitan areas do not operate in the simple world postulated by the "isolated city model."

Neenan's approach is guided by theory, his methods are systematic, and his findings are impressive. It is hoped that Neenan and others will conduct similar analyses in the future, for such studies seem essential if our metropolitan areas are to develop rational premises for policies designed to promote fiscal integration and fiscal equity.

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Indiana University Northwest

The Rise of the Unmeltable Ethnics: The New Political Force of the Seventies. By Michael Novak. (New York: Macmillan Co., 1972. Pp. 321. $7.95.)

Michael Novak's book, like his previous work on philosophy, religion and politics, is rich in insight and passion. The passion gets out of hand occasionally when he is expressing hostility toward the power and life styles of Anglo-Saxon Protestants. But few students of ethnicity or American politics would quarrel with many of Novak's major assertions: political unity depends in part upon cultural pluralism; WASPS have disparaged the cultures of Eastern Europe; Jews and blacks have sometimes failed to recognize and act on the political interests they share with white ethnics; and the substructures of family, neighborhood, community, and church can and should be strengthened by stronger ethnic consciousness and expressiveness.

Novak's ethnic sensibility is frequently brilliant. He exquisitely portrays the subtle pressures on second-generation immigrant children not to be "too" Jewish, Polish, or Italian. He writes sensitively of the sense of community in Eastern and Southern European immigrant life in contrast to the aseptic, rational drive toward mastery by Nordic Protestants. "People who had learned in life that nothing is done without family, without community, without loyalty, without cooperation, had to learn that the right way is to count only on yourself" (p. 96).

Novak is a Slav who loves the culture of his ancestors and resents having fellow Eastern Europeans called pigs or racists because they act to defend their religious and family values. But Novak's resentment sometimes clouds his judgment. White racism, he asserts, has become for the left what communism was for Joe McCarthy. Who on the left does he mean? Gene McCarthy, George McGovern, Ted Kennedy? If the comparison is with Abbie Hoffman, he should say so, but it is hardly apt since Joe McCarthy had enormous power in the Senate and a following in the millions. Novak charges the "intellectuals" with labeling white ethnics as "pig," "fascists," and "racists." Which intellectuals does Novak have in mind? Not those who write on ethnicity, such as Nathan Glazer, Father Andrew Greeley, Patrick Moynihan, Oscar Handlin, or any others I can think of.

It is the WASPS who come in for most of Novak's anger. He asserts that the first eyes which the immigrants from southern and eastern Europe faced were Nordic. But they may just as well have been unsmiling Irish eyes. By the 1890s, Irish Catholics, who certainly had strong loyalties to family, community and church, were functionaries and foremen in the political and economic life of America's large cities. Polish and Italian immigrants who might not have known a WASP if they smelled his after-shave lotion developed fierce hatreds for the Irish and each other, as Novak knows.

Novak is right in insisting that most WASPS did not and do not welcome ethnicity. From Emerson and Melville to Henry Adams and Theodore Roosevelt, they disliked and distrusted the celebrants of ethnic community, sensuality, mystery, and passion; they believed in the ultimate triumph of an individualistic Americanism. That vision of a good America has been harmful and dangerous to human—not just ethnic—life; and Novak does not miss an opportunity to point that out. Of course the ethnics have had to fight against WASP prejudice, but that does not mean that they had been entirely WASP-washed. Woodrow Wilson thought no one could be a good American who did not speak English as his first language. Now, bilingualism in Spanish-speaking areas is an accepted fact. Not long ago Harvard University placed strong barriers against the admission of Jews. By 1973, kosher food was provided by Harvard caterers for the orthodox. Theodore Roosevelt wrote in his diary that the Irishman is by nature "brutish" and "venal." But Americans of all backgrounds have made a hero of the nation's first Irish Catholic president.

The main point of the book—that politics should draw on "resources other than Anglo-Saxon history and values"—is not only acceptable, but is a fact of life and has been for a long time. The American polity, despite its largely Protestant basis, has allowed the development of a huge private Catholic school system and has encouraged the celebration of national ethnic holidays; it has permitted national Catholic and Jewish minorities to influence American foreign policy in behalf of coreligionists overseas. And in one or two generations it has elevated Irish, Jewish, Polish, Italian, Japanese and many other ethnic Americans to positions of influence and power (Novak himself wrote speeches for vice presidential candidate Shriver in 1972). To be sure, the ancestors of Agnew and Muskie pronounced their names differently, and, like John F. Kennedy, those two aspirants to the presidency have been substantially acculturated to the dominant culture. But how could it be otherwise? If a Greek, Pole or Irishman wants to appeal for the votes of all Americans, he can only retain so much of his ethnic heritage and still communicate effectively with the majority of his constituents. The personal price may be too high for many. But for those who wish to be totally ethnic, such as the Amish, it may be necessary to eschew politics altogether.

Novak's discussion of and quest for a new ethnic politics demands the clarification which he has given in articles written after publication of the book. The plea which Novak makes for blacks, Jews, and white ethnics to join against their WASP tormentors does not seem well founded if, as Novak says in the book, the new ethnic politics is to be "cultural" and not "interest" politics. He asks black leaders not to be too Protestant with Catholic ethnics. But they *are* Protestant! Why should they recognize that "Polish is beautiful" when Polish Catholics attack their family life styles, to say nothing of their battles with Poles for jobs and benefits? How can black leaders stop being evangelical in style when generations of them have come from evangelical Protestant churches? When Novak asks blacks to recognize Joe Colombo as a fellow sufferer under Nordic prejudice, he asks more than can be expected from even the most imaginative black construction worker.

The new political alliance as prescribed by Novak calls for Polish-Americans and Jewish-Americans to come together. He urges that Jewish-Americans protest restrictions upon Catholicism in Poland and Poles in the United States attack anti-Semitism in the old country. Once again, it is culture as well as interest that keeps some Polish and Jewish-Americans apart. Novak is correct in writing, "rare is the Jew who is not bitter against the Poles, rare the Pole not resentful of that bitterness." But the hostility is strong because Polish-Americans inherit what may have been the deepest, most pervasive and powerful Catholic anti-Semitism in history. Alliances between the two groups may be possible on the basis of interest, as in the New Deal, but hardly on the basis of cultural affinity or historic memory, at least not yet.

Despite weaknesses in the book, I admire Novak's positive biases. America desperately needs a politics which gives strength to families and communities including WASPS trapped by the cult of individualism; and, as Novak argues, ethnic consciousness and expressiveness do provide one answer to the desperate search for a sense of belonging in America. There can be no strong individuals without strong, loving families. To be strong and loving, families need to be rooted in and supported by communities. Ethnic communities are the only traditional organic communities in the United States. Long live ethnicity! But give the WASPS their due. If it weren't for three of their institutions—religious freedom, universal suffrage, and the right to assemble and petition—the rest of us would be a lot worst off, and, in some ways even less ethnic.

LAWRENCE H. FUCHS

Brandeis University

Parties: The Real Opportunity For Effective Citizen Politics. By John S. Saloma, III and Frederich H. Sontag. (New York: Alfred A. Knopf, 1972. Pp. 390. $7.95.)

Saloma and Sontag have tackled an ambitious task in this book—to provide "a comprehensive analysis of the American political parties, how they actually functioned, and what could be done by concerned and politically active Americans to make them more responsive and effective institutions" (p. xv). While the task is not fully accomplished, the authors have made an important contribution to the descriptive and prescriptive literature on party reform.

In the course of the book, the authors examine most of the major institutions of American parties, from the national conventions to the state party structures. Many institutions that are not strictly party institutions but affect the parties' performance, such as the media and the new campaign management industry, also receive their appropriate share of attention. Each aspect of the parties is described and evaluated in terms of its contribution to "party modernization," by which the authors mean "the development of citizen or participant parties" (p. 349). To remedy the deficiencies they find, the authors propose forty-one specific recommendations ranging from the major—"the congressional parties can contribute to the governmental capabilities of the national parties through improved procedures for recruiting congressional candidates and making committee assignments, through training and development of partisan congressional staff, and through the establishment of specialized party policy units" (p. 148)—to the trivial—"political consultants should take initiatives to increase public understanding of their role in politics and to continue to improve and upgrade their profession" (p. 313). I am dubious about the practicality of some recommendations and about the significance of others. I also suspect that some may have distinctly unintended consequences. But, overall, the recommendations add up to an impressive and ingenious set of ideas.

Political scientists, even those who, like me, are generally sympathetic to the idea of participant parties, will be disappointed in Saloma and Sontag's failure either to try to establish the desirability of such parties or to meet the objections of those who attack such parties as positively nefarious. James Q. Wilson's trenchant critique (in the *Amateur Democrat*) is perhaps the most famous representative of this line of argument. Wilson asserts that such parties are inherently elitist (because only the well-educated or ideologically extreme will participate) and that the centrifugal forces set up by all that participation will make parties inherently irresponsible (in both senses of the term).

Perhaps one should not criticize Saloma and Sontag for this omission. They obviously intended their book to be a battle plan for the already convinced rather than an analytic treatise for political scientists. In fact, an "action guide" at the end of the book directs specific groups to particularly relevant pages of *Parties*. The list ranges alphabetically from "candidates and campaign managers" to "youth," but does not include readers of the *APSR*.

Even those interested only in a battle plan will be somewhat annoyed by the authors' frequent failure to include relevant facts at appropriate points. For example, Saloma and Sontag criticize the style, tone, and general complacency of the Republican counterpart to the McGovern Commission (inappropriately known as DO) and quote disapprovingly the view of "one Republican professional" that "many of the things the Democrats are now trying to reform, the Republicans took care of one hundred years ago" (p. 35). Yet, the authors nowhere evaluate the truth of this claim or present the relevant convention rules.

In spite of these two caveats, Saloma and Sontag have written an interesting book that will undoubtedly be widely discussed. Members of our profession interested in American political parties will want to read it.

DAVID NEXON

Pitzer College

Making Federalism Work: A Study of Program Coordination at the Community Level. By James L. Sundquist with the collaboration of David W. Davis. (Washington, D.C.: The Brookings Institution, 1969. Pp. ix, 293. $6.95.)

According to the rhetoric of modern statecraft, to preserve freedom is to engineer a coup and establish a military dictatorship; to save something is to destroy it. Sundquist and Davis's *Making Federalism Work* is written in this tradition of statecraft. To make federalism work is to eliminate it.

The empirical base for this study consists of efforts by the Johnson administration to engage in its rhetorical "war" against poverty through community action agencies, to attack urban ills through the model cities' program, and to do a rural equivalent through rural area and community development. Sundquist and Davis take the Johnson rhetoric literally and formulate their own analysis as though comprehensive planning were possible and decision makers were sufficiently omniscient and omnicompetent to be able to see the whole picture and act systematically with reference to political configurations as wholes.

The empirical chapters use bits and pieces of data, observation and commentary to reveal that experience with community action agencies, model cities, and rural development is varied. This permits Sundquist and Davis substantial freedom to draw their own conclusions without having to confront evidence that could be used to reject the explanatory inferences that serve as a basis for their conclusions.

The book fails to address itself to a diagnostic assessment of the conditions of poverty, city problems, or rural development. Interesting impressions are conveyed in extensive use of quotations from interviews which reveal substantial suspicion, hostility, and animosity among neighborhood residents about city governments, professional civil servants, and the performance of public service agencies. There are no efforts to pinpoint the sources of institutional weakness and institutional failure that stimulated federal efforts to intervene in the affairs of central city neighborhoods.

The principal format for making the system (federalism) work is to strengthen the authority and effective control of chief executives: mayors, governors, presidents and presidential surrogates. The system works if there is enough clout to make it work. If a command structure guided by the Executive Offices of the President can be superimposed upon the jumble that passes for federalism then it can be made to work. But the guidance of the system "can come from but a single source of authority—the President" (p. 278).

In the post-Watergate era the principal value of this study will be to provide students with documentation to establish that a political science once existed when all problems were viewed as national problems and all national problems were to be solved by referring them to the Executive Offices of the President.

VINCENT OSTROM
Indiana University

In Search of Roosevelt. By Rexford G. Tugwell. (Cambridge, Mass.: Harvard University Press. 1972. Pp. x, 313. $12.95.)

Rexford Guy Tugwell, who first became an adviser to FDR in 1932, has given readers some unique insights into Roosevelt's mind and character. His writing reflects strong admiration—even awe—of the president; yet Tugwell sustains the detachment and skepticism of the scholar. (He also demonstrates a rarer quality among scholars: a warm and charming style.) For these good reasons the Harvard Press is justified in bringing together this selection of Tugwell's essays on Roosevelt. Most of them first appeared some years ago (1948 to 1956) in journals of limited circulation (e.g., *Ethics, Western Political Quarterly, Antioch Review*). Very few of us would have seen all of these articles; younger scholars would probably have seen none of them. This book is a reissue of the separate essays, arranged in a logical, useful, and pleasing order.

For those familiar with Tugwell's past books, especially *The Democratic Roosevelt* (1957), these essays add little to the author's established interpretation of his subject. Tugwell has moved gracefully through the years, from professor of economics (Columbia) and government and teaching service elsewhere, to his present position as a Senior Fellow at the Center for the Study of Democratic Institutions (Santa Barbara). While he respects and praises Roosevelt the man, he has consistently criticized him on two salient points: (1) FDR's rejection of the concept of "concentration and control" with respect to the national economy, and (2) his willingness to make compromises (or deals) as a means to attaining a portion of his desired ends.

Specifically, Tugwell believes that the new President missed a golden opportunity in 1933, when laissez-faire capitalism appeared to be on the skids. FDR should have responded then to the "technological imperatives" and should have sought "to recast the economy and redefine the objectives of our people" (p. 120). But the President had no plan ready, and in later years he could no longer bend the legislators to his will. In consequence, writes the author, "not much would ever be done" (p. 130). Tugwell's appraisal, evidently unchanged to the present day, discounts the important changes brought to America by the New Deal. It also rests upon a gratuitous presumption about what is good for "our people" and who has the right to make fundamental choices for them.

Tugwell, in my judgment, fails also in his criticism of the "compromising" Roosevelt (essay title, p. 256). Specifically, he objects to the president's concessions to powerful industrialists when the struggle with the Axis powers (after 1940) required that the nation become the "arsenal of democracy." Indeed, many liberals were unhappy about the price paid for this and other "must" programs of the President. But what real *alternatives* were open to Roosevelt? Tugwell suggests none in this instance, yet indulges in a dubious bit of deprecating.

Roosevelt was, as Tugwell's observations repeatedly confirm, essentially a conservative. He stood for greater "social justice," a "balanced" economy, and expanded government responsibility for the general welfare. It is not surprising that he stopped short of the kind of collectivism preferred by Tugwell, even though he was willing, on occasion, to think about it. Roosevelt had a powerful sense of his personal destiny and a conviction that he should seek and use public authority to "do good"—within a perceived context. And in spite of Tugwell's lingering disappointment with his political hero, he does accept FDR on those terms. The results achieved by Roosevelt, concludes Tugwell, "were less than they might have been but still more than Americans ought to have expected from a practitioner of their kind of politics" (p. 278). I would favor a more generous assessment than that of Roosevelt's work, but Tugwell's evaluation will serve as a modest (and unassailable) tribute.

THOMAS H. GREER

Michigan State University

Education and State Politics. By Michael D. Usdan, David W. Minar, and Emanuel Hurwitz, Jr. (New York: Teachers College Press, Columbia University, 1969, 1971. Pp. 190. $7.50.)

This book focuses on the relationships between primary-secondary and higher education in American states. Its thesis is that, although most people perceive these two levels of the U.S. educational system as completely separate, rather intense conflict between them is brewing in many states. The major sources of trouble are greater competition for scarce revenues due to rapidly increasing costs and a fight for stewardship of schools which handle the 13th and 14th grades.

Data are drawn from a 12-state survey which emphasized personal interviews with state legislators, executive branch officials, and educational policymakers; no indication is given of the length of the interviews or the number conducted in each state. The authors also utilize official reports, government statistical analyses, and similar documents. Nearly 80 per cent of the book consists of loose-knit descriptions of each state's socioeconomic-political "environment," "political institutions and processes," and "critical issues" (pp. 5–6). Approximately thirteen pages are devoted to each state.

It would be difficult to question the book's thesis: Increased demand for and decreasing supply of budgetary dollars is likely to intensify conflict among all state agencies, and discord over control of 13th and 14th grade schools has been widely observed. But we learn little about these phenomena from this extensive survey except that in some states conflict is overt, while in others it has yet to surface, although the authors assure us that conflict exists or is developing in all states. Thus, the thesis is based more on the authors' preconceptions than on any data.

It is just as well that the authors do not rely heavily on their data, since they are frequently inaccurate. For example, the reader is told that each of Indiana's four universities ". . . relates to the legislature in a quite independent way" (p. 68). I worked as the Purdue lobbyist's graduate assistant in 1966 and 1967 (years covered by this study), and I was given access to virtually all university and legislative meetings concerning higher education. Approximately six months prior to the 1967 legislative session, representatives of the four universities assembled in a series of meetings in which they decided each institution's percentage of capital and operating budgets and the total dollar figures which would be requested from the legislature. Then they presented a united front in support of their agreements. They had been following this procedure for more than a decade because it was prescribed by law.

Furthermore, the first sentence in the Indiana section reports that ". . . little is happening in the state" (p. 65). This was written after the authors had supposedly interviewed participants in what could easily be described as a revolutionary legislative session from the point of view of higher education.

The treatment of Illinois is only a slight improvement over the Indiana portrait, but it is peppered with sentences such as: "The pattern of education decision-making prior to the 1967 session of the legislature was predetermined and unalterable," and "The realities of the present political scene indicate that any change in state-level administrative structure is years away" (pp. 55, 57).

This volume is rife with unsupported and vague speculation such as the notion that rapidly increasing educational costs and other forces are encouraging a widespread revision of outlooks concerning the sharp separation between primary-secondary and higher education. The precise nature of this revision is not specified nor is evidence presented that anyone other than the authors are seriously questioning this separation.

Sometimes even empirically weak studies perform a service by assembling the literature in a field. Unfortunately, this work contains no bibliography and a total of only four citations (all of these are statistics from U.S. Government documents). It does not seem possible that anyone could even approach the topic of state level higher education politics without referring to Lyman Glenny's classic *Autonomy of Public Colleges: The Challenge of Coordination,* but the authors have done it. Difficult as it is to believe, they apparently have not read Glenny; if they had, they would not have written that state level coordinating and governing boards ". . . exist in only a few states, and where they do exist they are often new, underdeveloped and untested" (p. 173).

In searching for some redeeming qualities in this work one looks to the possibility that a novel theoretical framework has been created, but only confusion and methodological naivete are revealed. For reasons which are not entirely clear, the authors decide that "cooperation" and "conflict" will serve as useful "summary concepts" by which relationships among educational levels can be described (p. 164). Neither term is defined, however, and few hints are given concerning detection of these qualities. The term "interest" is also heavily utilized with similar disregard for even the most elementary standards of scholarship (p. 167).

CARL GRAFTON

University of Houston

The Political Economy of Public Organizations: A Critique and Approach to the Study of Public Administration. By Gary L. Wamsley and Mayer N. Zald. (Lexington, Mass.: D. C. Heath and Company, 1973. Pp. 111. $10.00.)

For over two decades students of public administration have struggled with the confusion and fragmentation of a field without a widely accepted paradigm. The paradigm problem has plagued public administration leaving it without common boundaries, a common research tradition, or even a widely accepted set of concepts. Wamsley and Zald have confronted the paradigm problem in public administration by proposing a framework that links the field's traditional concerns with politics and management to organizational sociology. In this short book, the authors only briefly present their critique of public administration's post World War II "drift" and sketch the basic features of their approach. Nevertheless, their critique is penetrating and their framework is sufficiently suggestive to make this book valuable reading for students of public organizations.

Wamsley and Zald argue that the development of empirical theories of public administration has been frustrated by a boundary problem indicating a lack of coherent focus for the subject. Public ownership produces unique problems for public organizations—problems of political legitimacy, nonmarket financial transactions, client demands, multiple performance standards, and involvement in public policy formation and implementation. These distinctive characteristics provide the theoretical foundation for the separate study of public organizations, for distinguishing between classes of public organizations, and for isolating those factors that promote change and stability in the structures and processes of public agencies.

Most of the book is devoted to explicating a framework that includes a variety of political, economic, and sociological concepts. The authors define the political economy of public organizations as "the interrelation between a political system (a structure of rule) and an economy (a system for producing and exchanging goods and services)" (p. 17). Their framework has four major components: external economies, internal economies, external politics, and internal politics. Throughout the book the authors point out the interrelationships and interfaces between properties of the four major components—explaining, for example, how external economic forces affect internal political relations, or how internal economies shape external political behavior.

Readers familiar with the open systems school of organizational analysis will recognize the influence of Thompson, Selznick, Perrow, and Cyert and March on Wamsley and Zald's framework. Regrettably absent or underemphasized are the concerns and findings of public choice theorists, decision theorists, and social psychologists. But, even though their framework is not exhaustive, the authors persuasively demonstrate how the findings of other scholars can be integrated by their framework and provide interesting, illustrative examples.

The book's intriguing treatment of interorganizational political and economic relations and exchanges combines power and policy analysis to explore linkages between agency support, goals, policy arenas, and agency behavior in a way that considerably extends previous efforts to explain interorganizational relations through power/dependency analysis. The discussion of external economic environments is this book's most original contribution. While business analysts have traditionally studied the industry and market structure of private firms, students of public administration have tended to overlook the market and industry characteristics of public agencies which must compete with one another for jurisdiction, public support, and appropriations. We know little about how these competitive factors affect policy, internal politics, interorganizational, and intergovernmental relations; and Wamsley and Zald provide brief but valuable insights into what some of these relationships might be.

The authors are extremely critical of classical and neoclassical treatments of internal politics and economies. They argue that public administration theory has not recognized variations in internal politics; has mainly focused on increasing efficiency only in situations where goals and means are clearly stated; has ignored internal conflict and "the mechanisms, such as succession processes and choices, by which new directions are set or imposed" (p. 57). To correct these deficiencies and account for the importance of intraorganizational politics, the Wamsley-Zald framework includes a number of political variables as principal components: the formal mandated structure or constitution; consensus building and conflict resolving mechanisms; authority structures; and patterns of leadership recruitment, socialization, and succession.

The framework's treatment of internal economic components reflects the authors' sociological bias. Wamsley and Zald recognize that the technical core of organizations—the structures and processes most directly related to the accomplishment of the organization's tasks—has been the major concern of classical and neoclassical public administration theory, but they devote only cursory attention to its major properties: buffering activities, incentive systems, technologies, resource allocation, and patterns of differentiation, specialization, and grouping. They identify these factors as economic; but they treat them as sociological, virtually ignoring the nonsociological dimensions of economic and technical factors like financial flows, automation, and productivity in organizational decision making. Also ignored are social psychologists' concerns with leadership styles, work group climates, morale, and job satisfaction and their relationships to productivity. These properties of organizations have both political and economic implications for public agencies and have properly drawn the attention of many students of public administration.

The Wamsley-Zald framework is suggestive but not exhaustive—a sociological sketch rather than a well-detailed multidisciplinary framework. Nevertheless, this book is a valuable first step toward focusing research and integrating findings about public organizations. At the very least it will make a real contribution to the field if it stimulates debate on the usefulness of sociological approaches to the study of public administration.

CHARLES H. LEVINE

Syracuse University

American Roulette: The History and Dilemma of the Vice Presidency. Revised Edition. By Donald Young. (New York: Holt, Rinehart, and Winston, 1972. Pp. xiv, 433. $7.95.)

> ". . . it's a kind iv a disgrace. Its like writin' anonymous letters" (Mr. Dooley, quoted on p. 372).

Most of us get a good laugh from our students with comments about the vice presidency like Dooley's. Yet we also know the sorry facts that eight presidents have died in office, five have been the subject of serious assassination attempts, and several, such as Wilson and Eisenhower, have been so ill that the functioning of government was seriously impaired (p. 6). In addition, current talk of Gerald Ford becoming the Republican presidential candidate in 1976 reminds us of the possibility that a vice president may become a leading contender for succession. Taken in this light, Donald Young's popular history of the office is a welcome addition to the literature.

A review of a revised edition may seem strange to the reader of this journal, but surprisingly enough no major journal gave notice of the original edition. Therefore, I shall treat the work as a new publication, referring to the earlier edition only in terms of the insight and analysis the author might have brought to his conclusions.

Donald Young, a senior editor for American History and Political Science for the Encyclopedia Americana, presents us with a well-researched and readable account of the historical development of the vice presidency. A popular history, as this may best be described, may still interest serious students even though copious footnoting and a discussion of historiography are missing. In their stead the reader is given a flavor of the men and their times; a book full of anecdotes and occasional insight into the workings of the American system. We see, for example, the beginning of the convention system as a result of Jackson's need to show national support for his choice of a running mate: Martin Van Buren. Indeed, to make the "nomination look as impressive as possible" (p. 37), the Democrats adopted the two-thirds rule. Or, in another section, we note the incredible instance of a man indicted for murder chairing the U.S. Senate, as Aaron Burr did in 1805.

Young's factual foundation may be quite good, but at times he slips into a historical determinism as unnecessary as it is inaccurate. For example, we are told (p. 31) that during a confrontation between Andrew Jackson and John Calhoun in 1830, the President "fired the first shot—only a verbal blast to be sure—that would echo down the years amid developments that would lead inexorably to another confrontation at Appomattox thirty-five years later." And later (p. 341) we are told that Nixon's choice of Agnew for vice president "was plausible and indeed almost inevitable." Moreover, the author's treatment of our recent vice president represents thinly veiled disgust, a style and tone totally out of keeping with the rest of the book.

Perhaps the major emphasis of the entire book is on the inadequacies of our procedures for presidential succession. Here, however, one must compare the first edition with the second. Young's entire concern is a technical one—how to tinker with the Constitution—but this is made largely irrelevant by the 25th Amendment, and furthermore it is not the real problem. How could a reading of the statute have predicted Tyler's succession or have given us an inkling of the transition to Truman or Lyndon Johnson? Surely we must simply wait to see what changes, if any, the 25th Amendment might make. Young's first treatment predates this amendment. His second, however, comes seven years later and could address itself in less space to more pertinent questions about succession. David S. Broder, for example, focuses on just such a major concern—the isolation of the vice president—in less space than this review (*Washington Post*, June 19, 1973, p. A18). Mr. Young's revision is unfortunately limited mainly to changes in names, numbers and dates, while leaving his substantive interpretations intact, even though events have made these interpretations dated.

These criticisms notwithstanding, Young's treatment is still a useful source of historical information and an interesting and readable rendition of an all too often neglected area of American government.

ROBERT S. ROSS
California State University, Chico

A Short History of the European Working Class. By Wolfgang Abendroth. Translated by Nicholas Jacobs, Brian Trench, and Joris de Bres. (New York and London: Monthly Review Press, 1972. Pp. 204. $7.50.)

Originally published in West Germany in 1965, this English edition has sported a postscript carrying the narrative as far as 1971. Professor Abendroth's ultimate purpose is to analyze the current state of the labor movement in Western Europe, and to assess the prospects for further advances. His "short outline" is a substitute for that reconstruction of "the working-class movement in its historical totality" (foreword) which he would like to see written. Five or six phases are distinguished: up to 1848; 1848 to World War I (or 1848 to the 1870s, the decline and fall of the First International; then from the Second International to World War I); the interwar years; the postwar period to 1965; 1965–71. The narrative encompasses about a dozen nations but also the international connections.

Judged in its own right, the history is interesting, especially coming from someone who, as a member of the underground in Nazi Germany, played some part in its making. But its compression into a "short outline" inevitably induces some distortion. One cannot simply say of the Swedish Democrats even in 1934 that they had been "in control of the government from 1920 . . . almost without interruption" (p. 113). Nor that, after the 1932 elections in France, various disenchanted elements "formed fascist groups," such as the Camelots du Roi, Action Française, etc. . . . (p. 98). By then those two already had quite a history behind them; there is also the debatable question (*vide* M. René Rémond) whether they should count as "fascist." Curiously, the organizations some think more worthy of the label are not mentioned, e.g., Doriot's name crops up as an expelled communist but not as the founder of the P.P.F. If, in contrasting the sociology of Marxism and of anarchism, with special reference to Spain, one relates anarchism to agrarianism/ruralism (pp. 33 and 46), one had better state for what period. By the early 1930s, i.e., just before Franco, the anarcho-syndicalist C.N.T. was more Catalonian (etc.) worker than Andalusian peasant. The other side of Andalusian anarchism as a secular form of Christian millennarianism also deserves mention.

More serious in a way, since it reveals the limitations of a still-too-rigid conceptual framework, is the explanation of the repeal of the English Combination Acts in 1824 in terms of "contradictions within the ruling political and economic groups" (p. 15). So much for Francis Place, the tailor, and Joseph Hume, the Radical M.P., in one of the cleverest lobbying operations of any time or place. Similarly, the attainment of the Ten Hour Day in 1847 is attributed to the Chartists alone (pp. 17 and 28). But the Ten Hours Movement was launched and sustained by Tories/Anglicans in and out of Parliament, though of course working with the operatives in their Short-Time Committees. This is, in fact, *the* famous case of Tory-Radical cooperation in England.

The book may be judged finally by the light it casts on the current state of the labor movement in Western Europe, which to Abendroth "appears to be paralysed within the self-imposed limitations of trade union conformism and to have adopted the ideology of the ruling class" (foreword). The drift of the narrative is that prosperity and the fragmentation of the movement are the principal variables accounting for both past and present disappointments. Since that fragmentation was largely the consequence of Comintern policy, the Cold War, etc., as well as of local reaction to the "periodic barbaric relapses of the Soviet bureaucratic dictatorship" (p. 154), the prospects for unity or at least a working united front, Abendroth implies, are now brighter. On the other hand, he does not analyze the prosperity/*embourgeoisement* issue, so his belief that the working class will resume its "historic mission" (p. 156) to liberate itself and others is left unargued. Nor does he even raise the worker-into-citizen theme à la Bendix and T. H. Marshall.

There are some factual errors and many "typos," e.g., Doherty (p. 15), Lend-Lease, and the date was March 11, not March 17 (p. 127), Defferre (p. 149), Olof Palme (p. 167). Still, it is useful to have in one's hands a conspectus of European working-class development that can be read at a sitting. It could be used for graduate courses in history, political science and sociology, subject to the suggested reservations.

GRAHAM WOOTTON

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Men Versus Systems: Agriculture in the USSR, Poland, and Czechoslovakia. By Arthur E. Adams and Jan S. Adams. (New York: The Free Press, 1971. Pp. 327. $12.50.)

This book will be of value to students enrolled in courses devoted to the comparative study of communist regimes. The authors, who teach at Ohio State University, visited the Soviet Union, Poland and Czechoslovakia in mid-1967. They talked with scholars, managers, specialists, and peasants, and inspected farms. Answers were sought to these questions: How does communist agriculture operate, and how well? What effects has collectivization or socialization of agriculture had upon the traditions, social systems and lives of those who work on the farms? How do these people carry on their work, and how do they view the changes they are living through? Later, the findings were collated with data for a number of Western countries and further evaluated in the light of published studies from the East European countries concerned.

The result is a series of vivid comparative descriptions of three systems-in-transition. A reader is persuaded that in each of the three nations, "Communist agriculture" works quite differently and that it is misleading to treat them as if they were identical copies of one model. The work leaves no doubt that several national Communist types of farming are developing to fit unique national circumstances. At the same time, elements that are common to all Communist systems are clearly identified, particularly the very large influence of the state over the agricultural decisions and hence the lives of the peasants.

Aside from helping students, this volume should be instructive for the specialists in Soviet affairs who lately have restated uncritically the official claims about a new drive toward social equality in the USSR. It usefully reminds us how Soviet data on the incomes of collective farmers are often deliberately misleading and how statistical guile is used to hide both the low payments of collective farms for work in the socialized sector and the relatively high proportion of the total income derived from private plots. Moreover, data are lacking about the proportions of cash and kind included in the collective farmer's income, about the cash or market value of produce paid in kind, and about profits gained from the private plots. When it comes to assessing the really important factors in the life of the Soviet citizen, every analyst is indeed compelled to be a "Kremlinologist."

A notable weak spot is that only one page is devoted to describing the vastly significant absorption of many collective farms by state farms in the USSR during Khrushchev's reign. The quiet but persistent mergers led to the transfer of millions of collective farm peasants to the status of wage-earning workers on the state farms. The authors tersely explain the change as one "in keeping with Marxist faith in the virtues of industrial management techniques applied to agriculture" (p. 23). Yet, soon after his advent to power, Brezhnev came out against the policy of massive conversions of collective farms into state farms. He said that many of the new state farms were formed hastily and turned out to be cumbersome, unmanageable, and unprofitable. This hardly indicates that Khrushchev's team was more Marxist than Brezhnev's, but it does suggest that the latter was more concerned about the great financial outlays and rural unemployment problem entailed by the farm conversions. The Khrushchev leadership, on the other hand, was obviously more impressed by the higher labor-productivity figures reported by the state farm directors. The Kremlin dispute over this key policy issue seems to have been remarkably free of doctrinal hairsplitting. The gist of the matter would have engaged the authors' attention had they delved more carefully into the literature, e.g., V. K. Logvinenko's penetrating monograph, *Kolkhoznaia sobstvennost i voprosy ee razvitiia pri perekhode k kommunizmu* (Kiev: 1966).

The same failing shows up in the imputation that officials and scholars in the Soviet Union who study their national economy and advise the planners are habitually rigid and dogmatic. If the authors had read what has been written by Lisichkin, Kassirov, and Venzher they would have found these Soviet experts to be just as intelligent and daring as the Polish scholars who so captivated them. Unfortunately, the reader learns nothing about how these Soviet economists in 1965–68 were openly skeptical of the organizational-centralist assumptions underlying Brezhnev's farm policies; how the same people frankly expressed admiration for Western forms of creative initiative on the land; and how they urged that free trade supplant the regulation of pricing and marketing from on high. Nor does *Men versus Systems* shed light on the post-Khrushchev controversy about creating in the USSR an East European-style council of farm cooperatives which might bargain with the state over terms of sale of produce or veto improper directives of the Ministry of Agriculture. The linkage of agrarian thought and action in the Eastern bloc, for which documentary evidence exists, is barely touched upon in this survey.

Now that it has become trendy in some quarters to stress the similarities rather than differences between Communist and Western patterns of government, it might not have been a bad thing if students were told how the nature of the Soviet political system makes the farm expert much less influential than he is in the West. An example is the Kremlin's erratic policy toward development of the Nonblack-Soil Zone, where agronomists have seen good prospects for intensive farming. But after 1958 the farming techniques imposed by Khrushchev led to weed-infestation, lowering of yields, and soil erosion in Kazakhstan. The interplay of politics and climate on the Soviet agricultural scene, however, cannot be appreciated if one wrongly states that "it would be difficult to deny that the [Soviet] specialists exercise far-reaching influence at all levels" (p. 263).

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Mao Tse-Tung's Cultural Revolution. By Tai Sung An. (New York: Pegasus, 1972. Pp. 211. $6.95.)

Six years after it began, the Cultural Revolution in China continues to be a dominant topic of discussion and analysis in all writing on contemporary China. Professor An indicates by the title to this work that he sees the great movement as the special creation and work of Mao Tse-Tung himself. This is a view widely shared and perhaps now generally accepted. Professor An is not one who believes, however, that Mao gained his real objectives. The hostile group around Liu Shao-ch'i was driven from public life, but he regards this as a relatively minor victory, and on the wider aims of the Cultural Revolution, he would record a verdict of partial or complete failure. This assessment depends in part on what one thinks Mao really set out to do; if it was to remould the thinking of a vast nation, making every man and woman in it a dedicated servant of Maoist ideals, one needs to have been a very convinced optimist to think such an aim ever was realizable.' Mao himself seems to have set his sights rather lower; he stated more than once that new cultural revolutions would be needed every fifteen or twenty years, i.e., every new generation, to rekindle the sinking flames of enthusiasm.

It remains to be seen whether this prospect is really attractive to his colleagues and successors, or whether if it were, the people of China could be once more galvanized as they were during 1966. One of the main charges Professor An levels is that in fact the significant result of the movement was to bring the military to power, especially in the ordinary ranks of administration, and thus the power to control the gun passed from the Party to the Army. The fall of Lin Piao, one more unexpected turn in the complex maze of Chinese politics, throws more than a little doubt on this conclusion. Clearly the Army had its day of power just as the "ultra left" and the Red Guards had had theirs. But it is by no means clear that military influence has survived the traumatic shock of the fall of the closest comrade in arms, the chosen successor, especially as that downfall was not merely a matter of ideological error which can be corrected, but of outright treason and conspiracy to usurp power. On the other hand Mao survives all these changes and reversals. Neither the factionalism of the Red Guards, which might seem so discrediting to his movement, nor the expulsion of some of his closest collaborators in the earlier stage of the Cultural Revolution, nor the elimination of his proclaimed heir—none of this seems to harm the prestige of Mao Tse-tung himself. No one dares to suggest that any or all of these shifts were the consequences of misjudgements by Mao.

Professor An traces the history of the Cultural Revolution from its origin, which he finds in the Great Leap Forward and Commune program of 1958. "By and large, Mao Tse-tung represented the 'opposition' minority that stood up and 'rebelled' against the 'revisionist' majority in the Party establishment" (p. 17). In the Red Guard Stage which followed, Mao mobilized the Red Guards to carry out his political assault on the establishment because he could not trust the Party apparatus to purge itself. But, inevitably, the Red Guards split into factions: it became necessary to curb the excesses of "ultra leftists," and "this drive against 'ultra-leftism' was also undoubtedly aimed at the Red Guards and other Maoist 'rebels' who refused to end their factional violence and lawlessness and to submit themselves to discipline" (p. 40). Thereafter Professor An sees the Cultural Revolution in retreat, culminating in the Ninth Party Congress which officially brought it to a close. The author sees the Cultural Revolution as a "fiasco" (p. 126), and he holds that the "thought of Mao Tse-tung is now very much dead as a source of national unity and as a force for progress in modernization." These words were written more than two years ago; recent events, and some contemporary assessment by informed visitors to China, suggest that other interpretations are admissible.

Professor An has included most valuable appendices giving what can be known of the members of the main administrative and political organs and their recent political experiences. The book is described as a pilot project for a larger study; it serves that purpose well, and the wider study will be eagerly awaited.

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Politics and the Labour Movement in Chile. By Alan Angell. (London: The Royal Institute of International Affairs by Oxford University Press, 1972. Pp. 289. $17.00.)

A decade ago the highly complex Chilean political scene, despite its uniqueness in a region periodically convulsed by upheavals, was among the least cultivated vineyards of political research in Latin America. Chile had probably made more consistent and peaceful strides toward a politically strong and orderly society than any other Latin American nation, yet inexplicably virtually nothing had been written in English relating to Chilean government and politics. Not until 1966 was a comprehensive account of Chile's politics published. And yet, this country in the two years before the coup, seemingly as a result of the Christian Democrat's "Revolution in Liberty" of the

1960's and Salvador Allende's "Peaceful Road to Socialism," had become a favorite meeting place for social scientists who were practically bumping into each other on the streets of Santiago.

Professor Angell's book, however, is a welcome addition to a growing literature of uneven quality concerning Chilean politics. Very few studies cover the same ground. The author, a British political sociologist, offers a just and objective appraisal of the political role of organized labor in Chile. The Chilean labor movement is as unusual in the Latin American *milieu* as the political system within which it operates. First, it is not only numerous but also fairly representative, and second, it has never been controlled either by the state or by the employers. More important, the allegiance of labor is the object of intense competition among political parties with no one single party dominating the unions to the exclusion of all others.

Divided into two parts, the book first examines the historical development of the union movement emphasizing the continuities in its attitudes, aims, and political activities. In the following chapter, it offers useful statistics on the number and size of unions, their distribution by occupation, their structure, and their growth. These figures indicate that in Chile, contrary to what occurred in many Latin American countries, the union movement developed undramatically through a gradual and slow process of growth. The first part of the book concludes with a discussion of the effects of economic factors—such as the structure of the economy, the impact of the unions on economic life, the relations of the unions with employers and with the government—on the industrial relations system. The author makes clear that industrial relations in Chile are the product of the interactions of these economic factors and the all-embracing labor code enacted in the 1920s by President Arturo Alessandri. This code accounts, for example, for the unusual structure of Chilean organized labor—characterized by a large number of small unions and a small number of national federations—since it established distinctions between blue-collar workers (*obreros*) and their plant unions (*sindicatos industriales*) on the one hand, and skilled blue-collar and white-collar workers (*empleados*) and their craft or professional unions (*sindicatos profesionales*) on the other. One of the effects of such a distinction has been a deterrent to the development of powerful national labor federations.

Part II, divided into five chapters, examines the relations between political parties and the labor movement. Socialism and communism are treated jointly in two of these chapters. Two other chapters are devoted to the Radicals and the Christian Democrats. The last chapter is an assessment of the extent and significance of politicization in the union movement. Two appendices examine the development of rural unionism since the rural labor law of 1967 and the influence of external groups on the political attitudes of Chilean labor, respectively. Among the external groups the author selected those of the United States and of the Catholic international labor organizations as the most influential.

As Professor Angell persuasively argues,

> union activity in Chile could hardly fail to be political. Because the government plays such an important role in virtually all areas of industrial relations, and because at least the larger unions must be as active in Congress and in the ministerial departments as on the shop floor, industrial relations necessarily take on a political dimension. The question at issue is not so much whether unions are engaged in politics—for they obviously are—but the extent of their politicization (p. 210).

While recognizing the difficulties of generalizing, Professor Angell chooses his points well in attempting to answer this question. He does not dispute that Chilean union leaders are for the most part highly politicized but questions "how relevant their political beliefs are for their industrial action and how influential they are, in their capacity as unionist-politician, over the bulk of the rank and file" (p. 212). He reminds us that Chilean unions are not splintered according to ideology and that precisely their most salient feature is the ideological pluralism of their governing bodies. At the same time he does not ignore the key role that unions have played in mobilizing support for the Marxist parties and suggests that unions are highly influential in shaping the general values of their members. He considers substantial the contribution of unions to the election of Salvador Allende in 1970.

Based chiefly on extensive interviews with trade unionists and political leaders, the book, though generally sympathetic, gives an objective account of the Chilean labor movement. Despite the modesty of its explanations and conclusions, Professor Angell's work is a useful and interesting source for the study of trade union political action.

FEDERICO G. GIL

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Hitler and The Middle Sea. By Walter Ansel. (Durham, N.C.: Duke University Press, 1972. Pp. 514. $12.75.)

The Naval War Against Hitler. By Donald MacIntyre. (New York: Charles Scribner's Son's, 1971. Pp. 376. $10.00.)

A final and almost exclusively British triumph, the naval war against Hitler was vital to British survival. To Hitler the naval war remained peripheral and *Fall Barbarossa*—the conquest of European Russia—central. Donald MacIntyre, with that specialized perspective of the naval historian, displays full mastery of his subject. Attending only briefly to European land operations he begins his history with the "Rape" of Norway—misplaced hyperbole—but thereafter presents a thorough account of naval warfare around the circle of water from the Aegean Cyclades to Novaya Zemlya. While presenting very detailed orders of battle and operations, he succeeds in making naval war humanly real, to the extent this can be done, given its inherent depersonalization. His account of early U-boat campaigns, the evolution of the submarine wolf packs and their eventual containment, is superior to most histories of the period. The effects of ice and ice packs, vast arctic storms, snorkelled submarines, German air cover from Norway, convoy losses in the North Atlantic, are sketched clearly. The book is not soulless—not a *Kriegstagebuch*—a war diary of sequential, ordered violence. The death of the great submarine commander, Guenther Prien in U47—"Further depth-charge attacks shattered the U47's hull. A vivid flash and an explosion from the depths told of her end . . ." (p. 83)—exemplifies death in the silent services of all combatants. Similar quick, crisp imagery illuminates convoy massacres in the Arctic (pp. 209–230), and the destruction in surface combat of the German battle cruiser *Scharnhorst* off Norway's North Cape (pp. 335–345). Casualties on both sides were heavy. This, after all, was a five-year war in which more than 35,000 German submariners alone were lost. (As of April 1972 after eleven years of war in Viet Nam the American Army suffered 30,491 killed in action.) MacIntyre gives short shrift to the politics of sea war. He does make one particular note of Doenitz's self-created political bind through his early assurance to Hitler that U-boat war would suffice to strangle England. In the end, and to his chagrin, Doenitz was forced back on dwindling air and piecemeal commitment of surface ships as the submarines were pressed back against the coast of Europe (p. 335). After 1943 "it became rare for a U-boat to survive more than two patrols" (p. 329). Doenitz here becomes one of those dreary but high ranking military men who give insistent assurance to policy makers that a single, almost miraculous weapon will bring the enemy to defeat.

Walter Ansel, a professional military man, gives considerable attention to the political as well as strategic circumstances surrounding German sea and land campaigns in the *Mittelmeer*. He places curious, but not too convincing, stress on Hitler's psychological fear of the sea (p. 31), while, more convincingly, building the case that the core of Hitler's longrange thinking was dominated by *Fall Barbarossa*. Ansel sketches fruitless German plans aimed at invasion of Britain, the reduction of Gibraltar and an air-landing assault on Malta. But like most scholars he agrees that for decades Hitler saw Germany's final goals as in the East, not the West. Through this first part of the book Ansel addresses the politics built into any large military apparatus, politics which involve control of expanding assets, new weapons, and above all promotion and prestige. Most of the treatment focuses on the Balkan campaign with the far greater share allotted to the airborne assault and conquest of Crete; the latter remains uniquely bloody for airborne and uniquely successful. Ansel treats the Balkan campaigns and Crete as flank security operations for *Barbarossa*. And although his strategic summaries are clear he quite excels MacIntyre's mild and rare military hyperbole; thus, Crete is conquered by "a drive of Teuton air kings" (p. 5), (more an assault by infinitely disciplined, trained, dedicated and well led troops). *Barbarossa* becomes "a great javelin of men and machines (that) would plunge deep into the vitals of Russia" (p. 61). (In fact much of the German strength was directed into three major drives along a 1200-kilometer front.)

But these points are cavils. Ansel's style is fast-paced, personal, and engaging. Above all he knows how to bring war close to the mind's eye by detailed vignettes of small-unit battle. Ansel personalizes his war in a way MacIntyre cannot personalize his. While the destruction of HMS *Hood* or the *Bismarck* is impersonal, ground infantry action is a most human affair. Action is sometimes blindingly quick, convulsive, and deadly violent. Ground combat detailed by Ansel is conducted by men with names, associated with settings given strong relief—a dusty mountain road or a grove of trees, a half smashed village. One action set in far Western Crete recounts a battle between British and German units, the former commanded by generals remote from action who lose control, the latter commanded by generals and other officers in close and constant contact with their men and the enemy. Names, places, events and times are given in sufficient but not confusing detail, along with lucid accounts of actions. The air moves, the dust from the guns rises, and you know who lives or dies (pp. 352–375). Ansel's use of direct quotes from field orders, war diaries (*Kriegstagebuecher*), and battle summaries adds to the immediacy of the text. He thus uses techniques found in *Pork Chop Hill, The Longest Day,* and *Enemy At the Gates,* as well as a style more conventional in the writing of military history.

Both MacIntyre and Ansel express an honest admiration for all brave soldiers, as well as professional military deference to a tough, disciplined, implacable, and skilled enemy (the Italians excepted)—one who at the level of close combat, reveals a high degree of soldierly honor. MacIntyre, with that peculiar British sense of grace and restraint does so indirectly. Ansel is more overt and ebullient, and he goes further in dividing the German war system into good soldiers misused in a bad cause, and German politicians playing at strategic fantasies (pp. 449–453).

Both books are worth the time and attention to events of great strategic scope and small but essential tactical detail required to grasp the enormous range of action in Europe in World War II. But what is here for the political scientist? Certainly a sense of history and the data of violent institutionalized conflict, which must be studied at all levels. For all the studies of human conflict in the discipline, few appear to come to grips in any detail with the enormous and formalized systems designed for violence, and fewer address the political sociology of men in battle. We understand even less why some military systems cohere under appalling conditions while still others disintegrate under far lower stress. How is it that within the structure of such vast impersonal forces, the personal concepts of honor, espirt, aggressiveness, and small unit pride serve as essential social bonds and without which war may be impossible? Works such as these are basic material for more theoretical studies. They provide the political scientist a basic sense of war at many levels and most importantly at the level of immediate battle, pointing to the need for detailed and continuing studies of social man in protracted warfare.

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The Irish Judiciary. By Paul C. Bartholomew. (Notre Dame, Indiana: University of Notre Dame Press, 1972. Pp. xi, 86. $3.95, paper.)

The Irish Political Elite. By Al Cohan. (Dublin: Gill and Macmillan, 1972. Studies in Irish Political Culture, No. 4. Pp. 76. £1.00, paper.)

Chairman or Chief? The Role of Taoiseach in Irish Government. By Brian Farrell. (Dublin: Gill and Macmillan, 1971. Studies in Irish Political Culture, No. 1. Pp. xiii, 110. £1.00, paper.)

The Founding of Dáil Éireann: Parliament and Nation-Building. By Brian Farrell. (Dublin: Gill and Macmillan, 1971. Studies in Irish Political Culture, No. 2. Pp. xx, 89. £1.00, paper.)

Irish Political Parties: An Introduction. By Maurice Manning. (Dublin: Gill and Macmillan, 1972. Studies in Irish Political Culture, No. 3. Pp. 120. £1.00, paper.)

These five short books provide useful additions to the growing literature on the politics of the Republic of Ireland. Paul C. Bartholomew's *The Irish Judiciary* is the fruit of study-leave spent in Ireland by an American authority on constitutional law. The book contains three chapters, on Irish courts, Irish judges, and Irish law. The chapters on courts and law are straightforward descriptions, containing no surprises, but useful for an outsider wanting a quick general view. The most original chapter is the one on the judges. Based on interviews with a large sample of the Irish judiciary, it provides an interesting inventory of their social characteristics. Bartholomew finds that they are a highly unrepresentative sample of the Irish population. A majority come from upper-middle-class backgrounds, and more than a quarter are sons of lawyers. Politics played a major part in their promotion. Nearly three-quarters were at the time of their appointment members of Fianna Fáil, which has been the ruling party for thirty-five of the past forty-two years. Less than ten per cent claimed to have had no political affiliations at the time of appointment. Bartholomew does not speculate on the consequences of drawing the judiciary from so unrepresentative a sample.

The remaining four books all belong to a new series, *Studies in Irish Political Culture,* published by Gill and Macmillan of Dublin, under the editorship of Mr. Brian Farrell of University College, Dublin. The first two contributions are by Farrell himself. *Chairman or Chief?* is an examination of the role of Taoiseach (i.e., prime minister) in the Irish political system. Farrell has supplemented the meager published evidence by extensive interviewing, and is able to offer a much fuller account of Irish prime ministers than any previously available. There is a revisionist element in his treatment of most of the five men who had held the office up to the time of publication. He stresses the leadership qualities of those prime ministers who have seemed less glamorous to the public—Cosgrave (1922–32), Costello (1948–51 and 1954–7), and Lynch (1966–73). He emphasizes the limitations of de Valera, who held office for longer than any other Irish prime minister (1932–48, 1951–4, 1957–9), and who made the deepest impression on the public. He treats Seán Lemass (1959–66) as being the strongest of the Irish prime ministers. The limitation of Farrell's work is that it is impossible fully to appraise one institution in a political system without appraising all the others, and investigations of other political institutions in Ireland have scarcely begun. We still know nothing about the power structure of political parties, about patterns of influence in Parliament, about the influence of the civil service and of most interest groups. But one has to start somewhere, and Farrell has started well.

Farrell's other book, *The Founding of Dáil Éireann,* offers a controversial reinterpretation of the revolutionary period 1916–22. He argues "that the Irish struggle for independence represents a deviant case in the usual record of twentieth-century insurrection and internal war; that a developed political culture was already established prior to independence; that there was no revolution involved in the creation of the new state" (p. vii). Later (p. 83) he explains more fully what he means by this "developed political culture." He finds in Ireland

> an acceptance of the ballot as a source of legitimate authority; an expectation of incremental rather than radical change or improvement; a consciousness that leadership must be representative as well as efficient. . . . There is an emphasis of localism. . . . There is a stress placed on personal—even familial—contact whether in relation to the political elite or to administrative procedures which is scarcely possible in a larger society. There is an innate conservatism. . . . There is, above all, sufficient acceptance of things as they are—"the politics of a contented people"—to stem any demand for revolutionary change (p. 83).

All these features of modern Irish culture were, he believes, well established by 1916. Farrell's thesis seems to the present reviewer both true and important; the unrevolutionary nature of the Irish revolution was a point that needed stressing.

Maurice Manning's *Irish Political Parties* provides a brief historical account of the main political parties in the Republic. The book is almost pure narrative; it contains no information on the internal structure of parties, and little on their sociological bases. It is not based on original research, but simply summarizes the existing secondary literature—though even here, there are gaps in the author's coverage. So far as it goes, however, the book is a useful and reliable introduction to the subject.

Al Cohan's *The Irish Political Elite* is derived from a survey of 130 politicians and 64 senior civil servants who have held office over the past fifty years. His purpose is to examine how far the elite of the present generation differs from the elite of the revolutionary period. He has discovered some contrasts. The revolutionary elite were more likely to be what he describes as "marginal men," by which he means that they were more likely than the present elite to have been born outside the area of the present republic or in its border areas. Their career pattern was also different: they were likely, after a preparation in movements of cultural nationalism, to have been catapulted to prominence in the political movement, while the present elite is more likely to have worked its way up, by a recognized career pattern, through service in local government and then in parliament, to ministerial office. But Cohan's main conclusion is that the similarities between the two elites are much more marked than their differences. This conclusion will surprise no one with a knowledge of Irish politics; but Cohan has given us a carefully worked out, if undramatic, contribution to knowledge.

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The Other Israel: The Radical Case Against Zionism. Edited by Arie Bober. (Garden City, N.Y.: Doubleday, 1972. Pp. 264. $2.50, paper.)

This is a collection of essays by members of Matzpen, the Israel Socialist Organization, a New Left anti-Zionist splinter group, which condemns Zionism as a species of western colonialism, working in collusion with capitalist imperialism. In return for a free hand in creating a racially exclusive society based on the displacement of the Arabs, the authors tell us, Zionism serves imperialism as a watchdog for the repression of anti-imperialist forces within the Middle East. The solution proposed is the de-Zionization of Israel and the integration of the Hebrew nation within a socialist federation that will arise after a successful revolution throughout the region. Thus, both orginal sin and the pact with the devil will be annulled by the messianic deliverance. One wonders if the title, "The Other Israel," is meant in a theological, more than in a political, sense.

It would miss the mark to criticize the views expressed in this work as biased or one-sided. Rather they are the product of a constricting ideology that is peculiarly inappropriate to the subject. Economic determinism is perhaps the least relevant factor in Zionist policies or in the growth of the State of Israel. In this book it is wedded to a conspiratorial reading of history, designed to reduce to a patterned simplicity the complex reality of events.

The first section, entitled "Israel in a Historical Perspective," develops the twin themes of colonialism and imperialism. The "specific feature of Zionism which distinguishes it from all other modern colonization movements" is that the indigenous population was "not to be exploited, but totally replaced" (p. 38). After the development of the Zionist infrastructure came the demand for political independence, its impetus derived—so we are told—from the desire to preserve the gains of wartime industrial expansion. (In the authors' view, the pressure of Jewish survivors in displaced persons camps against the immigration ban of the mandatory government only supplied a convenient political atmosphere.) The outcome of the 1948 war is explained, in total disregard of actual events, by secret deals between the Zionists and the Jordanians, which determined the future spheres of influence of American and British imperialism in the area. Israeli retaliatory raids in the early 1950s had the secret political motive "of forcing the Arab regimes to join the anti-Soviet Bagdad Pact, which was part of the American cold-war containment policy" (p. 61). The authors' explanation of the genesis of the Six Days' War is particularly enlightening with regard to the general orientation of the Matzpen group. The blockading of the Straits of Tiran created an intolerable situation for Israel, not because of any threat to its economy or security, but because "*for the first time* (emphasis in the original) since the beginning of the Zionist colonization of Palestine, Nasser broke the continuity of the chain of faits accomplis" (p. 84), in this case the fait accompli of freedom of navigation.

In the second section, "The Nature of Israel," we are confronted with the paradox that Israel has been "financed by imperialism without being economically exploited by it" (p. 94). This is taken as "proof" that Israel provides a mercenary counterrevolutionary army in the service of the imperialists. Even the relative lack of corruption accompanying the inflow of foreign capital becomes "proof" that the entire society is corrupt: everyone gets his share. The bankruptcy of the Left in Israel is demonstrated by its basic adherence to Zionism, the kibbutz is a communal exploiter of labor, and the Histadrut, the General Federation of Labor, places national interest above class interest. ("The internationalist approach to the class nature of society was never brought up at the Histadrut's founding congress" [p. 123].)

"A Critique of Zionist Ideology" concludes the discussion. Here we learn that the self-determination to be granted to the Hebrew nation after the successful socialist revolution has important limitations. Matzpen does not advocate the existence of a separate Jewish state. Its concept of self-determination "assumes the *overthrow* (emphasis in the original) of Zionism" (p. 180). While disagreeing with the emphasis Palestinian resistance groups place on the national struggle, rather than on the need for political and social revolution within the entire area, Matzpen recognizes the right and the duty of the Palestinians to carry on the struggle by any means they deem advisable. The test thus presented to the limits of political tolerance in Israel is likely to constitute the major source of interest generated by this book and its sponsors for the student of Israeli politics.

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The Protracted Game: A Wei-ch'i Interpretation of Maoist Revolutionary Strategy. By Scott A. Boorman. (New York: Oxford University Press, 1969. Pp. 234. $7.50, cloth; $2.50, paper.)

It is still a pleasure to review a good, innovative new book, even if in the last analysis one remains unconvinced by the argument. Author-mathematician Scott Boorman's impressive achievement is to draw an elaborate analogy between the Chinese strategic game of *wei-ch'i* (*go* in Japanese) on the one hand, and Mao Tse-tung's strategy for "people's war" on the other. The points of comparison are many, and the claimed relevance of the analogy seductive. Spaces along the side of the *wei-ch'i* board are more easily and more economically defended than central spaces, which seems to correspond to Mao's preference for armed base areas in border regions. The long duration of the game (200–300 moves on the average) parallels the protracted conflict characteristic of insurgencies. The jigsaw puzzle-type pattern of dispersed fronts common to the *wei-ch'i* "middle game" (roughly moves 50–170) also appears to describe the "intermediate stages" of a Maoist-style insurgency. And the placement of "stones" (the playing pieces in *wei-ch'i*)—first to influence or dominate certain areas of the board by virtue of the potentialities inherent in their location, and then later to surround and ultimately capture enemy stones—also bears more than surface resemblance to the Maoist choice of battlefield and limitation of the engagement; while falling short of immediate "victory," this strategy does advance insurgent influence in the total campaign, leading ultimately to the encirclement and annihilation of selected enemy forces. Altogether Boorman hypothesizes fully seventeen major points of analogy.

The first seed of doubt is sown by the author's failure to specify how his analogy should be applied theoretically. He says it provides "a useful tool for explaining the often paradoxical principles of Chinese Communist insurgent doctrine," a "model," a "key" and a "knothole through which to view the Maoist cow." But nowhere does he specify (or speculate) under what conditions the *wei-ch'i* analogy applies to the analysis of insurgency. Any "Maoist"-type insurgency? Only a Chinese one? Japan's armed forces, after all, despite an unsurpassed Japanese enthusiasm for the game, historically have behaved more like the legions of Prussia than like grand masters of *go*. Moreover, Michael Elliot-Bateman in his book *Defeat in the East* finds as many elements of "Maoism" in Clausewitz as he does in Sun Tzu. Interservice rivalries, Sandhurst traditions, and other debilitating factors, however, have effectively removed many of these ideas from standard British military training, a point which if true suggests an important hypothesis: Mao's military strategy itself was less uniquely a factor in the Chinese Communists' victories than was his political leadership, i.e., his capacity flexibly to implement sound doctrine appropriate to the circumstances he faced.

A second source of doubt is Boorman's general failure to contrast his analysis with any other published analysis of guerrilla warfare in China in order to highlight how his conclusions differ from or add to the findings of other writers on the subject. The following passage, unfortunately almost alone in the book, illustrates what can and should be done with such a new approach:

> Certainly erroneous, however, is Anthony Garavente's contention in *The China Quarterly*, 22 . . . that the Communist defeat in the fifth encirclement campaign against Kiangsi bases was due to an inherent inconsistency between territorial control and mobile warfare: the existence of *wei-ch'i* as a coherent conflict system embodying both motifs would seem adequate refutation.

The reader will find him or herself repeatedly asking if the author's hypotheses are not simply familiar arguments dressed up in a new conceptual vocabulary.

Third, I looked in vain for consideration of possible alternative sources of Chinese Communist strategy outside the game perspective altogether. Boorman fails to ask whether Mao selected his strategy in Kiangsi from alternatives according to *wei-ch'i*-related territorial imperatives, or whether he was forced up into the hills against his wishes. Would a non-*wei-ch'i* player have done the same as Mao faced with the same choices and in control of the same resources? Finally, although he frequently quotes Mao to suggest that the Chairman consciously thought in *wei-ch'i* terms, Boorman never deigns it necessary to ask if Mao might simply have been using folksy language attuned to his audience.

I enjoyed this work immensely, but my skepticism leads me to support the author's call for future research (Appendix 1) rather than to accept the argument as it stands at present.

GORDON A. BENNETT

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Russian Social Democracy; The Menshevik Movement: A Bibliography. By Anna Bourguina. (Stanford, Calif.: The Hoover Institution on War, Revolution and Peace, 1968. Pp. 391. $7.50.)

If the road to hell is paved with good intentions, it is safe to suggest that the Russian Mensheviks have done a good bit of the paving. Their illustrious names turn up consistently in Russian history, especially from 1900 to 1907, from 1910 to 1914 (abroad, during the crisis of "internationalism" caused by the "imperialist war") and, of course, as dominant figures, breaking polemical lances with Lenin and Trotsky in the soviets and in the Provisional Government of 1917. Always they stood for Marxism *cum* the democratic process, so that, whenever issues of political power were at stake they were unprepared, whether as ideologists or as personalities, to match the Bolsheviks with ruthless or militant action. With defeat, their movement disintegrated in exile, many Mensheviks completing their lives as academicians or journalists or, in old age, as sad chroniclers of their movement's demise.

This worthwhile listing of titles, many with brief content notations (in Russian) of hard to find and often pseudonym-disguised works by such as Martov, Axelrod, Dan, Martynov and Valentinov is bound to become an important aid to scholarship, especially since Moscow continued to pour out trash on the period 1890 to 1920, too much of which is filling up Russian collections in this country. Editor Bourguina deserves credit for producing this compilation.

STANLEY W. PAGE
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Parliament and Congress. By Kenneth Bradshaw and David Pring. (Austin: University of Texas Press, 1972. Pp. 426. $10.00.)

Readers of this *Review* will be familiar with drawings of those splendid 19th-century inventions that never quite came off: bicycles built for two hundred; pre-Wright Brothers flying machines, and railway carriages driven by compressed air that either would not move at all or else could not be stopped. This book resembles one of those inventions. It is well written and intelligent and deals with an important subject; yet one could not possibly recommend that anybody read it—or even take it seriously.

This seeming paradox is actually not difficult to resolve. Bradshaw and Pring are two Clerks of the British House of Commons who have spent a good deal of time in Washington talking to their opposite numbers in both Houses of Congress. What they do in *Parliament and Congress* is compare the two legislatures: House by House (the Senate = the House of Lords, the House of Representatives = the House of Commons); committee by committee (the House Appropriations Committee = the House of Commons Expenditure Committee); and even up to a point function by function (there are chapters on leadership, legislation, finance, and scrutiny and control of the executive).

Students of comparative politics can guess what has gone wrong. The authors are occasionally innocent of political realities underlying institutional forms. Their descriptions tend to be wholly formal. Things not comparable are sometimes compared. Conversely, things that are comparable are usually not compared if they happen to occur inside the legislature in one country but somewhere outside it in the other. To take one minor example, the authors discuss at length the House of Commons Question Hour, including the Prime Minister's Questions, but hardly mention the U.S. presidential press conference—simply because "it is not a congressional occasion" (p. 371).

Strangely, the authors, who are no fools, clearly recognize that there is something a bit unreal about all this. "To preserve the symmetry of comparison . . ." they begin one section (p. 37), but then make it clear that they are well aware that the comparison they are making at that point—between the two lower Houses' Speakers—is almost completely asymmetric. Of the Senate and the House of Lords they say, "On the face of it, the differences between these two assemblies seem so great as to make any comparison far-fetched" (p. 168); yet, having given good reasons why the comparison is indeed far-fetched, they nevertheless proceed to make it. It is as though they knew their flying machine would not fly but were absolutely determined to build it all the same.

The book does contain all sorts of useful tidbits of information: about legislators' salaries, about the length of legislative sessions, about how conflict of interest is handled (formally) on both sides of the Atlantic, and so on. And at this level the detail is undoubtedly right; the legislative specialist will almost certainly find himself wanting to turn up *Parliament and Congress* for reference. Unfortunately, once the authors move beyond "information" in the very strict sense, they become somewhat unreliable. It will not do English readers any good to be told that "in power and status [the U.S. Speaker] is probably exceeded only by the President and the Chief Justice" (p. 56). It will not do American readers any good to be told, for instance, that British "green papers" simply set out "the facts on which a future policy must be propounded" (p. 359), whereas in fact green papers are almost invariably preliminary—and sometimes not so preliminary—statements of what governments actually propose to do.

No one will put down this book feeling any lack of respect for its (in many ways) learned authors. But everyone, except possibly officials of Congress, will put it down feeling that Bradshaw and Pring could have spent their time better. It is sad to have to say it, but *Parliament and Congress* is really not much more than a curiosity, an exhibit for some out-of-the-way museum.

Anthony King

University of Essex, England

One Country or Two? Edited by R. M. Burns. (Montreal: McGill-Queen's University Press, 1971. Pp. 287. $7.50.)

Quebec—Only the Beginning: The Manifestoes of the Common Front. Edited by Daniel Drache. (Toronto: New Press, 1972. Pp. 279. $3.75, paper.)

Choose! By Pierre Vallières. (Toronto: New Press, 1972. Pp. 112. $5.95, paper.)

Concern about the possibility of Quebec's separating from Canada motivated John Deutsch, principal of Queen's University, to bring a number of academicians, almost all of them from his university, and former Premier of Ontario, Leslie M. Frost together. Urged on by Mr. Frost, the professors—historians, lawyers and political scientists—wrote the essays in *One Country or Two?* relying on material already available rather than busying themselves with new research because they felt that immediacy and relevance were over-riding considerations. The goal of the collection was to examine the basis of the Canadian political order, focusing on the federal system, the possibilities for ethnic accommodation, the probabilities of success and the consequences of failure.

Not surprisingly, the essays are uneven both in terms of quality and subjects covered. Those by T. H. B. Symons on Ontario, G. A. Rawlyk on the Maritimes, J. A. Archer on the Prairies, and R. M. Burns on British Columbia discuss these regions that would obviously be affected by separatism but only marginally deal with the major issues. The article by F. J. E. Jordan on the St. Lawrence Seaway is more a comment on Canadian-American relations in connection with that waterway over the last two decades than a discussion of the impact of separatism on the Seaway. And another by W. R. Lederman on the British Parliamentary system and Canadian federalism is so full of wishful thinking and loose generalization that it is virtually useless to those who wish to understand some of the issues involved in what is clearly a major crisis in the Canadian political system. For example, Lederman asserts at one point that "the Canadian parliamentary and cabinet system is more efficient than the American congressional and presidential system" (p. 19) and his essay seems to be little more than an attempt to make this case. One questions its inclusion in the collection.

There are four articles, however, that make this volume worthwhile:

R. L. Watts discusses the survival and disintegration of federations and attempts to place some of Canada's institutional difficulties in comparative perspective. His comment that Canada has reached a stage of "cumulative polarization of political cleavages" (p. 72) is probably the best analytic summation of the reasons for the separatist phenomenon.

Richard Simeon's "Scenarios for Separation" is a well-conceived and -executed set of "if . . . then" propositions that consider a variety of possibilities and the costs and benefits to English and French, elites and mass publics, should separatism come about. Unfortunately, the article is marred here and there by some questionable assumptions. For example, Simeon feels that the provincial governments would rally to the side of the federal government and consider it the logical spokesman and bargainer for English Canada. He also appears convinced that nonviolence is "such a strongly-held value" (p. 91) in Canada. Finally, the assumption that English Canada's retaliation is likely to be on economic lines should Quebec separate suggests a stereotyping that has been so much a feature of discussions of French-English relations in Canada. Inevitably, French Canadians are seen to be driven by concerns of the heart while English Canadians are motivated by hard-boiled economic considerations. If anything, the reverse may be the case. The entire debate in French Canada over the issue in the last five years is about the economic viability of the new state of Quebec. In contrast, sensitive English Canadians all across Canada seem disposed to ignore economics and to worry about the possible persistence of even the idea of Canada without Quebec.

J. R. Mallory's analysis of the future of English-speaking Quebeckers is a solid descriptive effort and John Meisel's relatively lengthy comment on what he considers Canada's options to be is an excellent review of French-English relations.

A full-scale study of separatism and its ramifications remains to be written. But anyone interested in the disintegration of political systems, Canadian politics or interethnic relations will find this book useful if only from the viewpoint of understanding some of the assumptions about their polity held by some of a country's leading spectator-elites.

Quebec—Only the Beginning is a collection of manifestoes put forward by three Quebec organizations: the Confederation of National Trade Unions, the Quebec Federation of Labor and the Quebec Teachers Corporation. Replete with Marxist interpretation and exhortation, they demonstrate how deeply discontent with the established order has permeated the ranks of some sectors of the working and lower-middle class in Quebec today.

Choose! by Pierre Vallières is a polemic by one of the leading philosophers of separatism in Quebec. It too is heavily Marxist as the author explains his feelings about the course of the separatist movement in the 1960s and his prescriptions for the future. Vallières here renounces violence as a viable political option for French Canadians and proclaims his support for the Parti Quebecois as the vehicle for the achievement of a separate Quebec. If this little book has any value beyond that of ideology, it is as another indicator that after the crisis of October 1970 and the murder of Quebec deputy premier Pierre Laporte by FLQ terrorists, even the most radical French Quebeckers will now devote themselves to focusing on the established political process to attain their ends.

PETER REGENSTREIF
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Land and Social Change in East Nepal: A Study of Hindu-Tribal Relations. By Lionel Caplan. (Berkeley and Los Angeles: University of California Press, 1970. Pp. xvi, 224. $6.00.)

With the rapid dissolution of the Empire, post-World War II British anthropologists (with a few notable exceptions) wrote more and more about less and less. In contrast, as the peculiar brand of American imperialism evolved, American anthropologists wrote less and less about more and more. Professor Caplan happily straddles the middle path; he is ethnographically substantive as well as suggestive of broader implications.

Caplan anchors his discussion on the land tenure system of the area. The Limbus institution of *kipat* ("an individual obtains rights to land by virtue of his membership in a series of 'nesting' kin groups"—p. 3) dominated long before the Hindu (mainly Brahman) migration began about 200 years ago. In 1964–65, when Caplan did his fieldwork, about one-fifth of the 511 members of the Indreni Cluster of four villages were non-Limbu.

At first, the author tells us, the Limbus welcomed the Hindu Brahmans and gave them land grants, for the population density was low, and enough land existed for all. Until 1886, the grant lands technically remained *kipat,* Limbu-owned land, but in that year, the Nepalese government decreed that all land granted to Hindus by Limbus would become *raikar* (freehold), so ownership shifted to the Hindus. The plight of the Limbus was accentuated in 1888, when the government decided that actual land titles had to be produced, or lands would be declared *raikar*. Fortunately, this regulation was never fully implemented in the zones of relative inaccessibility.

In addition, an economic interdependence linked the Hindu-tribal relationships. The Limbus needed money to satisfy their many ritual obligations, and, as the population grew and food production remained constant, to purchase foodstuffs and other commodities. The Hindus had money to lend. With the consent of kinsmen involved, a Limbu farmer could mortgage his land, and the mortgagee had farming rights to the land until the principal was paid. The actual owner (if he so desired) could work the land as tenant to the mortgagee. By 1965, 62.5 per cent of *kipat* land in the Indreni Cluster was mortgaged, 72 per cent of the total to non-Limbus.

Post–World War II development afforded additional economic opportunities to the Limbus, and many supplement their incomes through fishing, selling dairy products, bootlegging booze, working as seasonal or casual laborers in India, and portering lumber.

For the past 50 years, however, another major factor emerged in Limbuan: the enlistment of Limbus in the Gurkha units of the British and Indian armies. In 1965, 43.3 per cent of the Indreni Limbu males over 18 had served in an army, or about 11 per cent of the total adult population. One estimate states that about 60 per cent of a soldier's pay eventually returns to his home in Nepal. With accumulated pay and a steady pension, a returned Limbu could (and often did) "repossess lands pledged generations ago to Hindu creditors" (p. 5). He could (and often did) buy up mortgages held by Hindu Brahmans, and thus return the mortgaged land to Limbu control. These transactions naturally have not been favored by the controlling Hindu factions.

The wealth of the returned Gurkhas has created a new status situation, and caused tensions within Limbu cultural patterns. The new Notables (as Caplan calls them) want to maintain good relations with both the other Limbus and the Brahmans. Because of the caste system, they cannot intermarry with the Hindus, but seek out mates from other Gurkha (or equivalent) status Limbu families.

Although all Limbus apparently accept the dominant status of the Brahmans, they wish to maintain their cultural identity, and they resist all attempts to change any facet of the *kipat* system. They are defending not just their land tenure practices, but a way of life. In such dominant-subdominant contact situations, an important institution often becomes the symbol of resistance to the process of a smothering acculturation, and sometimes emanates an almost religious aura, but, in this case, not to the extent of the Cargo Cults or the Ghost Dance. Caplan believes this is because outlets for political expression and the means of satisfying increased economic expectations exist in the framework of the current Hindu-tribal confrontation. He further hypothesizes (probably correctly) that the contact between Hindu and local tribesman proved to be much less disruptive than contact between European and Asian in an imperialist situation, with its economic and political center thousands of miles away. Local exploitation (without outside interference) can possibly be successfully countered by local reactions within existing cultural patterns. Such a confrontation does not preclude violence, however.

We owe Caplan a debt of gratitude for rescuing these facts from Limbu.

Louis Dupree

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Readings in the Chinese Communist Cultural Revolution. Compiled and edited by Wen-shun Chi. (Berkeley and Los Angeles: University of California Press, 1971. Pp. x, 530. $12.00.)

The third in a series of readings edited by Wen-shun Chi, this impeccable volume presents twenty-one articles and documents published in China between November 1965 and November 1968. Designed for the advanced student, each Chinese text is accompanied by an introductory note in English and vocabulary lists concentrating on compounds, with clear indications of simplified characters. There are explanatory notes and an extensive apparatus of glossaries with romanization conversion tables and a reprint of the conversion tables for simplified characters officially approved in 1964.

No attempt is made to follow all the twists and turns of the Cultural Revolution, but the student who works through these readings will gain a clear impression of one very important aspect of Chinese politics—how factional struggles are officially presented and what the fighting is *supposed* to be about. Scurrilous or unreliable Red Guard material is not included; this is the official story starting from Yao Wen-yuan's article attacking Wu Han in *Wen-hui pao* and finishing with the Party Communique of October 31, 1968 which announced the resolution to expel Liu Shao-ch'i. How cultural, economic, educational and military issues are manipulated to "finger" key factional figures is a fascinating political drama, even if we remain unsure of the true structure of the factions.

Arthur Huck

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The Impact of Labour, 1920–1924. By Maurice Cowling. Cambridge Studies in the History and Theory of Politics. (New York: Cambridge University Press, 1971. Pp. ix, 570. $17.50.)

The Impact of Labour is an evocative title. Doubtless someone, some day, will write a book to match it. Meanwhile we have Mr. Cowling's volume. This is in fact a study of the "high politics" of 1920–1924, into which backbenchers, civil servants, pressure groups, the press, public opinion and, for that matter, issues intrude only insofar as they impinge on the maneuverings and calculations of the leaders of parties and factions. The political system, as Mr. Cowling sees it, "consisted of fifty or sixty politicians in conscious tension with one another whose accepted authority constituted political leadership" (pp. 3–4). He explains in terms as characteristic of his style as of his approach that "in this book we posit the existence of a number of plebiscitary demagogues whose chief way of understanding the reaction of other members of it and whose chief purpose was to jostle each other as they picked their way through the limitations imposed by all these forces to a position of creative sympathy with an unknown public" (pp. 11–12).

For all the questions so egregiously begged, one would welcome a study of leaders' behavior during a period of exceptional uncertainty as they contended with the disintegration of the Liberals and the rapid emergence of Labour. Aided by a substantial deployment of private papers, Mr. Cowling offers a detailed illumination of the politicians' response to the threats and opportunities arising from this transition. (Oddly, though, the Labour Party itself remains unfocused and offstage for most of the book, Lyman's work on the first Labour Government is ignored, and the ILP and TUC as such are mentioned only half a dozen times.) Mr. Cowling is at his best in tracing the downfall of the Lloyd George coalition, and if he adds little to the established outlines of this well-known story, he does give interesting detailed touches.

Nevertheless, the book is a sad disappointment. As narrative it is often uneven and confused. Edwin Montagu resigns on p. 161 but is dismissed on p. 163. The chapter enticingly headed "The Conspiracies of Lord Robert Cecil" quite fails to show him conspiring. Page 112 tells us that "there was something resembling a Cabinet dictatorship in 1919," and yet that in the "Cabinet some measures received minimal attention." Chanak, which intensively if briefly preoccupied politicians, passes unexplained, yet there is space to record that Sir Douglas Hogg had studied sugar-growing in the West Indies, and Sir Thomas Inskip was Chancellor of the Diocese of Truro (p. 438)—though not to show us how this relates to the impact of Labour. Again, we learn on p. 211 that 188 Conservatives opposed Chamberlain at that historic meeting at the Carlton Club, but must wait to p. 256 to learn he had only 88 supporters. (Uncharacteristically, Mr. Cowling has got both figures wrong.)

One also squirms at the dramatic oversimplifications. The dust jacket proclaims starkly: "Modern British politics begins with the Labour victory in the Spen Valley by-election in 1920." It might be pleaded that blurbs are allowed some special unscholarly license. But then comes the assertion that between 1920 and 1924 "the Conservative Party made three long-term decisions": "to remove Lloyd George from office," "to take up the role of defender of the social order," and "to make Labour the chief party of opposition" (p. 1). Unquestionably a decision was made to oust Lloyd George, but one reads Mr. Cowling's account in vain for any convincing documentation of "decisions" on the other two issues. Indeed as his own account suggests the situation was more ambiguous and the process of change more subtle and instructive than his simplistic formulation indicates. Overall there are some wise and stimulating observations in the introduction and conclusion, but the intervening text is curiously dissociated from them. No one acquainted with Mr. Cowling's scepticism about our discipline would expect the book to answer the preoccupations of political scientists, but one regrets that it is also irritatingly unsatisfying history.

Martin Harrison
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Provincial Magistrates and Revolutionary Politics in France, 1789–1795. By Philip Dawson. (Cambridge, Mass.: Harvard Univ. Press, 1972. Pp. 424. $15.00.)

This is primarily a history of the judicial and political activities of 2,700 magistrates who staffed the *bailliage* (lower) courts of France before the Revolution. Professor Philip Dawson devotes about one-third of the book to an institutional history of the bailliage courts before 1789, another third to the political role of the magistrates in the spring of 1789 and as deputies to the National and Legislative Assemblies, and the remaining third to the liquidation of their offices, their purchases of national property, and a sample of career profiles. As institutional and parliamentary history this is a very serviceable monograph. With the liquidation of offices in 1790, institutional history must necessarily cede to a study of individual professional careers as each magistrate tried to adapt to the new order. As "revolutionaries," Dawson's newly baptized "notables," emerge as "moderates" (opportunists?) and as "supporters," not "leaders" or formulators of revolutionary legislation. He would have it that their essentially passive support for the "revolutionary process" was important to its success and that the magistrates were motivated by a belief in a more open society and not by expectations of personal gain, material or professional.

This book raises several issues. Were it offered as an institutional history without any attempt to deduce the motives or ideology of 2,700 magistrates, there would be little problem. Unfortunately, Dawson attempts much more. He would present a social analysis of this entire "population" and deduce its values, motives, and political attitudes. In this effort he overreaches himself for the evidence he presents is either inadequate or inappropriate for the kinds of questions he poses and the interpretations he gives. In fact, his heavy apparatus of tables, sampling, percentages, and correlations, not only exposes the narrow base of his social and economic data, but also casts some doubt on his general conclusions about the political motives of his magistrates.

Dawson's sampling of the notarial materials (marriage contracts, acts of succession, death inventories) concerns 221 magistrates in Burgundy and Poitou. In fact, he has unearthed only 36 marriage contracts for Burgundy and only 12 in Poitiers. Dawson's habit of expressing his findings in percentages tends to hide this narrow base of cases, though it is all there in the appendix. Moreover, *summaries* of marriage contracts (used in the Burgundian sample) give only limited information about family or kinship relations and only a rough idea of wealth transfer. Consequently, I find the claim on the book flap that "the book is based on biographical data relating to 230 magistrates" somewhat misleading.

It would be an injustice to suggest that Dawson limits himself to notarial sources. In his chapter on "political mobilization" he relies on "ten extant cahiers" which he claims represent "the political views of one-tenth of the presidial courts" (p. 153). Although the discussion of the cahiers is one of the most persuasive parts of the book, Dawson's effort to treat "silences" in his materials leaves me unsatisfied. For example, he observes "the absence of a written cahier does not prove that concern and awareness were lacking" (p. 151). But then, what does it prove? This is the kind of interpretive acrobatics that Dawson is frequently tempted to perform to bend the evidence to the purposes of "motive" and to extend his conclusions to all 2,700 cases.

What about other sources? Is roll-call analysis not feasible for the magistrates as deputies? Why not pursue a content analysis of the speeches alluded to so briefly (p. 192)? The sixteen pages (p. 112–128) devoted to the intellectual life of the magistrates needs expansion; were there no more tracts, political or legal, written by 2,700 judges? Finally, what about personal correspondence? It is again unfortunate that the chapter on the "magistrates as deputies" is based on the letters (475 of them, to be sure) of only two magistrates from the old province of Maine. In any case, Dawson can not use such incomplete data to make assertions about the "magistrates' state of mind" (p. 206), "typically sympathetic to federalist ideas" (p. 301), or "their own most cherished ideals" (p. 340).

I remain unconvinced that these magistrates were bound by like-mindedness in political matters, and that regional variation did not play the role it apparently did among advocates. A lingering local corporate identity even after 1790, and local family rivalries like those at Auxerre (p. 62) may tell us more about political intentions (and about human opportunism, accident and contingency) than a host of broad sociological hypotheses about noble vs. commoner, rich vs. poor, ambivalent "intermediary classes," or unrealized "multiple potentiality" (p. 128). But whatever the appropriate range of questions, the 230 bailliage magistrates of Burgundy and Poitou would have been a more solid field of social and political analysis than the much wider canvas chosen by the author. The strength of this book lies in its explanation of the workings of institutions in the formal sense.

Robert Forster

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The Chilean Revolution: Conversations with Allende. By Regis Debray. (New York: Pantheon Books, 1972. Pp. 201. $6.95, cloth; $1.95, paper.)

This is a dated and not very significant book on the early days of the Chilean regime under President Salvador Allende as told in an introduction by the author, extended interviews with President Allende, and a postscript which consists primarily of Allende's First Message to Congress in 1971. It is not a book of professional research and offers no information that is not contained in numerous other publications or resources. It is superficial in its treatment and sketchy in analysis, and contains numerous errors (e.g., the description of Congress on p. 149; economic status vis-à-vis Mexico on p. 25; Allende's plurality vs. majority on p. 40). Debray also demonstrates in his questions to President Allende a very limited and superficial knowledge of Chile, its history, traditions, and prospects for the future. The book simply records in glowing and Marxian tones Allende's goals, the place of the experiment in the record of Latin American and world revolutionary experience, and the prospects for sources of support and opposition. The book is worth a cursory review—and only a minor place in the literature on this period of Chilean history.

The above comments should not be construed as my personal commentary or evaluation of the Allende "experiment," however. As I have written elsewhere, many of Allende's goals were admirable (e.g., efforts to improve the status of the marginal groups and to improve the country's capability to chart its own independent destiny in international affairs, economic and social development, etc.). It is also true that Allende inherited a series of shortcomings in the political system (e.g., the judicial system) requiring an overhaul. But, it is *also* true that the promise of Allende's regime was never fulfilled—whether as a result of outside or inside pressures or of the regime's own shortcomings. In any event, the book does not treat any of the above points or any of the period following Allende's ascent to power.

WESTON H. AGOR

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Developing Democracy. By William A. Douglas. (Washington, D.C.: Heldref Publications, 1972. Pp. 232. $8.50.)

This is a book with a message. It is also a book which attempts a comparative analysis of alternative political systems in less developed countries. The analytical aspects of the book are presented in the first section, while the prescriptive proposals are largely confined to the last chapter. Although the author seems to assume that the comparative analytical sections provide the basis for his concluding proposals, these two enterprises can be separated so that neither need prejudice the other.

Dr. Douglas spent almost a decade in Korea and Peru, both as teacher and as Education Director of the American Institute of Free Labor Development. During his service abroad he became preoccupied with the problems of forging democratic institutions in the developing areas. This book reflects his experience and views on that subject.

He begins by asking whether democracy is possible in developing nations, and if so, whether democracy can out-perform other political systems. He first compares democracy with dictatorship, and then with communism. Finally, one-party and multi-party systems are analyzed for their suitability for developing countries. From these comparisons, Dr. Douglas concludes that, although both dictatorship and communism have some advantages, over the long run democracy is superior because it provides adequate incentives for effective economic development, it assures social justice, it prevents exploitation, it checks corruption and it provides for the peaceful transfer of power. By contrast, both dictatorship and communism waste human and material resources, they are aggressive and expansionist, and they usually become preoccupied with organizing total political control rather than being primarily concerned with human welfare and the social betterment of all the people in society.

Having opted for democracy, the author acknowledges that it is difficult for democracy to take root in the underdeveloped areas of the world because the social and cultural prerequisites for democracy are lacking. Despite the impediments found in developing areas, he concludes that none foreclose the possibility or suitability of democracy, provided modern party structures are created with a mass base and led by a modernized elite who can "tutor" the masses and yet remain responsible to the public through genuine elections. What is needed, he argues, "is to build *two* or more elitist, mass-membership, ideological parties" (p. 99), which system he refers to as "dual tutelage." "Although tutelage and democracy may seem mutually exclusive in theory, they may actually coexist reasonably well in practice" (p. 99), provided there is a competitive party structure. The "dual tutelage" system he advocates will produce, he argues, a "regimented democracy" in which resources are mobilized, but with a continued use of incentives and persuasion as well as some coercion.

Readers who expect a rigorous comparative analysis of the performance and achievements of developing countries will be disappointed, for the author is content to make generalizations, some of them definitional and tautological, and then merely illustrate his propositions with a few examples or homely analogies. Many of his key categories are imprecise and based on his subjective evaluation. For much of his analysis, he is heavily indebted to others who have written interpretive works on the developing areas. Yet, he is far more audacious and categorical in his conclusions and far more prescriptive than most of the authors he relies upon. He continually explains what governments must do, but gives little attention to the strategies of how what must be done can be done.

Unfortunately, these defects and shortcomings may deflect many readers from completing the book without confronting the substantive proposals put forth by the author in the last chapter. Being concerned with techniques of promoting democratic institutional structures, the author proposes the formation of an "International League for Democracy" to be composed of democratic parties throughout the world. Such an association would give "political aid" to nascent democratic parties in developing areas and would be assisted by those member parties sympathetic to its cause. The League would support programs to transmit practical political skills, to promote modern attitudes among party functionaries, to assist with political analysis and to aid in the development of party ideologies and programs. Party leaders from various countries would meet annually to review their activities and programs and to discuss strategies and techniques of mutual assistance. Such a league might be supported by some government donations, but the bulk of its funds would be solicited from private sources, especially through contributions of individual citizens, particularly if the developed countries changed their tax laws to permit taxpayers to earmark a small portion of their income taxes for financing political assistance to such an international association of democratic political parties. The author fails to face many of the difficulties which would confront the formation of such a league. For example, what parties do not claim to be democratic? What criteria for membership would be applied? Does foreign "political aid" involve intervention in domestic politics? If so, will such intervention be acceptable merely because its sources of support are private? While these proposals deserve to be seriously discussed, the shortcomings of this book are such that it is unlikely to generate much momentum for the author's cause.

GORDON P. MEANS

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China and Ourselves: Explorations and Revisions by a New Generation. Edited by Bruce Douglass and Ross Terrill. (Boston: Beacon Press, 1970. Pp. xxii, 259. $7.50.)

Although several of the ten contributors have lived in or visited the People's Republic of China during the 1960s, this is not a book of reportage. Rather, having emerged from a project of the World Student Christian Federation, the book deals largely with certain profound philosophical contrasts between China and "ourselves." In the process the contributors explain the basic assumptions, values, and goals of Chinese Communism, thus helping the reader to find conceptual patterns among the mass of empirical observations reported by others in the past two years.

The authors are all young scholars. Unlike many of their elders in the field, they do not denounce China for refusing to adopt Western political institutions. Instead, they demonstrate the legitimacy, indeed the virtue, of the Maoist objectives, thereby establishing the standards by which China's performance should be measured. With this approach, the authors are led to an intelligent, though by no means uncritical, admiration of both the goals and the performance of the Chinese people, and are able to avoid the intellectual paralysis revealed by the use of such adjectives as "mystifying," "opaque," "baffling," and "irrational," which have been liberally applied to Chinese Communism by certain China scholars in the United States.

To several of the authors in this book, the Chinese process of modernization is not only intelligible but it also contains enormous promise for the Chinese people and points in the right direction even for the rest of the world. This promise is based on a deep-rooted humanism which, as the Chinese have shown, can dominate the process of modernization rather than be impoverished by it. Thus John Saari points out that "the Chinese have long had a passion and a genius for social morality" (p. 62) and suggests that this may be the key difference between Chinese and Western paths of political development. Kazuhiko Sumiya, using the perspective of Weber's sociology of religion, holds that whereas capitalism has long succeeded in perverting the ascetic ethos of early Protestantism, the Chinese Communists, by mobilizing the dispossessed masses against the institutionalization of greed, have taken over the spirit of religious asceticism, and thereby the "switch lever of history" (p. 221). And Neal Hunter argues that, in contrast with capitalism which abuses and wastes material things (as well as human beings) in its search for profits, the Chinese, from ancient times to the present, have had a profound respect for their material environment as the source of their life. The Chinese, therefore, unlike capitalist societies, will long continue to work *with* nature (including man as part of nature) rather than *against* it.

Within socialism, however, there are different degrees of regard for human values. Douglass finds that socialism from Marx to Lenin and Stalin, in its emphasis on rapid industrialization, has paid the price of diminishing the original humanistic vision of socialism, but that the Chinese, for several historical and cultural reasons, have been able to combine industrialization with commitments to egalitarianism, community, and democracy. But even within China, the Maoists have adhered to these commitments much more intensely than the revisionists have. Ray Wilie stresses this internal divergence in his chapter on the meaning of the Cultural Revolution, whereas Stephen Fitzgerald (who has since become Australian Ambassador to China) sees the conflict between the two camps more as a struggle for power than over genuine differences in ideas.

Feliciano Carino describes the fascination many Asians find in the revolutionary fervor, the optimism, and the emphasis on idealistic struggle and sacrifice in Maoist theory and practice, at a time of an "inner crisis and disintegration of Western society" (p. 69) and of continued economic and cultural domination by the West of their former colonies.

Two of the chapters deal with American China-watchers of the late 1940s. Tom Engelhardt shows that most American journalists and diplomats residing in China at that time were contemptuous of the Nationalist regime for its multiple failings, but they did not grasp the depth of suffering of the Chinese masses and therefore could not understand the need for revolution. When the revolution did come, these reporters saw it as an artificial and alien phenomenon.

This misperception of Chinese Communism was later succinctly expressed by Dean Rusk when he spoke of the Peking government as a "colonial Russian government" which was "not Chinese" (p. 127). Terrill's chapter illuminates the idiocy of this type of nonthinking which led John Foster Dulles to sack one of America's most experienced China hands, John Carter Vincent, for not sharing these types of views.

Edward Friedman reviews the evolution of U.S. policy toward Taiwan and rightly blames the U.S. for the 20 years of tension in the Taiwan Straits. Rapprochement between the U.S. and China is possible, he says, as soon as Washington cuts itself loose from Chiang Kai-shek. In this, Friedman's analysis has been partially borne out by subsequent events. But the rapprochement is neither far-reaching nor solidified enough to substantiate Friedman's belief "that there is no cluster of fundamental, vital, irreconcilable interests separating the People's Republic of China from the United States of America" (p. 171).

In sum, most of this book deals penetratingly with the distinctive and pathbreaking qualities of Chinese Communism. It will enrich those who are already sympathetic toward this noble experiment, and, it is hoped, provide a flash of comprehension for the rest.

OLIVER M. LEE

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The Impact of Government Subsidies on Industrial Management: The Israeli Experiment. By Nachum Finger. (New York: Praeger Publishers, Special Studies in International Economics and Development, 1971. Pp. 100. $12.50.)

Technology of Necessity: Scientific and Engineering Development in Israel. By Roy Popkin. (New York: Praeger Publishers, 1971. Pp. 185. $7.95.)

Professor Finger's study is an empirical analysis of the subsidy system as an incentive to industrial investment which takes the Israeli experience as a model. Subsidies in general and those provided to manufacturing enterprises in particular "have become accepted tools for the implementation of preferred government policies" (p. vii). Three major areas of management decisions in which the impact of government subsidies is felt are examined: the financial area, the plant locations, and the choice of technology. Analyzing each of these areas, Finger develops a "subsidy maximizing firm" concept (pp. 74–83): that subsidies are accepted tools for the implementation of preferred government policies. In a comparative analysis of the classical case of profit-maximizing firm strategy and tactics with those of a subsidy-maximizing firm, Finger traces five ways in which the impact of the government affects the latter; (a) The subsidized industrial firm has a built in higher probability for survival; (b) management costs are higher where the role of government financial support is greater than in nonsubsidized firms; (c) firm managers are drawn ex-governmental officials; (d) capital structure loan guarantees result in partial payment guarantees; (e) and these firms have lead time to create market certainty.

Although this is a most persuasive case study of strategy and tactics of industrial management in Israel, the book suffers from a lack of conclusions and any clear indication as to the author's preferences, unless one opts for the subsidy-maximizing firm as an efficient managerial decision-making system. Still, it is a first rate analysis of economic decision making and for this reason a serious contribution to economic theory. No economist should miss this systematic and careful analysis. It is truly elegant. The author's professionalism is immaculate.

Roy Popkin, on the other hand, has written a sloppy narrative of the Israeli "miracle" in the field of technology. His ideas, his spelling of names, and his transliterations are inaccurate.

Tracing early Israeli innovations in agriculture, in phosphates, and in chemical industries, and finally the development of her complex nuclear industry, Popkin has given us a book which comes across as a travelogue. There is an endless marveling at Israeli ingenuity without any attempt to explain the socio-psychological factors that contributed to this quality. The book does not explain why in Israel, necessity innovated a technology of scarcity, only that it is so.

The last chapter is the poorest of all. Here the author deals with the Israeli defense industries. One wonders why he bothered to submit his work to the Ministry of Defense for review (p. 157). Most of the "secrets" presented in this chapter are common knowledge. They can be found in the Israeli dailies, the International Press, as well as in several monographs on Israel. The Israeli defense industry is a most fascinating human organizational and engineering effort. Popkin, unfortunately, treats it in a drab descriptive manner. Here is where he should have begun a socio-psychological analysis of Technology of Necessity (*Ein Breira*). How did the industry grow in view of the psychology of necessity? What are the relationships between necessity and efficiency?

No analysis of the Israeli army is even offered. Yet it is in this organization that the Technology of Necessity reached its apex.

The emergence in Israel of an independent military industrial complex under Shimon Peres, Deputy Minister of Defense (1953–1963), needs to be explained beyond the "necessity" claim. It is not even alluded to in Popkin's book. The Israeli contribution to the theory and practice of the Technology of Necessity could have afforded the author some excellent ideas as to how it actually works. Instead we are left with cliches, poor stories, and no analysis. How could such a fascinating topic be so abused?

AMOS PERLMUTTER

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Governing the London Region: Reorganization and Planning in the 1960's. By Donald Foley. (Berkeley: University of California Press, 1972. Pp. xiv, 223. $8.50.)

Governing the London Region inaugurates what promises to be an important and valuable series. The pressures of urbanization and suburbanization are creating problems of metropolitan government in most countries of the world. The universality of the many problems involved and the variety of solutions sought in different parts of the world make this an obvious area for comparative study. But while comparisons are obviously useful, they can also be dangerous. For an author they raise two particular difficulties: he must communicate the key features of a system of government to an audience largely unfamiliar with it. At the same time he must provide the evidence for an evaluation of that system and demonstrate its relevance to another setting.

The first of these tasks has been admirably done by Professor Foley. In Chapter 1 he traces the growth and changing distribution of population and the emerging employment patterns in what he calls the 'many Londons.' He draws on various statistical sources and successfully avoids the aridity which such material often produces. Chapter 2 is equally effective in describing the new two-tier government established in London in 1965. The Herbert Commission which recommended the changes is set into the historical development of London government and some of the transition problems are briefly considered. This clear outline of the structure of metropolitan government provides the necessary background for an examination of the operation of metropolitan politics. Unfortunately the brevity of the book restricts the discussion of this aspect to a formal review. Thus while the legal framework and the pattern of institutions is clear, there are only occasional glimpses of the life of politics which surrounds them.

The result is that the reader does not acquire a feeling for how London politics actually operate. Formal relationships between Boroughs and the Greater London Council, or within the individual Boroughs, offer only limited guidance. Relationships between officials and elected members and the role of political parties remain concealed. The key issue of the benefits of reorganization remains difficult to answer. In an attempt to do so, several problem areas such as transportation, housing, education, and crime are examined in Chapter 6. These are common to most large urban areas and their treatment in London is obviously significant. Again, however, the reader is not given sufficient information to judge the effectiveness of that treatment. Education, for example, is dealt with in only five pages, and though several key issues are referred to, too little is said to permit a real understanding of their complexity. Inevitably too there are omissions, particularly of issues like "positive discrimination" which are becoming critical in the metropolitan context. The same could be said of the other areas singled out which is probably inevitable in a brief volume of this kind. Planning is the exception, as one would expect, but concentration on questions of land use is a limitation here.

These limitations become much more significant when Professor Foley seeks to demonstrate the relevance of London experience elsewhere. One question involves the applicability of a centrally dictated reform to countries with very different systems of national politics. More important, the desirability of such a reform must depend on answers to questions which are missing here. The question of whether and how well the system works is obviously one requiring a long and complex answer. There has not been a breakdown of government, and services are not noticeably worse than before. In this sense it may be said to have worked. But London's problems remain, and there is little sign that the new system will solve them any more than the old.

This is particularly interesting in relation to Chapter 7, where Professor Foley describes the application of a two-tier system to other British metropolitan areas. These come into operation in 1974, and are a further demonstration of the capacity for reform in a centralized system, once the need is established. But there are current doubts about whether they will solve the problems which they were designed to deal with. Corporate planning needs within the metropolis are not easily handled in the two-tier system. Nor are the particular units necessarily the most appropriate either in terms of resources or of politics. These remain imponderables, but like London one waits for answers related to performance and impact before recommending adoption of the structure. Foley's careful advocacy of two-tier government may be right but his book does not prove the case. Rather it is the necessary basis for an understanding of the debate to come.

Noel Boaden

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Kin, Clan, Raja and Rule: State-Hinterland Relations in Pre-industrial India. By Richard G. Fox. (Berkeley: University of California Press, 1971. Pp. 187. $8.00.)

Generations of anthropologists have searched for methods of transcending single community studies. Few investigations, however, have succeeded in combining the depth of the single community ethnography with imaginative use of comparative and historical materials. Anthropologist Richard Fox's analysis of Rajput kinship and of state-hinterland relations in northern Indian history is a self-conscious attempt to show how the unit of analysis problem can be handled in complex societies. He believes that: "The value of a research technique which does not link the local area to the larger society in terms of historical and contemporary social processes is highly questionable" (p. 3).

Using historical materials to look at the role of genealogical reckoning in pre-industrial Rajput politics, Professor Fox analyzes how local areas organized political autonomy and in what ways local political organization influenced or was influenced by the state. These issues ultimately lead him to grapple with the problem of the origin of the state. Fox makes an excellent summary of the anthropological literature on state development, and finds that in India the territorial state was only a stage in the Rajput lineage developmental cycle. Stratified lineages remained important features of northern Indian local government until the advent of the British.

Development of the cycle of Rajput lineage is the central theme of the book and worth the effort involved for a political scientist to plough through earlier discussion of Rajput lineage and kinship terminology. Fox's goal is to show the structural principles of lineage organization. In the process he illuminates the changing historical roles and political relationships among political elites, clan leaders, and peasants. The lineage development model has five stages, and differentiation in the characteristics of the stages reflect differences in the interrelationship of four variables: the availability of land; the population, cohesiveness, and martial powers of the lineages; the viability of the lineage; and most important, the power of the state.

Yet it is precisely in the discussion of the Rajput lineage cycle that Fox's methodology most often fails the historian or political scientist. One of his central points is the correlation between state power and Rajput kinship structure. He hypothesizes that: ". . . when the state is strong and locally intrusive, the kin behavioral order may be completely deposed. When the state weakens, such attributional survivals may provide the rationale and charter for a new growth of local corporate kin groups" (p. 171). But this key independent variable (state power) is not explored. Fox acknowledges he knows little about what weakens the state, but acknowledgment does not mitigate the importance of omission. Early in the book he rejects the crucial methodology by omitting ". . . detailed examination of specific political events between state authority and kin groupings" (p. 11).

Nevertheless, *Kin, Clan, Raja and Rule* is a good book, important for political anthropologists, Indianists, and others interested in the broad spectrum of methodological, philosophical, and theoretical issues raised by Fox.

Marguerite Ross Barnett

Princeton University

The Origins of Capitalism in Russia: Industry and Progress in the Sixteenth and Seventeenth Centuries. By Joseph T. Fuhrmann. (Chicago: Quadrangle Books, 1972. Pp. viii, 376. $12.50.)

Teachers of Russian economic history will welcome this book as a useful addition to their still modest list of sources in English; and students of economic development will find in it many interesting parallels. The author begins with the background of Russian urban life, artisanry, and commerce in the fifteenth and sixteenth centuries, and then proceeds to the description of economic development in pre-Peterine Russia. Most of the book is devoted to the activities of foreign entrepreneurs in iron manufacturing (the subject of the author's doctoral dissertation). This is certainly the best and the most informative part of the book. The technical and financial aspects of these enterprises, their owners' relations with the Moscow court, and their personal lives are described in great detail. The reader gets a good picture of what it was like to be a foreign entrepreneur in Moscow in the seventeenth century.

With this kind of focus, it might have been better to call the book *The Activities of Foreign Entrepreneurs in Russia in the Seventeenth Century,* or something like that, rather than to claim to have presented the origins of Russian capitalism. The latter is a complex subject which cannot be properly treated in a book of this size and kind. The book has no analytical framework and contains little, if any, economic analysis. Hence the author does not get far beyond a few terminological comments (pp. 4–11, 263–68).

Statistical data for the period are hard to come by, and we should be grateful to the author for his attempts. But one wishes that he had "worked" a bit more with his figures and, among other things, converted absolute numbers (as for instance customs receipts as a fraction of state revenue) into percentages (p. 57). A difference in grain prices of 15 to 20 per cent between Russia and the West need not have produced "tremendous profits" (p. 59) if transport costs (not mentioned) were high. Low wages in Russia did not necessarily result in low costs (p. 180), particularly with high salaries of foreign masters (pp. 180–81). The market mechanism which allowed a *pud* of raw silk delivered in Archangel for thirty rubles to be resold forthwith for forty-five (p. 33) deserved some examination, particularly in view of the low profits obtained in Russian trade with Persia reported on the following page. It is probably true that serfdom retarded the economic development of Russia by limiting the supply of wage labor (p. 258), but it would be nice to be shown some data on wages.

Like many Americans writing on Russia, the author had a few scraps with the language. Thus the word *malomochen* (p. 81) is more likely to mean "not strong" (financially, in this case), rather than "of poor character." The expression he translates as "in council and friendship" (p. 81) probably means "in agreement and friendship." *Luchina* is an easily-burning wood splinter rather than a "special lamp" (p. 123).

Several historical inaccuracies and ambiguities have also crept in. Russia cannot be said to have been "relatively free from attack from . . . the South" by the early 1500s (p. 136) if Moscow was burned by the Crimean Tartars as late as in 1571. The Great Northern War ended in 1721 and not in 1724 (p. 129). The navy which Peter I built for the Azov campaign can hardly be called "oceangoing" (p. 203). Is it really true that Germany produced 30,000 tons of iron in the later 1400's and less than 18,000 tons in 1740 (pp. 262–63)? How could generals receive estates of 800 *sazhens* (p. 222) if a *sazhen* is a linear measure?

These, however, are minor issues. They hardly affect the general quality of the work.

EVSEY D. DOMAR
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The Era of Trujillo: Dominican Dictator. By Jesús de Galíndez. Edited by Russell H. Fitzgibbon. (Tucson: The University of Arizona Press, 1973. Pp. 298. $4.50, paper; $8.95, cloth.)

Russell H. Fitzgibbon has performed a notable service. After a long and persistent effort in locating and gaining permission from the heirs of the martyred Jesús de Galíndez, he has edited and caused to be published in English this devastating study of the Trujillo dictatorship, for which its author was apparently killed. *The Era of Trujillo,* as Fitzgibbon makes clear, is a uniquely valuable study not only because it was a pioneer contribution to scholarship but also because of its dramatic link with abduction, murder, dictatorship, and international intrigue.

Galíndez was a Spaniard, a Basque; his loyalties in the Civil War of the 1930s were Republican. Following the defeat of the Republicans by Franco's forces, Galíndez was forced to go into exile, settling in the Dominican Republic because at the time it was one of the few countries to open its doors to European refugees. In Santo Domingo (then Ciudad Trujillo) Galíndez taught and worked in the labor ministry. In 1946, having outlived his welcome in Trujillo's dictatorship, he moved to the United States, settled in New York, wrote, taught at Columbia University and completed there the doctoral dissertation on which the present study is based. Shortly after defending the thesis before his committee, he disappeared on March 12, 1956, and was never seen or heard from again. Although the evidence is incomplete and though Galíndez's own activities remain shrouded in mystery (for example, at the time he was also serving as treasurer of the Basque government in exile, the CIA was immediately involved in his case, etc.), the most plausible explanation, supported by much circumstantial evidence, is that Trujillo had Galíndez kidnapped, flown to the Dominican Republic, and killed, both to prevent the dissertation from being published and to serve as an example to other scholars contemplating studying his regime. Galíndez's disappearance became a cause célèbre prompting a number of congressional, FBI, and other investigations, drawing in a variety of shadowy and not-so-shadowy public personalities, causing an international uproar, and perhaps eventually helping lead to Trujillo's own demise in 1961.

The original dissertation was 689 pages and constituted an overwhelming indictment of the Trujillo dictatorship. The author was a lawyer and marshalled his case as a lawyer would. He was objective but by no means dispassionate. At the time his work constituted the first scholarly study of one of this century's longest-lived and most complex dictatorial forms.

Galíndez's account begins with some general, comparative, and theoretical comments on the nature of dictatorship in Latin America. This is followed by a long chronology of the Trujillo era, from 1930 to the time of Galíndez's writing in 1955. The heart of the book, however, lies in Galíndez's analytic dissection of the methods of Trujillo's control. He has chapters on the perversion of the constitutional system and on elections and the use of forced resignations. Galíndez shows how Trujillo kept the society and its people atomized so as to prevent any threat to his power. He discusses the uses of Trujillo's single party system as a bureaucratic appendage of the regime, and of controlled labor unions employed to harness social change. He shows how Trujillo dominated all social institutions, analyzes Trujillo's personal style as *jefe* and *benefactor,* and demonstrates how Trujillo skillfully manipulated the "anti-Communist" issue. The book contains additional chapters on international politics during the Trujillo era, on the regime's positive accomplishments, and on its reactions to criticism and forces of opposition. The conclusion points to the absolutist nature of Trujillo's rule, his use of both traditional and totalitarian methods, and, because of this, the total institutional vacuum that his death ushered in.

Fitzgibbon's editing of the volume is careful and thorough. Drawing upon his long Latin America experience, he has added a preface regarding the Galíndez case, a prologue showing Trujillo's legacy, an epilogue that brings Galíndez's treatment to the end of the Trujillo era, and a bibliography to supplement and update the author's own. He has cut out some repetitious materials of the original dissertation while preserving its completeness and scholarly credentials. He has added an occasional note to the book to correct Galíndez or provide more up-to-date information, but he does not intrude and preserves the integrity of the Galíndez manuscript. Moreover, Fitzgibbon's additions are in keeping with the spirit and thinking of Galíndez himself, seeing Latin America largely in terms of the struggle between dictatorship and democracy.

More recent scholarship would challenge this perspective, arguing that Latin America should be understood on its own terms rather than in the light of North American liberalism. Moreover Galíndez's largely formal-legalistic approach, while useful for gaining insight into some key aspects of the Trujillo regime, serves to obscure the socioeconomic changes occurring under Trujillo, the political-cultural context in which his rule took place, and its more functional aspects. By now, in addition, the Galíndez thesis has in considerable measure been superseded by the work of other scholars (often using a briefer, earlier Spanish edition of his book as their base). Nevertheless the Galíndez work still serves as a prime source and a nice complement to these others; its publication by the University of Arizona Press and through Fitzgibbon's efforts is both a significant contribution to scholarship and, given the impact of Galíndez's dramatic disappearance, an *event* that is certain to rekindle these old controversies.

HOWARD J. WIARDA
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The Politics of Mexican Development. By Roger D. Hansen. (Baltimore: The Johns Hopkins Press, 1971. 267 pp. $11.00.)

Roger Hansen has written a book that no serious scholar of Mexico or the Latin American scene can afford to ignore. More than any single volume currently available, it provides a balanced assessment of the Mexican government's successes and failures in handling political, social, and economic change since the 1910 Revolution. Integrating penetrating economic and political analysis, it offers a survey of contemporary Mexico that will appeal to the specialist and general reader alike.

There are a number of reasons why this is the case. To begin with, Dr. Hansen, an economist by training, examines the determinants of economic growth in Mexico from an institutional perspective. Rather than restrict himself solely to economic factors, he sees the emergence of a modern economy in Mexico as the result of a complex set of political, social, and cultural conditions. He convincingly shows that the success of Mexican economic development is largely the consequence of conscious choice by a ruling elite that has combined traditional and modern values in a manner congruent with the norms of Mexican society.

Next, Hansen demonstrates that the roots of Mexico's economic growth are to be found as much in the last decades of the 19th century as in changes institutionalized after 1940. Examining the interplay between politics and economics since independence, he observes that there have been only two periods of sustained economic growth and that these have coincided with eras of political stability: the Diaz dictatorship, around the turn of the century, and the Revolutionary Coalition's rule since 1940. Yet, if stable political leadership distinguishes both periods, the content of these authoritarian regimes has been markedly different. The Revolution, writes Hansen, "removed an economically nonproductive elite, foreclosed old patterns of nonproductive investment, and redistributed some wealth" (p. 40). In the aftermath, a highly pragmatic *mestizo* political elite, committed essentially to its own social and economic mobility, emerged. Sound policies of public investment, indirectly encouraging and sustaining a dynamic private sector, appeared. And income policy was altered. The effect was to expand the upper-middle income sector, at the expense of those at the top and the bottom of the income scale, and to create a national market adequate to support new levels of industrialization.

While engaging in comparative analysis, by contrasting different historical eras in Mexican development and by juxtaposing Mexican experience with that of other Latin American countries, Hansen reviews the established literature in economics and politics on Mexico. He offers a creditable evaluation of standard sources—among which are to be found the works of Raymond Vernon, Pablo Gonzalez Casanova, Robert Scott, Frank Brandenberg, and Vincent Padgett. Simultaneously, he provides the informed reader with a well-documented interpretation, showing that the economic successes achieved by modern Mexico have come from an authoritarian regime whose power basis was consolidated in 1929, well before the epoch of sustained economic growth began. For him, economic expansion in Mexico has been due largely to the regime's ability to limit demands from the lower sectors, to its capacity for stimulating diffuse support for the political system through manipulation of revolutionary symbols and use of a single mass-based political organization (the PRI), and to its capability for retaining the support of the "politically relevant members of Mexican society" (p. 175).

Another reason this book merits careful consideration is that it weaves together the economic, social, and political transformations that have swept across Mexico in the 20th century, without falling into the sterile debates that have characterized much of the political development literature and earlier individual country case studies adhering to the standards of structural-functionalism. From the outset to the conclusion, Hansen takes as his central concern the political economy of modern Mexico. He thus arrives at a synthesis of the central issues in the Mexican policy process. In so doing, he achieves an understanding of the dynamics of Mexican society that has escaped many earlier social scientists, men who have restricted themselves either to examining economic development patterns, without doing careful political analysis, or to debating the degree of political representation or repression, without considering sufficiently the economic content of governmental policy.

In the course of this book, Hansen moves from an analysis of the sources of Mexico's economic growth into an examination of what he calls the era of "economic fruition." After establishing what these growth rates have meant internally, for Mexico, and comparatively, from the vantage point of other major Latin American economies, he then raises a series of questions that are preeminently political: Who have benefited from these economic changes? How do the PRI and the Revolutionary Coalition fit into the larger economic picture? What are the dynamics of Mexican politics? Utilizing the views of Samuel Huntington as a means for setting the course of Mexican modernization in a wider comparative context, he at the same time draws attention to discrepancies between Huntington's theory and Mexican reality. He points out that while Huntington "offers Mexico as the very model of an 'institutionalized civic polity,' . . . the behavior of successful Mexican politicians as measured by the governmental policies for which they are responsible clearly exhibits . . . pronounced 'praetorian' tendencies" (p. 102).

Contrary to the interpretation that views Mexican government as immobilized by the broad range of demands placed on it, he maintains that "what actually holds the present regime together is . . . a system of mobility that attracts the personal allegiance of spokesmen for all the PRI sectors from the bottom to the top of the party hierarchy" (p. 220). It is the very structure of personal loyalties developed by the Revolutionary Coalition, he says, that has allowed it to suppress or to ignore opposition and discontent.

In summing up this survey of Mexican development, Hansen sees the present political arrangement as one that is likely to endure for the foreseeable future and to continue to underwrite sustained economic growth. He probes the present sources of political stress and concludes that, given the legitimacy accorded to the government by major components of Mexican society and the willingness of the government to use force to preserve the present status quo, any other outcome than continued rule by the Revolutionary Coalition is indeed remote.

LAWRENCE S. GRAHAM
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Telling Tongues: Language Policy in Mexico, Colony to Nation. By Shirley Brice Heath. (New York: Teachers College Press, 1972. Pp. 300. $10.00.)

As the book's subtitle indicates, this is a study of language policy in Mexico. Language policy? The idea is an intriguing and novel one, and, as Professor Heath saw, a history of the policies adopted on language questions could, in the Mexican context, not only be of interest in itself but could also shed light on the evolution of Mexico's social and cultural problems. For in Mexico it has not been self-evident, until at least very recently, what language people should speak.

Of course, the problem has simplified itself through time, with the first colonial administrations facing the more complex questions. It was not simply a matter of Spanish and the Indian languages. There was the question of whether to deal with individual Indian languages or to continue the Aztec policy of encouraging the general use of Nahuatl as a lingua franca. Indeed, for a time the pro-Nahuatl position triumphed and in 1570 a cédula of Philip II made Nahuatl the "official" indigenous language of New Spain. But even Spanish did not have things its own way; the earlier educators were friars for whom it was more important to teach Latin than Spanish. But enough priests suffered embarrassment when better-educated Indians detected how poor their Latin was, to eliminate Latin instruction for Indians. Creoles opposed the teaching of Spanish to Indians because it tended to make them impudent and get ideas "above their station." The attempt to promote Nahuatl died away, although Maximilian issued decrees in both Nahuatl and Spanish.

To tell a story as interesting and complex as this, one ideally should have skills in linguistics, anthropology, education theory, and political science. Perhaps it is not surprising, therefore, that Mrs. Heath, an anthropologist specializing in sociolinguistics, should fall rather short of the ideal in a valiant effort at what proves to be too difficult a task. Her handling of the colonial period is the best part of the book, perhaps because the issues involved were then faced squarely and explicitly so that they could be presented for decision to the monarch. For the period covered by the rest of the book, the author frequently gets bogged down in summarizing the nebulous views of rival educational theorists without showing clearly where they differed from each other, or indicating the actual importance of the rival theories in practice, or delineating the political or economic interests linked to one or another view. Political decision makers are too frequently presented as though each question were decided solely on its merits without reference to class or other interests involved.

The principal educational issue in the language field has for some time been the desirability of bilingual education (which begins the pupil in his mother tongue, in which he becomes literate before tackling the national language) vs. the "direct method" (in which only the national language is used in the school and the student is forbidden to use his native language). According to all the evidence, the bilingual method is more effective, apart from being more humane. Nevertheless, misguided pedagogues, patriots, elitists, and assorted ideologues have opposed bilingualism. Doctrinaire liberals (including Miguel Alemán) thought that public authorities should treat all people equally, refusing to recognize ethnic differences. Avila Camacho's first minister of education tried to eradicate bilingualism in an anti-Communist fervor; under Cárdenas, the ministry had been staffed by Communists who supported their pro-bilingualist position with citations from Stalin's writings on the national question (surely the most intelligent theoretical work the Georgian produced).

Related to the bilingual versus direct methods question are a host of other issues: academic versus practical education, Mexican versus peninsular Spanish, acculturation of the Indians versus the preservation of the indigenous cultures, the problem of the differential functions languages have for bilinguals. Some of these questions are treated by Mrs. Heath in passing, but one misses clear discussions of the problems involved and of the present state of knowledge. Perhaps this is expecting too much of the 200 pages of text and 100 pages of appendices, bibliography, and footnotes of what is clearly a revised doctoral dissertation. One must hope that others will continue to build, now that Professor Heath has broken ground.

Martin C. Needler

University of New Mexico

La Politique dans L'Espagne Franquiste. By Guy Hermet. (Paris: Librairie Armand Colin, 1971. Pp. 160. 8.80 Fr.)

This little volume is an edited work designed to give an introductory overview of the Spanish political system. As such, it has many of the virtues and shortcomings shared by so many books of readings in the social sciences. On the one hand, the reader is given a certain feel for the breadth and complexity of the subject under scrutiny; the frequent shifts from one author to another, and from book excerpt to periodical article to legal document, make for a refreshing change of pace; the variety of interpretations prevents the material from being forced into a conceptual straitjacket. But on the other hand, the great number of brief contributions present a bewildering array of conflicting approaches. Breadth and catholicity are sometimes achieved at the expense of critical analysis. The author's introductory chapter, and introductory passages preceding each section, help to mitigate these shortcomings somewhat and to give the volume a certain unity; but the problem of coherence is always present in a work of this nature.

All things considered, however, this kind of book fulfills a useful function as a pedagogical device for the general reader. And in this respect, Hermet's book seems to have considerable value. Its very brevity is a saving grace; for intellectual smorgasbord can easily become indigestible if consumed in large doses (editors of painfully lengthy American books of readings please take note!). Professor Hermet has been quite ruthless in pruning many of the contributions to workable size, so that the reader never loses sight of the basic themes laid down in the author's introductory passages.

The source material on which the author draws is rich and varied. There are excerpts from certain documents—the pre-Civil War platform of the Falange, a number of articles from the Concordat of August 27, 1953, the law of May 17, 1958 on the principles of the National Movement, etc.; excerpts from a large number of books and journal articles; and a number of vivid newspaper accounts of various aspects of Spanish political life. Particularly interesting are the brief discussions of the relationship between the Franco regime and various power centers in the Spanish political system: the army, the Church, and the bureaucracy, including the technocrats of the Opus Dei. The section dealing with the role of the pseudo-opposition is very intriguing, as are the analyses of Spanish political attitudes. Despite its brevity, Hermet's book manages to illustrate the peculiar character of Franco's autocracy, with its frequent oscillations between repression and grudging tolerance of some forms of dissent, with its not always successful attempts to serve as broker among a number of conflicting elite groupings, with its half-hearted gestures in the direction of liberalization. It also reveals the growing strength and outspokenness of oppositional tendencies, held in check as much by the apathy of the mass of the population as by the coercive resources of the regime.

As Hermet is the first to concede, there are serious gaps in his presentation, and the book falls far short of giving a comprehensive analysis of the Spanish political system. But within its limited terms of reference, Hermet's volume does a creditable job of touching on the main characteristics and problems of the Franco regime and of stimulating the serious reader to further studies of this topic.

Raphael Zariski

University of Nebraska

Madagascar. By Nigel Heseltine. (New York: Praeger Publishers, 1971. Pp. x, 334. $11.00.)

One of the minor mysteries of contemporary writing on Africa is that most authors who set out to widen our knowledge of Madagascar seem to feel a compulsion to start from the letter "A." The present volume is no exception. While it is true that the literature in English on Madagascar is not extensive, there would appear to be a sufficient number of works available that a new volume such as this should not require the first third to be devoted to the land, the people, and the society.

Once Heseltine has gotten over the hurdle of elementary description, he has produced an interesting and straightforward discussion of the rise and development of the Malagasy state prior to the onset of the French colonial period in 1896 and of the political process which led up to independence. These chapters are followed by a description of the Malagasy economy and a final essay on the prospects for development.

The author is intimately acquainted with the island, having served as economic adviser to the President of the Republic. In this capacity he had an opportunity to see the country at firsthand and to make comparisons based on his extensive experience in development planning in various countries of continental Africa. It is not surprising, then, that the most satisfactory chapters are those dealing with economic problems and prospects.

The story of the conquest of Madagascar by the Merina, the first contacts with Europeans, and the remarkable acceptance and employment of Western technology by the ruling group is one of the most intriguing in all nineteenth-century African history. The Merina had developed a complex administration before the European contact based on an oligarchic system which survived through the nineteenth century, although the monarchy was gradually weakened through internal struggles and by the rapid spread of literacy in the indigenous language as a result of competing missionary efforts. The consequence was that when the French finally succeeded in establishing colonial domination, they were faced with substantially different problems of administration from those of the West African colonies established at about the same period.

As the author points out, France was fortunate in having as governor during this initial period one of the great figures in French colonial history, Galliéni, whose policies of pacification, primarily by political rather than military means, were well suited to the temperament of the proud Merina people. But Galliéni's skills were not matched in his successors, and the embers of Merina nationalism smouldered on under the French colonial administrative control, although the Merina rulers benefited materially from the economic growth made possible by French private capital.

Nationalism flared once more in spectacular form with the revolt of 1947, one of the less well known but probably the bloodiest episode in the postwar independence struggle in Tropical Africa. Although independence was not to come for more than a decade, the 1947 revolt convinced the French that, ultimately, there could be no other solution. The author of the present volume treats the event with too light a hand; its significance not only in hastening independence but in its long-range results in the disruption of Malgache society cannot be overestimated.

The chapters dealing with the Malgache economy set the stage for the subsequent discussion of developmental prospects. Heseltine's description of the island, its resources and its possibilities is a brief and excellent compendium, clearly drawn from firsthand knowledge. Similarly, the discussion of the first Five-Year Plan (1964–69) gives a succinct sketch of the major economic directions toward which the island's independent government has directed its efforts. As with most African states, major emphasis was successfully laid on agriculture, which achieved 127.6 per cent of its objective; it is indicative of the problems of development, however, that only 49.5 per cent of the objective in education and health was achieved in the same period.

This is a useful and thoroughly objective discussion of Madagascar by an author whose knowledge of his subject cannot be contested. But this complete objectivity diminishes the value of the volume as a whole. The reader might have wished that Mr. Heseltine had ventured a few opinions of his own and had engaged in much more analysis and substantially less description. It is inconceivable that Malgache history was as dull a procession of events as the author makes it seem or that he did not disagree at some points, at least, with the standard versions of it. The real fault with the volume lies neither in its accuracy nor in the comprehensiveness of its coverage. Rather, it is simply that the island, its people and its beauty are reduced to such a degree of arid scholarship and dull prose that they are robbed of all the essential dramatic vitality which any one who knows Madagascar cannot fail to observe.

L. Gray Cowan
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Trade and Industrialization in the Central American Common Market, the First Decade. By Karel Holbrik and Philip L. Swan. (Austin: University of Texas [Bureau of Business Research, Graduate School of Business], 1972. Pp. 67. $2.50.)

This short study is the most recent of a handful of monographs on the Central American Common Market and the forces which are tearing it apart. Three short chapters introduce the subject—an all-too brief one on the political and historical background of Central America's attempts at politico-economic integration, another on the economic theory of common markets in general, and the third on the major institutions that administer the market and its trade relationships. The next two chapters on trade expansion and industrial growth are the significant contributions of this monograph.

The authors point out that elimination of most tariff barriers between member countries during the first nine years of the Central American Common Market brought a 700 per cent increase in inter-regional trade and a 10 per cent increase in trade with countries outside the market. These impressive gains for the market as a whole were not shared equally by individual member-countries; Nicaragua and Honduras gained only slightly, while El Salvador and Guatemala improved their trade considerably. Since customs duties were largely abolished for commodities in intra-regional trade, revenues from that traditional source of income for national budgets rose only slightly. Income and sales taxes did increase to compensate for losses in customs revenue, but this increase benefited the member-countries with the greatest degree of economic development and put the others at a relative disadvantage.

Because the trade balances of all member-countries were adverse from 1960 onward, the members imposed tariff surcharges and luxury taxes to restrict imports from outside the market. By 1970 the trade and payments positions of all members were improving with the exception of Honduras which became heavily indebted to Guatemala, Costa Rica, and Nicaragua. Honduras thereupon reimposed duties on intra-regional trade which effectively removed it from the market. The other members retaliated by reimposing duties on Honduran goods. This created surpluses in the semi-manufactures produced by Guatemala and El Salvador for the Honduran market and they soon flooded the markets of Costa Rica and Nicaragua. Costa Rica saw this as such a threat to its economy that it announced trade controls and quotas to check the flow of intra-regional imports; the remaining three members of the market, El Salvador, Guatemala and Nicaragua, then closed their borders to Costa Rican goods. Fortunately, these trade restrictions were soon lifted and by 1972 intra-regional trade began to recover.

The monograph shows that the two countries which benefited most from the market's trade expansion, El Salvador and Guatemala, also gained the most from the market's industrial growth, since new investors found those countries most attractive for the establishment of new industry. Industrial investment in El Salvador and Guatemala was four times that of Honduras and twice that of Nicaragua. As a result, Honduras has threatened to withdraw from the market completely.

Professors Holbrik and Swan conclude that Costa Rica, Honduras, and Nicaragua are better off economically than they would have been if there had been no common market at all, but seeing their more prosperous partners grow relatively even more prosperous became politically intolerable for them.

The more obvious means of improving the relative positions of the weaker members of the market are mentioned in this monograph—channeling new investment to the less developed members and restricting their imports from Guatemala and El Salvador. These would no doubt slow the growth of the market as a whole, but that may be the price that must be paid to save the Central American Common Market from total disintegration.

This study should be read as a supplement to the earlier monographs of Carlos Castillo, David E. Ramsett, and Andrew B. Wardlaw which are cited in its excellent bibliography. As a means of bringing those works up to date, this publication is highly successful indeed.

MELVILLE E. OSBORNE
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The Green Flag: The Turbulent History of the Irish National Movement. By Robert Kee. (New York: Delacorte Press, 1972. Pp. 877. $15.00.)

This is a large and impressive addition to the growing shelf of Irish narrative history. As an up-to-date and detailed account of Irish political events and personalities from the late eighteenth century to the Treaty of 1921 it could properly be described as thorough; Mr. Kee, a television journalist, has read all the published work and has delved into the more important manuscript sources. But Mr. Kee's original subtitle, "a history of Irish nationalism" (modified in this American edition to "the turbulent history of the Irish national movement") seems to offer something more than narrative for its own sake. It appears to imply a line of interpretation, an ordering of historical data, which is not achieved.

Irish nationalism, by a curious and perhaps unique blend of militant minority insurgency and mass democratic development, within a context of massive social change and unshaken religious dominance, managed to create a modern, stable—if incomplete—constitutional state system in the early twentieth century. Ireland might be considered a deviant case in the confused and frequently unsuccessful processes of contemporary and near-contemporary nation- and state-building. The contradiction between the resort to arms manipulated by the few and the creation of a regime representative of the many; the successful adaptation of a party-based parliamentary model so closely associated with the alien occupier; the capacity to contain the threats posed by the demobilization of a large revolutionary army, by the continued existence of a disputed political border, and by relative economic underdevelopment—all of this requires explanation.

The exhaustive detail presented in Mr. Kee's account of the historical background offers no coherent analysis. In part it fails because the emphasis on political events obscures rather than illuminates the underlying social developments that shaped modern Ireland. Ireland jumped from prefeudal fragmentation—still, in Binchy's famous phrase, "Gaelic, rural, hierarchic and familiar"—to become a recognizable west European bourgeois society in the course of the nineteenth century. The great Famine marked the traumatic transition between those two Irelands: it might be regarded as the birth-pangs of modern Ireland, but it is given only a few short pages in this large volume. In measure, too, *The Green Flag* fails because of the unexplained choice of end-date.

No doubt the Anglo-Irish Treaty of 1921 was a turning point in Irish historical, political and social development. It ended the era of the Union. It introduced for the first time a formal internal political boundary. But it neither terminated the history of Irish nationalism nor concluded the Irish national movement. Indeed, it was precisely at this point (through the Treaty debate, the negotiation and debate on the Irish Free State Constitution, the creation of two separate and hostile administrative systems) that the inconsistencies, incoherencies, unexamined assumptions and unresolved contradictions implicit in that vague notion of Irish nationalism began to be explored in practice. In choosing to stop short at 1921 Mr. Kee deprives us of his very considerable capacity to construct a vigorous and detailed narrative and greatly reduces the explanatory value of his book.

Even judged solely as a contribution to historical synthesis, the book, for all Mr. Kee's skill as a writer, is disappointing. Certainly it gives a very full account of the surface changes in public life and the underground revolutionary movements, but as the franchise extended through the nineteenth century it becomes necessary to document the changes in popular Irish attitudes and values. Apart from a brief chapter on "the growth of national consciousness" (pp. 426–437) little is attempted here. Yet already emigration and internal migration were shaping the contours of a new population. Although the introduction of mass literacy (in English) and competitive public examinations were both formidable moulders of Irish society there is no discussion of educational developments (the topic does not even rate inclusion in the index). Even more significant is the downgrading of the series of Lands Acts, 1881–1909, often regarded as "the real Irish revolution," of which only three are touched upon in *The Green Flag.*

There is a great deal more to the history of modern Ireland than the chronicling of political and military events. This book, despite its size, is severely limited in its compass. It does not compare in scale or comprehension with F. S. L. Lyons, *Ireland since the Famine: 1850 to the Present,* revised edition (London: Collins/Fontana, 1973).

BRIAN FARRELL

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Government and Politics of Korea. Edited by Se-Jin Kim and Chang-Hyun Cho. (Silver Springs, Maryland: The Research Institute on Korean Affairs, 1972. Pp. 331. $9.25.)

A book by twelve authors almost automatically limits its merits: there is unlikely to be a sustained theme; nor is originality or profundity encouraged. Where, as here, there is no elaborate summing-up chapter, careful integration and comparisons among contributions come hard.

The present volume's merits are limited in these ways, yet it has much to be said for it. The authors—all Korean-American scholars—were well chosen, and their apparent obedience to careful instructions has produced a needed work covering the gamut of politics and government in South and North Korea which is, almost without exception, workmanlike and candid, with more unity and thoroughness and less overlap than predictable. Korea has been moving fast in the many months since the book's birth: new constitutions in both Koreas and some key policy changes in the South have already dated parts of some chapters. With this exception, the book can be recommended as a good and convenient reference source for those who want a moderately detailed introduction to politics and government in the Korean peninsula since World War II.

Dr. Sung-Chul Yang's thoughtful and well-organized survey of the place and roles of political ideology in South Korea would have been still more valuable if it could have included North Korea, where the role of ideology is far greater than in the South (or almost anywhere else). Dr. Sung-Joo Han provides a reasonably complete and admirably fair-minded capsule of the neglected subject of political dissent in South Korea which, more unfortunately for us than for him, omits the key period of vigorous leftist influence from 1945–48. His awareness of the difficulty in defining what is leftist and rightist in a country where the government owns not only the means of production but almost everything else except real estate and retailing, does not suffice, in the short space available, to solve this key definitional and communication problem.

Dr. Se-Jin Kim's chapter on the formal set-up and functions of government in South Korea was thorough and just until the unforeseen devastations of the new post–October 17, 1973 constitution outdated it. Dr. Chang-Hyun Cho, in one of the book's best chapters, gives a candid and hard-hitting account of South Korea's extraordinary centralism and recalcitrance toward allowing any local self-government. Dr. Cho, reaching similar conclusions but necessarily with less intimate depth, does much the same job on North Korea in a later, separate chapter. The need for more explicit comparative treatment so blatant here is unmet by the book's structure. One harbors the suspicion that, between these two rigid centralisms, that of North Korea may allow for a slightly better local-central communication system than South Korea's now permits—a conclusion far too important to leave suspended as a mere suspicion.

Dr. Y. C. Han's chapter on "Political Parties and Elections in South Korea" is a little weaker, has less theoretical structure than Dr. Han could have availed himself of, and fails to stress the desiccating effect on parties and politics of declining legislative and party roles under the present Seoul regime. Han's conclusion that party development in the South is unsatisfactory and its prospects discouraging is hard to deny. Dr. Sung-Hee Kim's "Economic Development of South Korea" is a good, workmanlike but slightly cut-and-dried report. If "orderly and peaceful transfer of political power is prerequisite to uninterrupted economic development in the 1970's," then South Korea is clearly already in more potential economic trouble than I suspect she in fact may have. This piece is balanced—again without sufficient comparison—with a good chapter by Joseph Sang-Hoon Chung on "Economic Development of North Korea"—as centralized—and even more planned—an economic system as South Korea's.

Chin O. Chung's "The Government and Power Structure of North Korea" and Benjamin Min's "North Korea's Foreign Policy" are both adequate coverages made somewhat dated by recent developments. Dr. Min's piece is a little less forceful and underplays the importance for North Korea of foreign investment and trade needs; it would have been improved by a listing with dates of countries recognizing or having relations with North Korea, a list which has recently swelled impressively. Professor Byung-Chul Koh's "North Korea's Unification Strategy: An Assessment" is good analysis of a more probing character than usual in this volume. North Korea's campaign for unification, however, has yielded more in terms of foreign recognition and probable U.N. equality than Dr. Koh then estimated it would.

Chong-Han Kim's "The Korean Unification Issue in the United Nations," while highly conscientious, is marred by the fact that Dr. Kim—perhaps through the vagaries of editorial communication—takes no regard of the unification initiatives of the last two years, including those well before the volume's publication. This chapter is therefore the most dated of the book.

The most unsatisfactory aspect of the work of this team of Korean-born scholars is the uniform lack of interpretive depth which they bring to their own pre-modern culture. The first chapter, "Historical Perspective on Korean Politics," is the poorest, crammed as it is with the conventional wisdom about Confucian authoritarianism and unrelieved by sensitivity to the existence of an older egalitarian and counciliar Korean culture, probably rooted in premodern nomadic patterns, which has always existed just under the most authoritarian surface, as the findings of Dr. Vincent Brandt and others have shown. Similar faults echo in Dr. Se-Jin Kim's chapter, while from Dr. Sung-Chul Yang's statement that "Confucianism . . . may be sociologically but not politically significant since it has not directly served elite publics as a device for their political manipulation" (p. 26), we can only draw back in open-mouthed incredulity.

The organized knowledge and sound judgment of these twelve authors of the modern politics and government of both Koreas, however, far overrides any shortcomings of this useful volume.

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Korea: An Analytical Guide to Bibliographies. Edited by Hesung Chun Koh and Joan Steffens. (New Haven: Human Relations Area Files Press, 1971. Pp. 334. $12.00.)

According to the editors, this book was prepared "primarily for scholars who are working on social science research to aid them in finding bibliographic reference to a wide range of topics, both general and specific, which are related to Korean society and culture" (p. xi). This useful volume is the product of a computerized bibliographic system developed and refined by the senior editor (Hesung Chun Koh), a sociologist, under the auspices of the Human Relations Area Files. In addition to being the most comprehensive collection of bibliographies on Korea published in one volume to date (containing about 400 annotated items), it is also a superb example of how bibliographic material can be effectively collected, organized, stored, manipulated, and retrieved by using the computer.

The HRAF Automated Bibliographic System (HABS) used in this book provides all conceivable ways of classifying the resource data, making it possible for each researcher to obtain a tailor-made bibliography for his topic. It even offers categories "specific to Korean culture" such as "Japanese occupation" and "Korean war." The researcher will find this device very useful. This book contains bibliographic data on Korea in eight different languages including Chinese, Czech, English, French, German, Japanese, Korean and Russian. Thus, the collection gives the researcher access to sources of much variety and depth. The guide is written concisely and free of computer language, making it easy to understand how the system works.

The usefulness of this book will depend upon the researcher's subject and the quality of bibliographies. Those whose interest is in historical subjects, will find that this volume can save them much time and confusion in their search for research material. Those interested in contemporary subjects, however, will find its utility limited because of the inevitable time lag between the date of a publication and its inclusion in bibliographies. Another difficulty for the users of this book will be that, with a few exceptions, the entries are accompanied by only ordinary library information such as title, date and place of publication, number of pages, etc. A more informative description of the *substance* of each item will be very helpful for the users. Future volumes, one hopes, will not only add new entries to the existing ones, but also expand the description of all entries.

The primary value of this book for the political scientist is not so much in what it offers now as in what it promises to offer in the future by demonstrating the possibility of an effective and efficient way of handling bibliographic data. The scheme introduced in this book will prove to be useful to not only Korean studies but also many other areas and subjects. The editors should be commended for their painstaking, imaginative, and pioneering effort.

SUNGJOO HAN
Brooklyn College

The Organs of Soviet Administration of Justice: Their History and Operation. By Samuel Kucherov. Foreword by John N. Hazard. (Leiden: E. J. Brill, 1970. Pp. xxiv, 754. $30.00.)

The death of Samuel Kucherov in 1972, in his 75th year, deprived comparative lawyers of a highly esteemed colleague whose unique legal experience spanned both the Tsarist and Soviet periods. The legal profession and the administration of justice under both regimes were his special interest, and his *Courts, Lawyers and Trials Under the Last Three Tsars* (1953) remains the standard work in English. The present study carries his earlier volume up to the present and, like its predecessor, is an original, refreshingly opinionated, and valuable legacy.

What Kucherov has attempted is a veritable history of the Soviet court system and the personnel who operate it. About a third of the book is devoted to the early regular and extraordinary (Cheka, etc.) tribunals, to the various reforms in their structure and operation from 1922 to 1958, to the institutions employed to settle disputes among state enterprises, to comrades' courts, and— a curious digression—to the anti-parasite laws. Fully half of the materials treat "men of law": legal education; judges; people's assessors; investigative organs; the procuracy; advocates; jurisconsults; and social accusers. The concluding chapters examine "justice in operation," by which is meant the factors influencing the determination of guilt or assignment of sentence (revolutionary law consciousness, theories of "truth," value of confessions, use of analogy, role of public opinion), procedures for review of court judgments by appeal or cassation, and the amorphous notions of revolutionary and socialist legality.

Kucherov's thesis is simplicity itself. In his view the "Soviet state assumed from its very beginning a totalitarian character which it has retained up to the present time . . . As a result, a yawning abyss between the written law and its implementation was created, and flagrant violations of legality take place whenever the Party chooses to interfere with the administration of justice." Now, however, Kucherov perceives a "process of liberalization"; the pendulum of history has begun "to move in the opposite direction. How fast and how far it will go, nobody knows" (pp. xvii–xviii). Much attention is given over in this connection to the trials of Soviet dissidents in the 1960s, and the author quotes at length from published accounts of these proceedings.

However much one may deplore the excesses of Soviet political life, the comparative legal historian ultimately must decide whether these events are most deserving of treatment amongst the multitude of developments in Soviet legal life to the virtual exclusion of all others. As one who felt keenly and personally the course of Soviet developments in and after 1917, Dr. Kucherov can be forgiven for answering that question affirmatively. But those interested in a nonpartisan and accurate account of the "history and operation" of Soviet agencies administering justice are unlikely to be satisfied either by the questions Kucherov poses or by his processing of the materials. Scholars who do undertake a proper and much needed history of this era nonetheless will derive much of value from Dr. Kucherov's efforts. He reminds us constantly of the links modern Soviet institutions have with their predecessors, and he has utilized a substantial corpus of Soviet legal writings, particularly from the 1930s, which up to now have been largely ignored.

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Australia in New Guinea, second edition. By L. B. Mair. (Melbourne: Melbourne University Press, 1970; U.S. Agents—International Scholarly Book Services Inc., P.O. Box 4347, Portland, Oregon 97208. Pp. 254. $8.80.)

Dr. Lucy Mair is well known for her work on African societies and governments. She was invited in the later years of World War II to join the Landforces Headquarters School of Civil Affairs, a new venture of the Australian Army established alongside the Military College at Duntroon, Canberra. The School was established under the control of the L.H.Q. Research Unit, a group of intellectuals recruited into the Army as perhaps the first Australian "think tank." It prepared young officers to work in the territories of Papua and New Guinea, where most of the Australian armed forces had been engaged by the time the war ended. The invitation to Dr. Mair was well justified. Her knowledge and experience of African situations helped to establish intellectual resources to prepare the postwar generation of Australian colonial administrators.

This administrative-social history of Papua and New Guinea until just after World War II was first published in 1948 and updated after a visit in 1968. Up until the war, there had been two administrations—of Papua (as a territory of Australia), and of the mandated territory of New Guinea. Papuan policies had been paternally protective, or at least comparatively so by the standards of the time, in laws about labor and in concern for "native welfare" generally. The administration of New Guinea, on the other hand, exemplified the principles of mandate government far less than did that of Papua. The bureaucracy in Canberra saw land and people of the mandated territory mainly as economic assets. It had been governed by an army unit when it was taken from the Germans in 1914–21 and the people had been subjected to tougher labor laws and taxation systems. The Japanese in the invasion of 1942 had overrun New Guinea and entered Papua. As these areas were reoccupied by the Australian army, they were administered by the Australian New Guinea Administrative Unit (ANGAU). Joint government of the two territories was continued after the war in the face of opposition from the Trusteeship Council and has remained ever since.

These events formed the background of the first edition of this book. Two decades after its publication in 1948 Dr. Mair returned to a very different Australia and spent some weeks in a territory of Papua and New Guinea which was very different indeed. In 1948 few would believe that 20 years would see the end of the colonial system as an internationally acceptable arrangement for the government of tropical areas; fewer foresee the nature of postindependence problems in a Third World. In Papua and New Guinea (now Papua New Guinea, bound for self-government in 1973 and independence in 1974) political and social changes were accelerating. Four years before there had been elections on a common roll, with adult franchise, to the House of Assembly. Very steep increases in investment and increasing government grants had produced the classic problems which follow the impact of industrial goods and cash on a subsistence economy in a largely preliterate society. These changes made the work of updating the book extremely difficult.

In this edition, the early historical chapters remain much as they were. Others deal respectively with central government, indigenous affairs (what, in a colony, is not?), local government, economic development, land policy, with three chapters on labor policy and administration, and one on welfare services. In her effort at updating, Dr. Mair had to rely on the official reports, so often written as much to justify and conceal as to inform, on her own brief visits, press reports, legislation, parliamentary debates, and what she could gather from her old students. The result is a concise and useful reference text, which does not purport to analyze social and political change so much as to describe policies and administrative measures.

Since it is rather important I will mention here the only error I could find. On page 56 Dr. Mair quotes me as saying or implying that executive policy in 1969 was "that of the departments in Canberra to which civil servants are responsible" and remarks that such controls did not apply to District Administration. What I said was that a single department of government in Canberra, and a single minister, exerted control, under the Papua and New Guinea Act, over all the departments of the Papua and New Guinea administration, because this federal minister-department complex directly controlled the Papua New Guinea executive. The minister (and his department) also controlled the local civil service authority. District administrators were tied through the Port Moresby executive so inflexibly to the Canberra "law and order" policy that there were not, until too late, moves for political bargaining with local protests. But all this happened after Dr. Mair's last visit.

This second edition brings the story of Australian colonial administration into the period of the real politics of decolonization. It will have a permanent value for students of the politics and history of the Papua New Guinea nation.

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Vietnamese Anticolonialism: 1885–1925. By David G. Marr. (Berkeley: University of California Press, 1971. Pp. 322. $12.50 cloth, $3.65 paper.)

This book sets a new and higher standard for Western scholarship on Vietnam's modern political history. In addition to drawing on recently available French archival material and pertinent Chinese and Japanese sources, Professor Marr makes effective use of a substantial body of Vietnamese sources that have been largely untapped by other Western writers. Covering a period of Vietnamese history that has been little understood in the West, he analyzes the continuum of Vietnamese resistance during the period that began in 1885 with Emperor Ham Nghi's belated decision to fight the French and ended in the mid-1920s when Vietnamese nationalism had begun to develop a broader political base spanning a wide range of societal divisions. Marr gives more attention to the cultural and intellectual side of modern Vietnamese nationalism than to its socioeconomic context; but he does not overlook the latter and provides a convincing critique of some of Jean Chesneaux's ideas concerning class structure in the colonial period.

Beginning with a concise historical introduction in which he discusses the nature of Vietnamese identity as it evolved over some 2,000 years prior to the advent of the last imperial dynasty, the Nguyen, Marr provides some striking evidence about why they were even less able than their counterpart in China (the Ching) to cope with the impingement of Western power. He then focuses on the first phase in the struggle to throw off colonial rule, one dominated by anticollaborationist elements of the scholar-gentry who rallied around the Emperor and continued to draw inspiration from his abortive three-year resistance long after the French captured him.

Most of the remainder of the book, well over half, is devoted to its major theme—the subsequent reaction of the residual scholar-gentry to French rule. While a majority of this Confucian-oriented mandarinate elected to serve as collaborators, an anticolonial minority—the principal focus of the study—sought a path that would lead away from what they feared was a growing loss of national identity and towards a reassertion of Vietnamese control over their own country and culture. The noncollaborating minority, however, was always divided. Apart from hatred of French rule, their only substantial common denominators were opposition to Confucianism, to the vestigial French-manipulated imperial court, and to the collaborationist majority of the surviving mandarinate that still identified with that system.

The two principal streams of oppositionist ideology were represented by Phan Boi Chau, the paramount activist, and Phan Chu Trinh, the most influential reformist. These two protagonists dominate much of Marr's detailed account of the period 1900 to 1925, their political careers and evolving political philosophies interwoven with the main strands of its political history. Familiarity with the ideas they came to personify is important not only for an appreciation of the character of the anticolonial movement during these years; it is also essential, as Marr argues, for an understanding of the decisions of later anticolonial leaders as to how best to wage the nationalist struggle. Indeed, the subsequent stage of that struggle, culminating with Ho Chi Minh's Vietminh, could be said to represent something of a fusion of the two currents.

While strongly attracted to Montesquieu and Rousseau, Phan Chu Trinh maintained a healthy respect for French power and never moved on to accept Robespierre or Danton. His vehement rejection of Confucianism did not entail an abandonment of elitism and was unaccompanied by faith in the capacity of Vietnamese peasants to respond to the changing economic and social environment with initiatives of their own. Phan Chu Trinh was convinced that a period of French tutelage was absolutely necessary for the modernization Vietnamese society required if enough power was ultimately to be marshalled to oust French colonial rule. Rejecting violence, he and his followers made the fatal assumption that French promises to modernize Vietnam could be trusted, and they waited in vain for the arrival of a French Governor-General willing and able to carry them out.

For two decades, until his arrest in 1925, Phan Boi Chau was the pre-eminent leader of the postimperial phase of Vietnamese nationalism and the leading proponent of active, forceful resistance to French rule. As with Trinh, though trained as a mandarin, he uncompromisingly rejected both the vestigial court and Confucianism. During his long political career he moved across the spectrum from absolute monarchism through constitutional monarchism and bourgeois republicanism, finally becoming attracted to a vaguely defined socialism; but he was never able to work out the tactics and strategies necessary for successful resistance to French rule. Like Trinh, he early perceived the necessity for understanding Western learning in order to advance Vietnamese national interests; but unlike him he had no expectation that France would ever provide a more beneficent colonial rule, and he was consistent in perceiving the French as implacable opponents of Vietnamese independence.

In his quest for politically useful modern ideas and access to anti-French allies, Phan Boi Chau spent considerable time in Japan, where his thinking was strongly affected by the eminent Chinese emigrée nationalists he met there, in particular Liang Ch'i-ch'ao, of whom he saw much, and Sun Yat Sen, whose influence on him was subsequently greatest. In 1920 he had read a Japanese study of Russian communism which in his enthusiasm he translated, and by 1924 he was writing in favor of a social revolution that would involve workers and peasants and be led by a politically conscious elite. In 1924 he arranged for Vietnamese to be trained in Canton at the Kuomintang's Whampoa military academy and founded a new political party, the Vietnam Quoc Dan Dang (Vietnam National People's Party). An almost exact copy of the KMT, this organization came to play an important role in the years immediately after his capture by French agents in Shanghai in 1925 when his own career as a political activist was brought to a close.

Phan Boi Chau and Phan Chu Trinh failed to attain their goals. After Phan Chu Trinh's death his ideas were taken over and warped by the collaborationists to rationalize their own self-serving opportunism; but Phan Boi Chau emerged as a major symbol of sustained anticolonial resistance, and after he died came to occupy a position in the pantheon of modern Vietnamese nationalism somewhat analogous to that of Sun Yat Sen in China.

By the time one has read this book it is not difficult to agree with the author's conclusion that given the alternatives available and the history of the previous few decades, the decision of Nguyen Ai Quoc (Ho Chi Minh) to participate in the foundation of the French Communist Party and join in the Comintern's proclaimed assault on imperialism was almost inevitable.

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Cultural Engineering and Nation-Building in East Africa. By Ali A. Mazrui. (Evanston, Ill.: Northwestern University Press, 1972. Pp. xviii, 301. $10.00.)

Ali Mazrui, Professor of Political Science at Makerere University, and at this writing a Fellow of the Center for Advanced Study in the Behavioral Sciences, is accustomed to attacks. In East Africa he is frequently and severely criticized by radical social analysts who find his traditional scholarship irrelevant and his liberal principles infuriating. Conversely, some mainstream political scientists feel that his work, of which there is now a substantial body, is lacking in analytical rigor, and that no amount of graceful erudition can disguise the fact that Mazrui is more a political essayist than a political scientist. This book will provide considerable ammunition to both sets of critics. Equally, it provides useful and provocative reading for a middle group who welcome clever, literate, and integrative discussion of East African personalities, ideas, and events.

The work is centered on how cultural factors such as language, literature and marriage customs are politically important in Kenya, Uganda, and Tanzania, and how these and other cultural issues can be and have been manipulated by East African regimes to overcome short-term crises and create long-term unified stability. Sweeping though it may be, the above summary is insufficiently general; Mazrui's limitless interest leads him to brief discussions of economic affairs and electoral structures, detours to deal with Ghanaian and Nigerian events, discussions on the African military as governors, disgressions to political philosophy, and comments on what he finds fascinating in the East African daily press.

The volume is much more than a set of unrelated ideas and essays, but in spite of a determined effort to tie each chapter to the core construct, the result is not a neat, coherent whole. Part of the reason for the lack of unity stems from the creative eclecticism of Mazrui's mind; he cannot resist a fascinating side-issue. Less forgivable is the repetition of certain themes, suggesting that some chapters are rewrites of earlier papers, changed minimally and made to fit the structure of a book. This repetition especially shows through in the numerous discussions of Ugandan issues, some of which mention the Amin coup of 1971 and place events of the previous Obote regime in the past tense, while at other places pre-coup policies are treated as though they were still in effect.

The overall weakness of the book can best be illustrated by a specific example. Discussing the results of the revolution on the island of Zanzibar, Professor Mazrui writes:

> The result has not been a transformation of the political system as such but a devastation of the polity, a dislocation of the economy of the country, a general sense of insecurity in the population, a lack of sense of direction in national policies, and a rapid erosion of human values. . . . Revolutions may be best carried out for the sake of the masses, but they are not necessarily best initiated by the masses (p. 206).

Radicals will dispute the truth of the allegations, citing what they regard as the Zanzibari regimes' economic and social achievements to support their counterarguments. They will also be confirmed in their assumption that Professor Mazrui is a timid bourgeois intellectual, protective of the present and its amenities, fearful of fundamental change. Professional political scientists will ask Mazrui for precise definitions of a "devastated polity," a "dislocated economy," a "generally insecure population," etc.; and they will want to know not only what does he mean, but how does he know? The volume will not provide them with any solid evidence.

The volume puts forth grand generalizations based on very limited instances (pp. 118–119, p. 187), offers insights and proverbs in the name of hypotheses (p. 263), relentlessly coins new phrases (some of which work; e.g., "lumpenmilitäriat"—". . . that class of semiorganized and semiliterate soldiers which has begun to claim a share of power and influence in what would otherwise have become a heavily privileged meritocracy of the educated" [p. 263]), and in general wanders about a very large area while failing to analyze in great detail any specific part therein. It is not first-rate political *science*.

It is nonetheless a veritable factory of fascinating ideas and assertions and integrations, all of which demonstrate the inventiveness of Mazrui's literate talent. One's annoyance with the glib generalization, the diffuseness of approach, or the failure to support some contentious remark is again and again counteracted by the sweep of the language and the provocativeness of the content. Thus, of Nyerere's experiment in Tanzania, Mazrui writes: "The real danger posed by state socialism in a society with fragile institutions is not the danger of making the government too strong but the risk of making the government more conspicuously ineffectual" (p. 282). In chapter 11—"Political Control and Military Power"—he offers an excellent discussion of the differing manner of civil/military relations in the three East African states from independence to 1971. This chapter, especially, will make fine reading for courses in African politics. The ultimate point is that the book is so stimulating as to make one ignore its methodological shortcomings; it makes very worthwhile reading. I hope that eventually Professor Mazrui will focus more narrowly his extremely impressive knowledge and ability and write the major work of which he is so clearly capable.

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The Church as a Political Factor in Latin America: With Special Reference to Colombia and Chile. By David E. Mutchler. (New York: Praeger Publishers, 1971. Pp. 460. $18.50.)

This book is an attempt to describe and explain the current state of the Latin American Church. According to Mutchler, the Church has undergone a rapid process of disintegration in recent years and now exists, if at all, in a state of collapse. This process of dramatic breakdown is viewed as the result of dependency ties that developed between the Church's newly established staff agencies and foreign centers of financial support. Linkages with foreign hierarchies, consummated to relieve shortages of funds and personnel, increased the power of the clerical experts over line authority, thereby subverting episcopal control. Subsequent financial dealings with agencies of foreign governments generated vicious political divisions among the clerical experts, destroying already fragile bases of cooperation and cohesion. Today, says Mutchler, the Church is little more than a pile of ecclesiastical wreckage.

In the course of shaping this thesis, Mutchler provides details about encounters between national hierarchies, sequences of organizational change, and tensions that arose among clerical experts. In reporting these materials, Mutchler adopts a particularistic detection and disclosure policy. Excerpts from personnel letters, financial reports, and other documents (many of which Mutchler identifies as "confidential") are published with the names of the persons intact. Throughout the chapters, these particularized materials are juxtaposed in ways that, in my judgment, place some of the named individuals under evaluation. Other readers may feel, as I do, that ad hominem considerations frequently guide the author's hand. Mutchler is unclear about the conditions under which he gained access to these documents, the extent to which all of his Latin American hosts understood his intentions regarding publication, and his reasons for identifying these individuals and holding them up for public view. Men who have contributed to the development of sociology and empirical research in Latin America and who have been, I submit, especially gracious in their responses to many North Americans' inquiries about the Church, are needlessly exhibited. Informal assessments of Mutchler's policy and tactics assure me that several of these men feel victimized, if not betrayed. There seems to be no other conclusion than that breaches of confidence and trust have occurred.

Mutchler's policy violates professional principles of confidentiality in communicating field data. It involves an unwarranted appropriation of personal identities. It also casts doubt on the author's manifest sociological intentions. No less important, it detracts from, if not buries, the book's potential theoretical import. Structural patterns and organizational relationships that could have been explicated and analyzed sociologically are curiously bypassed in favor of personalized reporting. By allowing particularism to prevail over anonymous sociological inquiry, Mutchler risks the possibility that he will be read as a petty inside dopester rather than as a potential scholar.

As to the intellectual and methodological merits of the book, several issues warrant discussion. First among these is the author's generalization about the disintegration of the Church. In my judgment this is an exaggerated and misleading characterization of the sociological changes under way. Although Mutchler succeeds in showing that priests compete with each other and engage in politically related entrepreneurial activities, he does not come to grips with the distinctive features of the Church as a multilevel, transnational system, its changing bases of integration at each level, and the ways in which conflicts in local situations are relieved, if not resolved, at higher levels of authority and symbolization. He assumes that short-run conflicts among staff experts are valid bases for positing the disintegration of the whole system. The staff agencies he observes, however, are neither microcosms of the whole Church, nor representative samples of the Church's diversified organizational activities. Should one want to study the extent of disintegration in the Church, then I think it would be necessary to attend to three things: (1) the extent to which the members' conceptions of salvation and religious achievement are viewed as dependent on the sacramental authority of the priest; (2) the extent to which the policies, practices, and pronouncements of the hierarchy (whether at the local, national, or international level) effectively symbolize and meaningfully represent the Catholic-Christian "tradition"; and (3) the extent to which the recent trends toward diversity, pluralism, and differentiation within the elite structures of world Catholicism can be given boundedness and direction by the transnational center in Rome. Some of the Church's organizational efforts that led to the tensions Mutchler writes about are part of a wider process that has to do with Catholicism's realignment in the Western system and with the international system more generally. Unless this more global evolutionary problem is studied, including the implications of a growing polycentrism in the Communist bloc, the general integrative problem of the Church remains untouched.

Thus it is to be expected that disorganizing processes are under way in the Latin American Church, but there are also intimations of important reorganizing and reintegrating processes, as Mutchler himself observes: "At the end of the 1960's [the supposed high point of disintegration] there were clear signs that Latin American bishops were operating across national boundaries, linked by common concerns, affected each in his own diocese by what was being said and done by other bishops in other nations" (pp. 129–130). He refers to these developments as an "integration" of the Catholic system "at the top," which in a transnational norm-creating and value-symbolizing organization is by no means insignificant, particularly when the bishops possess a formal, continent-wide structure (The Latin American Bishops' Council, CELAM) through which common concerns and policy directives can be expressed. These integrative developments at the top are paralleled by several other important processes: a growth of conciliar bodies in dioceses and parishes; an increase in the number and types of interdiocesan and intermovement activities; and a concerted shift in pastoral work that is attempting to relate the liturgy to natural communities and popular religiosity. These slow, but nevertheless significant, changes suggest the emergence of a new integrative paradigm that, expectedly, undercuts the traditional pattern of local, territorially-organized segments. Because Mutchler skirts these broader aspects of the Church's collective life and thereby the higher dialectics of disorganization and reintegration, it is difficult to take his arguments seriously.

The book also struggles to find a basic argument about the Church as a "political factor," which by the title should have a central place in the discussion. This occurs, I think, because Mutchler takes as his dependent variable the "state of the Church's health" rather than national political change, political instability, etc. In short, he avoids bringing his observations about changes in the Church into an explicit discussion of their political implications. This is unfortunate because one of the most intriguing questions, and certainly a significant one, is how a "disintegrated" Church affects central political processes and the nature of civic life. Is a disintegrated Church a positive or negative force in political change? If the Church collapses in Latin America, doesn't this remove what has been one of the major obstacles to political change? Or is a disintegrated Church another name for a politically polarized Church, with each faction retrogressively aligned with opposing, short-run political forces? If this latter interpretation is correct, then surely a disintegrated Church carries tremendous implications for Latin America, because religiously anchored political cleavages obstruct nation building and political compromise. Mutchler worries so much about the ways in which foreign political influences (read U.S.A.) presumably destroyed the Church that he never gets back to the aim that he sets for himself in the first paragraph of the book, namely to examine "the critical impact of the Catholic Church" on the developmental processes in Latin America. The telling clue to this oversight is the fact that he ends his book with a chapter entitled "A Model of Church Disintegration."

The book's logic of causal explanation is also unsatisfactory. Church disintegration is supposed to be caused by foreign economic dependency, but this is not validly demonstrated because a single cause is being asserted without holding constant other possible sources of variation. Because certain effects are observed in both Colombia and Chile, and both churches had "foreign linkages," the conclusion is drawn that the latter are responsible for disintegration. But foreign linkages have to be placed against other factors, such as international theological currents, oppositions among priests about their role in political life, contradictory pressures from organized sectors of the laity, and the historically conditioned autonomy that many priests possess vis-à-vis their bishops. Mutchler had at least two choices regarding comparative design, either of which could have helped him. First, he could have chosen one case of dissolution and another case exemplifying cohesion, taking care to select these cases to maximize similarities of an internal and contextual nature, except for the crucial independent variable—degrees of linkage with foreign money centers. If the case marked dissolution was also the one embedded in foreign financial structures, he would have gained a tentative, but plausible, basis for asserting his explanation. Or, second, he could have chosen a series of national or subnational cases that varied in degrees of internal breakdown and then examined the level of association between degrees of breakdown and degrees of foreign financial involvement. He chose neither of these designs, preferring instead to rest his explanation on a weak version of the method of agreement.

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The Politics of Afghanistan. By Richard S. Newell. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 236. $9.50.)

This work is one in a series on South Asian political systems, published under the general editorship of Richard L. Park, which addresses itself to contemporary political problems in the context of traditional institutions and value systems. The book is well written and provides an excellent analysis of the crucial political, economic, and social factors in contemporary Afghanistan. Like the other studies in the series, this one is organized under five headings; Physical and Social Setting, Historical Setting: The Struggle for Unity and Independence, Government: Structure and Functions, The Economy: Internal Growth and External Aid, Recent Political Development, 1964–1971. The author gives a brief discussion of Sources and Bibliography and Suggestions for Further Reading.

On the topic of physical and social setting, Newell describes the land, the climate, agricultural and other resources, location and communication, peoples, ethnic groups, social characteristics, Islam in Afghanistan, traditional patterns of power and wealth, and class relations. Because of the scope of his work Newell is satisfied with giving us only short outlines which are of interest primarily for the general reader who has no prior acquaintance with the area. The same can be said for the second chapter which gives a brief sketch of the historical setting. It is in this section that Newell uses a somewhat unconventional system of transliteration, as for example "Shere Ali" for "Shir Ali," "chaudury" for "chatri," and "Bacha-i-Sachao" for "Bacha-i-Saqqau." There are also two or three minor errors in this section; "Khushlal Khan" should read Khushhal Khan; the Russo-Afghan Friendship Treaty should be dated 1921 rather than 1919; the blood feud referred to (page 61) was between the Charkhi and the royal family, not between "the royal family and another prominent Mohammedzai family."

In chapter 3 the author skillfully analyzes the structure and function of the Afghan government. He examines the position of the royal family in the context of internal Afghan forces, the military forces as a power factor in Afghanistan, and the influences of class interests represented by the major landed and commercial interests, the religious leadership, tribal authority, and the educated and professional class. The author then examines the contemporary structure of the Afghan government and the interaction of factors affecting administrative performance. Finally, the author examines the Afghan constitution of 1964 outlining the ideals embodied in it, representing essentially democratic and equalitarian values, and the attempt to implement them. It is an irony of parliamentary democracy in Middle Eastern countries that the granting of universal suffrage to the people, as experienced for example in Turkey and observed by Newell in Afghanistan, leads to the establishment of a representative government which is largely conservative and frequently hostile to the innovations promoted by the native modernized intelligentsia.

The chapter on the Afghan economy, equally well executed by the author, examines the growth of the Afghan economy and the stimulation and distortion to the economy caused by foreign aid to Afghanistan. Newell discusses the period of competitive foreign aid, largely extended by the Soviet Union and the United States, and the accomplishments in Afghanistan during two development plans (1957 to 1967). It may be added here that it is *not* the first time in history, as maintained by Newell, that "Russians were able to achieve substantial economic and political involvement in Afghanistan." The Soviet Union was heavily involved in Afghanistan throughout the Amanullah period (1919–1929) and was in fact predominant in the Afghan Air Force. Newell shows that projects undertaken with foreign sponsorship, badly needed as they were, produced strains and imbalances in the Afghan economy, necessitating measures for economic integration.

In chapter 5 Newell examines recent political development from 1964 to 1971. Newell outlines the events leading up to the constitutional crisis of 1965. A fundamental problem is the relationship between cabinet and parliament and the tendency toward rivalry and obstructionism as the representative body flexes its political muscle and sees its primary task in challenging the executive branch of government. Newell's predictions are cautiously optimistic, but in view of recent events in Afghanistan, he was wise not to make far-reaching predictions.

Newell has produced a superb analysis which I would highly recommend to anyone interested in Afghanistan or in the problem of socioeconomic and political change from a traditional Islamic to a modern political system.

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Survey Research in Africa: Its Applications and Limits. Edited by William M. O'Barr, David H. Spain, and Mark A. Tessler. (Evanston: Northwestern University Press, 1973. Pp. 349. $15.00, cloth; $4.95, paper.)

This volume includes 23 papers, mainly by anthropologists and political scientists, that deal with various epistemological and methodological problems encountered by survey researchers in Africa. Part 1 focuses on the merits and demerits of survey research in Africa; Parts 2–4, on difficulties experienced and solutions employed in sampling, measurement, and administration respectively.

Part 1 begins with a vigorous paper by van den Berghe (Chapter 2), who argues that survey research is less useful than observation (e.g., at a political rally) or unstructured interviews (e.g., bull sessions) for studying subjective phenomena such as attitudes, especially at the group level. His statement is weakened, however, by a tendency to confuse (1) subjectivity of data with invalidity of measurement, (2) sufficient knowledge to ask appropriate survey questions with already knowing the answers, and (3) problems with impossibilities. Subsequent articles by Ijomah (Chapter 4) and Drake (Chapter 5) add to the criticism of excessive quantification, interviewer effects, and Western cultural biases of survey methods employed in African research. Cohen (Chapter 3) replies effectively to many of these criticisms, while Harrell-Bond (Chapter 6) persuasively illustrates the value of social survey methods used in conjunction with other research methods.

Part 2 includes four papers that show ingenuity in dealing with sampling problems but unfortunately contain limitations undermining their value as research guides. Mitchell (Chapter 8), lacking a complete enumeration of his research population, used aerial photographs to locate all houses in his area of interest. He was then able to collect basic census information from household heads on 634 people residing in a random sample of the houses. He subsequently selected for intensive interviewing a sample of 200 residents stratified by religion (a major focus of the study), sex, education, and age. (Unfortunately the strata within his religious categories are not strictly comparable and may confound data analysis.) In principle, these sampling procedures are reasonable, although potential difficulties (e.g., impossibility of aerial photography; reactivity of initial census on subsequent interview data) should be kept in mind.

Cohen (Chapter 9) and O'Barr (Chapter 10), attempt to defend the representativeness of their nonrandom samples by showing either parallel relationships in data from two nonrandom samples selected using the same procedures (Cohen), or parallel distributions on selected variables between a nonrandom sample (all members of a single geneology) and a census of residents from the same locale (O'Barr). Neither argument is defensible since the nonrandom sampling biases in each case can easily be repeated, and neither sampling design provides a basis for estimating error probabilities within specified limits.

Weisner's (Chapter 11) study of men who are part of a rural and urban migrant labor pool is based on all 24 members of a single clan living in a particular urban housing estate and 24 rural males *self-matched* by the urban males according to criteria which maximized kinship closeness and similarity in subclan membership, age, and educational level. (It is little wonder, given this sampling design, that his research hypotheses regarding similarity between the sets of urban and rural migrants are confirmed.) Weisner claims that the representativeness of his rural-urban network sample to general migrant worker populations is less relevant than whether it represents "the varieties of such rural-urban systems in Kenya" (p. 132). Clearly it does not because it is a single case of such systems; thus, generalizations to other systems and controlled analysis are largely precluded.

Osborne's paper in Part 4 (Chapter 23) also focuses on sampling issues. He discusses how he confirmed the accuracy of a household census in a community characterized by rapid turnover in household personnel by three months later taking a second census which produced virtually identical age and sex distributions despite the high mobility of family members. Unfortunately, systematic selectivity in both censuses (e.g., underrepresentation of a category such as youth away at boarding school) may have produced similar, but inaccurate, results.

Part 3, containing eight papers on measurement topics, begins with Tessler's (Chapter 12) contention that high coefficients of reproducibility for a set of items demonstrate validity and reliability. Few methodologists will agree, since it is quite possible that method effects or conceptual biases will systematically affect scale items even if Guttman scalogram analysis shows them to be unidirectional.

The remaining papers are primarily concerned with the difficulty of operationalizing theoretical concepts. The Fletchers (Chapter 13) argue that for certain studies, measures of educational attainment based on examination certificates of achievement are more appropriate in Nigeria than measures based on number of years of schooling. This suggestion is valid but should be coupled with the reminder that years of schooling and other measures are more appropriate for other research purposes and that such generalizations apply to research areas other than Nigeria. Rather than including multiple exploratory measures as in their survey, their paper should remind researchers of the importance of conceptual clarity about research objectives and of the value of pretesting measures. In a similar vein, Quinn (Chapter 15) shows how different assumptions about social and cultural patterns can affect question wording which, in turn, affects results and interpretations, while Ross (Chapter 14) attempts to clarify alternative conceptions and measures of ethnicity. His conceptual distinction between "social" and "psychological" ethnicity versus ethnic "behavior" and "value orientation" is ambiguous, however, and his proposed questionnaire measures fail to correspond clearly and comprehensively to the conceptual categories.

Moving from direct questions to projective techniques, Barkow (Chapter 16) and Spain (Chapter 17) describe interesting attempts to operationalize theoretical concepts by means of culturally-adapted TAT instruments. The studies are careful and informative even though the range of behavioral topics included in Barkow's TAT analysis was small, and the method is probably uneconomical for many survey studies.

In another departure from structured items, J. F. O'Barr (Chapter 18) reports on a method for putting questions in the form of a story-sequence which respondents can be asked to complete. Her intent was to avoid replies affected by "official ideology" or social desirability response sets, but the method clearly does not preclude such biases. It is simply a special type of open-ended questions and shares most of the well-known advantages and disadvantages of such formats.

The concluding paper in this section by Bennett and Eastman (Chapter 19) discusses the relevance of survey methods for linguistic research and presents a questionnaire useful for studying Bantu languages.

Part 4 focuses on the role that investigators or interviewers play in obtaining cooperation and accurate data. Mitchell (Chapter 20), for example, investigated the effects of interviewer and respondent characteristics (e.g., sex, ethnicity, etc.) on a "respondent cooperation scale." This scale is treated as a measure of interview ease or difficulty, even though "cooperation" is not defined and evidence of measurement validity is lacking. Respondent cooperation is an important problem, but given the measurement ambiguity, the small number of interviewers (N = 6), and the possible differences in their respondents or in their ways of evaluating interviews, the conclusions drawn by the author are extremely tenuous.

Tessler (Chapter 21) investigated the degree to which his presence as a foreign researcher distorted the interview data he collected by comparing his data with data collected from a matched sample by questionnaire methods. Foreign-researcher effects cannot be tested in this way, however, because both interview and questionnaire data-collection techniques may be viewed by respondents as foreign in origin or sponsorship and may thus produce equivalently biased results.

Kinney (Chapter 22) provides an interesting personal account of techniques that gained her entreé and cooperation from informants in a study of shrine ceremonies. The techniques consisted of performing dances and displaying photographs and peculiar articles (dolls, herbs, etc.) that demonstrated to shrine leaders that she had valuable information about similar shrine activities practiced among various cultures in the Americas. This set of techniques is actually a particular example of general strategy for gaining access to and cooperation from respondents (1) by demonstrating expertise or special status (through letters of introduction language fluency, etc.) and (2) by offering something in return for cooperation (money, information, etc.). However, Kinney does not detail the factors, conditions, or general principles governing successful use of her particular techniques or their general significance for both survey and nonsurvey research.

Knauss's study (Chapter 7), although included in Part 1, also deals with the problems of access and sponsorship which he encountered in a study of European settler attitudes towards Africans in Kenya. Unfortunately, he found it necessary to abandon most of his originally intended sampling and data-collection methods, and the resulting data are difficult to evaluate for numerous reasons. The major point to be derived from this paper is that local prestigious sponsorship, familiarity with local customs and values, and unobtrusive data-collection methods are often useful in survey research, especially on sensitive topics.

With regard to the whole volume, it should be emphasized that *African* survey research methods do not differ from survey methods in general. Most of the authors acknowledge or imply that concepts and methods should be dictated by the particular research objectives and sociocultural contexts of each study, and these are no more or less diverse in Africa than elsewhere. It seems clear that the major problem in African survey research is *not* that special difficulties associated with African societies require novel survey research methods; rather, African survey research has all too frequently been done by careless or ill-trained researchers. The collective message of these papers, then, is that successful survey research requires ingenuity, hard work, and clear thinking. Mindless borrowing of concepts or techniques from previous survey research will usually produce meaningless or misleading results. Unfortunately, survey research in Africa has tended to be of poor quality (as some portions of the papers in this book illustrate), and this failing contributes to the skepticism and criticism it sometimes receives. On the other hand, good survey research is just as possible in Africa as elsewhere (as other portions of this book illustrate), and both the features and the failures illustrated in this volume will, one hopes, contribute to the improvement of future survey research, both in African and non-African contexts.

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Nigeria: Dilemma of Nationhood, an African Analysis of the Biafran Conflict. Edited by Joseph Okpaku. (Westport, Connecticut: Greenwood Publishing Company, 1972. Pp. 426. $14.00.)

In this volume eight African writers contributing ten original essays analyze and interpret the Biafran conflict from various perspectives. Moyibi Amoda and Peter Ekeh trace the conflict to its roots in the colonial past. Jimoh Lawal and Okwudiba Nnoli each taking a Marxist and socialist position, relate the war to incipient class formation, class conflict, and the international market place. Victor P. Diejomaoh assesses the economic costs of the war, while Ogbemi Ola Omatete turns to policy recommendations stemming from the war. Finally, M. Nziramasanga addresses himself to the implications of secessionist conflicts for Africa in general and for the O.A.U. in particular. Bracketing these more analytical chapters are two essays by the editor who writes as a participant observer, and who alerts us that "it is the expressed hope of every contributing author to this original book that its greatest usefulness will be to fellow Africans" (p. 13). The theme of "usefulness" —what lessons might be gleaned for Africa out of the Biafran war?—is one that concerns all the essays and gives unity to this volume.

The Biafran conflict, the war between the former Eastern Region of Nigeria and the rest of the Federation, lasted for three years, killed people in the hundreds of thousands, and cost close to £1600 million or "about Nigeria's gross domestic product of 1966" (Diejomaoh's estimate). In the usual interpretations of such conflicts there is either an overemphasis bordering on caricature of the ethnic factor (see for example the *New York Times* editorials of a few years ago) or there is a complete disregard of culture, with the interpretation finding the causes of conflict in postcolonial relations of economic dependency or class war, or both. Joining other scholars working in Africa and beyond, Amodah and Ekeh see in ethnicity not necessarily a prime cause of conflict. They see it as an important intervening variable between economic and political competition on the one hand, and civil war on the other hand. Basing his work on that of Shils and Geertz, Ekeh points out, for example, that uneven levels of development between regions led to an overemphasis on "primordial" rather than on "civil" ties which in turn made it difficult for the Nigerian state to become institutionalized.

Though there is much in Lawal's essay to disagree with—he sees ethnicity as an epiphenomenon of forces based on class divisions—his tracing of the Biafran conflict solely to the development of "commercial" and "bureaucratic" classes, themselves underpinned by external economic interests, is stimulating and deserving of attention. What lends particular force to his Marxist analysis is its societal and international perspective on local conflicts. Nonetheless an analysis based on an orthodox notion of class is problematic. Do classes in present-day Africa stem from the means of production as Lawal argues, or do they rather stem from the organization of political power more narrowly defined? Lawal's essay like other Marxist interpretations would have been more fruitful had it analyzed its conceptual machinery before applying it to the African reality.

Turning to some of the shortcomings of the essays and of the volume as a whole, one is disappointed to see the first coup of January 1966 referred to as an "Ibo Coup." Such a reference is not only false—there were other nationals besides Ibos involved in the planning and execution of the coup—it contradicts the main thrust of the volume which argues that it was the historical and structural situation of Nigeria that led to tragedy. In terms of organization, an introductory essay attempting to highlight and synthesize some of the discussion would have been enlightening. Useful too would have been short sketches of the contributors allowing the reader to know the background of the writer whom he was reading. Lastly with the exception of Diejomaoh who uses federal statistics, original data is a source of strength missing from this volume. Most of the contributors base their work on published material or secondary sources. Criticisms aside, *Nigeria: Dilemmas of Nationhood* is in most ways a solid contribution. It is an effort which is doubly welcome as it takes its place among other works by African scholars and intellectuals who are in increasing numbers making their own interpretations of Africa's historical and political situation.

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The Politics of Cultural Subnationalism in Africa: Africa and the Problem of "One state—many nationalisms". Edited by Victor A. Olorunsola. (New York: Anchor Books, 1972. Pp. 340. $1.95, paper.)

The editor has drawn together five case studies of cultural subnationalism in sub-Saharan Africa. Four of the articles were written for the publication, providing an excellent opportunity for comparative analysis of a major aspect of contemporary African politics. In a brief introduction, the editor gives an overview of the issues surrounding the concept of "one state-many nationalisms" —a condition which characterizes many African states. He emphasizes the roles played by (1) heterogeneous and incompatible traditional political cultures, and (2) the country's colonial and later experience. The contributors have been asked to concentrate on these issues and review possible solutions to problems of national unification created by these conditions.

Unfortunately, there seems to be disagreement among the contributors about what constitutes cultural subnationalism. It is never clearly stated what propositions are to be evaluated in the case studies or what theoretical framework could be followed. To have asked the contributors to evaluate specific propositions within their respective case studies would not have been forcing a "monistic view"—a concern voiced by the editor. Indeed, without such a framework there is the risk of a melange of particularistic viewpoints which renders comparisons across nation-states difficult.

Nevertheless, the editor hints at the examination of a provocative thesis which might be summarized as follows: First, the assumption that early (primary) and even later (secondary) socialization leads to the internalization of attitudes, norms, value orientations, and belief systems which have a major influence on an individual's behavior throughout his life. Second, the observation, as earlier noted by Furnivall, that African nation-states are culturally plural, (*Colonial Policy and Practice,* [Cambridge: Cambridge University Press, 1948]) implies that most individuals in these countries, especially prior to the last two decades, were socialized in environments that tended to be sealed off from the experiences of those from other ethnic groups. At independence, all these peoples became part of the same nation-state. These distinct patterns of socialization could result in different and even incompatible values relative to political or social legitimacy, reward distributions, or the role of achievement in the distribution of things of value. If these points are valid, we could expect that the integration of members of these groups into larger social units (nation-states) would be difficult. An interesting corollary would be to evaluate whether or not the leaders of these new states were exceptions to such patterns.

The above should be clearly differentiated from two related hypotheses. The first deals with the question of multiple loyalties held by individuals in the political system. Shifts in the relative importance of loyalties and their related identities are presumed to be less intractable than those reflecting conflicts in basic political orientation. This is especially the case when loyalties to such units as country, region, and ethnic group are not necessarily inconsistent. The most common expression of this phenomenon is between groups whose essential difference is one of identity, and the attendant feeling that, their "group" should receive a "fair share" of the social goods, rather than fundamental conflicts over the legitimate basis of political decision making. The second deals with a lack of vertical integration (social mobility) and resultant political dissatisfaction when differential mobility is experienced between groups. Often in a given political environment, two or more of these situations (cultural subnationalism, multiple loyalties, or a lack of vertical integration) might occur together, but in principle, their effects could be separated.

In the first study, Olorunsola treats classic issues such as reward distribution as being at the heart of Nigeria's political conflicts. Following his outline, he briefly examines the traditional political cultures of the major ethnic groups in Nigeria: Ibo, Hausa/Fulani, and Yoruba. His characterization of them is superficial, however, and the study suffers from lack of documentation as well as unsubstantiated generalization throughout. For example, he imputes individual psychological properties to ethnic groups with regard to differences in group behavior (pp. 21–22), group achievement (p. 29), and status deprivation (p. 35). His assertion that there is a tradition in sub-Saharan Africa of coups being followed by a return to civilian rule within a relatively short time (p. 41) might surprise the peoples of Nigeria, Sudan, Zaire, Uganda, Mali, Togo, Central African Republic, and Burundi—i.e., about half of independent black Africa's population.

The chapter on Uganda by Kasfir is useful and informative, but the author vacillates on what boundaries to use in defining ethnic groups. At one point he resolves this problem with a modified version of "situational ethnicity" (p. 6) in which ethnicity is defined by location within the social matrix. While there may be some validity in this definition, it certainly leaves the reader wondering what role is played by "cultural" subnationalism. Nevertheless, Kasfir later states that he will use differences in traditional political cultures to classify groups in order to "permit us to hypothesize that differences in traditional cultures will produce different political values which in turn will create conflict when integrated into a new state" (p. 58). He concludes, however, that this hypothesis is much less important than other factors such as "threats to security" when he considers six case histories from Uganda that seem to depend as much on competition over political rewards as on effects of differing traditional structures. A model involving the analysis of multiple loyalties seems much more consistent with the author's data. He also argues that education and other political goods are allocated in an unequal way to ethnic groups. This in turn promotes further inequality.

The chapter on Sierra-Leone by Simpson,

while short, is helpful. Simpson implies that ethnicity is a matter of "identification" and, in fact, states that the major groups, the Mende and Temne, show no significant differences in traditional political culture. Here again the model of "competing loyalties" and threats to social and economic security appear to be relevant.

The Congo-Kinshasa (Zaire) chapter by Turner provides considerable descriptive material on the ethnically related conflicts in Zaire but stresses the "underlying unity of Congolese cultures" (p. 195). The chapter is useful, but it is ambiguous when interpreted as a test of conflicts over differences in political culture. Rather, it gives the reader a view of conflicts which appears to be similar in structure to those in Uganda and Sierra-Leone.

The last chapter by Rothschild on Kenya had been published previously and does not conform as well to the central question of the book, because it is concerned with the conflicts between Europeans, Asians, and Africans as "primary" groups.

Since the book was to give us insight into the problems of "one state-many nationalisms," we should reasonably expect comparability among case studies in terms of the previously discussed propositions. Yet one is left with the view that the book is not successful in achieving the original objective of contributing to our knowledge of comparative political development although the individual case studies can be usefully consulted. The comparability of the case studies suffers both from a lack of definition of the issues in a brief introduction, and minimal linking of the articles. If a detailed examination of the issue of ethnicity in African politics is to be made, then both a clarification of the various aspects of ethnicity and a specification of its hypothesized relation to other behavior is necessary.

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Society, Culture, and Change in the Middle East, third edition. By Raphael Patai. (Philadelphia: University of Pennsylvania Press, 1969. Pp. 560. $5.95.)

Professor R. Patai's book, "Society, Culture and Change in the Middle East," represents the third edition of the hardcover book originally published in 1962 under the title, "Golden River to Golden Road: Society, Culture and Change in the Middle East." The general reception of the earlier versions of the book, as well as the steady demand for them, attests to their value as general ethnographic sourcebooks for students of the area. The latest edition of the book remains a laudable effort to synthesize a large body of material on a complex area and to present it all in a lively manner. Nevertheless, I feel impelled to point out some of the book's shortcomings which became apparent to me when I used this book as a supplementary source in a course on the Ethnology of the Middle East.

Perhaps the most serious drawback of the book is its general lack of a clear and consistent temporal focus. Granted that this is an endemic problem and a real challenge to anyone engaged in the study of a rapidly changing society (and the Middle East is one such "transitional" or even "post-traditional" one), it seems to me that Mr. Patai has only added to the confusion by neglecting to indicate the current prevalence as well as the significance of the various cultural patterns and institutions which he describes. By failing to locate in time and place such marginal practices as the *mut'a* marriage, female circumcision, father's brother's daughter marriage, and the noble-vassal tribal dichotomy, he implies that these patterns are dominant and widespread throughout the area he designates as the Middle Eastern "culture continent"—an area stretching from the West African littoral to the Caucasus. One gets the impression that the author seized upon a number of beliefs and customs of secondary importance in the culture, and then used them as pegs on which to hang the whole fabric of social reality. Thus, in the first chapter (p. 25), Mr. Patai informs us that "the largest traditional social grouping to be found all over the Middle East is a loose, informal twofold faction which, at the same time, is powerful in its hold over population whether nomadic or sedentary. This social grouping resembles . . . the kind usually referred to by the term 'moiety' or 'dual organization.' " In Chapter 7, Mr. Patai describes this amorphous "dual organization" as it operated and presumably still operates today. Under this rubric, the author proceeds to include all types and manifestations of social and political groupings: holy lineages, ethnic divisions, sectarian and political parties, and factions as well as segmentary tribal organization. Surely, it would have been more appropriate to identify the different principles by which groups are organized in the Middle East (e.g., patrilineality, holy descent, ethnicity, sectarian affiliation, etc.) and then to examine the different contexts within which each of these principles becomes the crucial one in terms of self-identification and group action. Grouping all into the static, if not archaic frame of "dual organization" prevents the reader from fully appreciating the dynamics of social organization and action in the Middle East.

The general emphasis throughout the book is on the "traditional" to the neglect of the "modern." This antiquarian approach tends to obscure the rapid rate of change so salient in Middle Eastern society. Moreover, since Mr. Patai generally fails to integrate the customs and institutions of the area into the overall social structure, the book becomes in places a sheer compendium of cultural tidbits that are grouped and juxtaposed with no indication as to relative value or importance. For example, in the chapter "Women in a Man's World," in the section devoted to male and female circumcision, Mr. Patai writes: "female excision, reportedly practiced widely in many parts of the Middle East among the Moslems, as well as among the Copts of Egypt, is in all probability more prevalent than would appear from its reported incidence" (pp. 444–45). Reported by whom? What evidence do we have that it is more prevalent than reported? It is true that the practice may have existed in pre-Islamic Arabia, and it is also possible that it was sporadically practiced in some areas of the Middle East during the eighteenth and nineteenth centuries as reported by some travelers. But Mr. Patai does not let it go at that when he adds "numerous Orientalists have noted in general terms that the operation is still widespread in the twentieth century" (p. 446). This general statement is apparently based on the qualified reports of three Orientalists: Robertson Smith, A. Musil, and J. A. Jaussen whose books were published in 1903, 1908 and 1927 respectively. I have dwelt at length on a seemingly insignificant detail in order to illustrate some of the generalizations and unsupported claims that Mr. Patai makes throughout the book. Students become fascinated by such curious and dubious ethnographic odds and ends to the detriment of the more vital question such as that of the proper assessment of the role and changing status of the Arab woman.

Finally, nowhere does Mr. Patai mention the politics of oil in the area or its role in the modernization of the Middle East. I managed to locate one reference to the subject and that in the Appendix. On p. 482, in discussing the per capita national income for the nations of the area, Mr. Patai writes ". . . the area also includes the one country with the highest such income in the world: Kuwait. The explanation lies in the small population of this sheikhdom and its very sizable oil revenues which are counted in figuring the per capita national income, although in fact, only a small portion of it filters down to the population at large . . ."

Despite the above reservations, Mr. Patai's book remains a valuable contribution to Middle Eastern ethnology. The scope of the author's knowledge is impressive, and the book is a useful source of material for students and teachers of the area.

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Politics in Plural Societies: A Theory of Democratic Instability. By Alvin Rabushka and Kenneth A. Shepsle. (Columbus, Ohio: Charles E. Merrill Publishing Company, 1972. Pp. 217. $8.95.)

The effects of cultural pluralism on political systems have been the subject of substantial research and considerable debate for many years. The authors of this study seek to explain democratic instability in one type of culturally diverse society and present a theory which is rigorous and systematic, relating it to a large number of empirical cases. Such an effort is laudable and long overdue. It is also a major undertaking. Not surprisingly, *Politics in Plural Societies* has shortcomings as well as making important contributions.

Rabushka and Shepsle limit their universe to a very carefully defined segment of pluralistic societies. They are not interested in societies which are culturally plural *per se,* but only those in which (1) politics is organized in terms of cultural communities and (2) the overwhelming preponderance of political conflict is perceived in terms of ethnicity (p. 62). Such societies are called "plural" societies as opposed to those which are only culturally diverse, which are defined as "pluralistic" societies. It should also be noted that ethnicity is defined very broadly to include organization on the basis of race, language, religion, tribe, and custom. This definition creates some problems for cases in which several of these items seem to be salient but not reinforcing. Which is the "real" ethnicity? One might say that *a* rather than *b* is the real ethnic group because it seems slightly more salient—but that is not entirely satisfactory. It might have been more useful to limit the definition of "ethnicity" or speak of the various "communal groups" (a term which seems to be used interchangeably with ethnicity) so that the communities can be identified more easily. This would also help avoid confusion in places like Africa where "tribal" identities are often quite fluid.

The book is divided into two distinct sections. The first part is devoted to the presentation of definitions, theory building, and the elaboration of a paradigm. The second part of the book consists of a number of case studies which exemplify some of the propositions made in part one. Countries included in this examination are Guyana, Belgium, Yugoslavia, Malaya, Ceylon, Trinidad, Cyprus, Rwanda, Zanzibar, Lebanon, Sudan, South Africa, Switzerland, Rhodesia, Ireland, Burundi, and the Congo.

The most important contribution of this study lies in the attempt to delineate carefully and define a number of concepts which can help us understand cultural conflicts. Much of the literature on ethnicity has been hampered by the assumption that cultural diversity induces conflict and instability. It may well be that cultural ties can also foster integration and stability. The important question, as Rabushka and Shepsle convincingly demonstrate, is not whether culturally diverse communities *exist* within a nation, but whether these differences *are seen to be politically salient*. In trying to deal with the difficult problem of measuring salience, the authors put forth some very interesting and useful material on preferences, coalition formation, utility, and intensity. Unfortunately, after some valuable insights into the problem, the authors argue that they cannot fully examine salience, or deal with why particular issues become salient, without a theory of political entrepreneurship (pp. 60–61). After an excellent start it is unfortunate that they did not pursue this question further with or without a theory of entrepreneurship. Such a view also assumes that the masses are unable to create politically salient issues (and perhaps drag reluctant political leaders along).

In the third chapter Rabushka and Shepsle present a paradigm of politics in plural societies which comprises five distinct features of plural societies (over time): ethnic cooperation, the growth of ambiguity, increased salience of ethnicity, the decline of multi-ethnic coalitions, and electoral machinations and mistrust. The first three features of the paradigm are particularly well analyzed, and all of them are nicely illustrated by the case studies.

Perhaps the most serious problem in this study has to do with the relationship between plural societies and democracy. In the last two sentences of the book the authors ask: ". . . Is the resolution of intense but conflicting preferences in the plural society manageable in a democratic framework? We think not" (p. 217). Given the authors' definition of plural society and related propositions, this conclusion comes very close to being true by definition. Since plural societies by definition are organized politically on the basis of ethnic divisions which are politically salient, and since it is assumed that preferences are uniform and communities are in conflict on all issues (p. 67), if we accept these assumptions there is virtually no alternative but democratic instability.

Rabushka and Shepsle present a number of assumptions and propositions about plural societies. While it is useful, as a research strategy, to state and test them in absolute and unequivocal terms in order to see how much they explain, it is not clear that this is what the authors are doing. For example, how much does the assumption (p. 67) that all members of an ethnic group ". . . may be represented by identical 'ethnic preference functions' . . ." (which is clearly empirically false as an absolute) help us to understand ethnic conflict and its relationship to democracy? We learn very little from the data presented here. Given the fieldwork done by Rabushka and Shepsle (in Malaysia, Northern Ireland, Belgium, Switzerland, Holland, Trinidad, Quebec) one would have hoped that some of these assumptions had been tested in the field to see how far they would take us. The authors might have made more progress in dealing with ethnicity if they had presented half as many cases, which are often repetitive even in this brief form, and devoted more effort to testing their theory and the validity of their assumptions.

While *Politics in Plural Societies* has serious limitations, they should not detract from its strengths. Many of the difficulties stem from a commendable effort to be precise and systematic. These problems make a contribution of their own in spelling out how much more we need to do in analyzing societies with sharp cultural cleavages. Rabushka and Shepsle have made a contribution to that effort. They present a useful theoretical framework which is both imaginative and suggestive. Those who are interested in cultural pluralism will find their study well worth reading.

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Jewish Reactions to German Anti-Semitism, 1870–1914. By Ismar Schorsch. (New York: Columbia University Press; Philadelphia: Jewish Publication Society of America, 1972. Pp. 291. $10.00.)

The title of Ismar Schorsch's compact monograph, a revision of his Columbia University doctoral dissertation, is most descriptive. It is designed (and conscientiously carried out) to be a study of the storms and stresses in the German-Jewish emancipation process from Bismarck's Second Reich to the outbreak of World War I, with a few references to the Weimar Republic. Since it is primarily intended for the benefit of the historian, the study can hardly be criticized for not offering more to the political scientist in terms of a stronger analysis and a more extended, more comprehensive appraisal, particularly in the concluding chapter. Nor can it be reprimanded for its inadequate inclusion of such behavioral aspects as political culture and socialization, related to the phenomenon of anti-Semitism. I would conjecture that it will be difficult for any future student of the relationship between anti-Semitism and German-Jewish responses to it to disprove or in any significant way to weaken the author's major presentations. These are cogently formulated and diligently sustained and substantiated by more than six hundred footnotes. The study is occasionally excessively pedantic in style, at times distracting from the main theme by entirely too many details and perhaps too slow in pace—almost inevitably the result of being a revised dissertation. But it is most carefully researched and deserves to enter the circle of indispensable literature on trials and failures of German Jewry (the "indisputable leaders" [p. v] in world Jewry!) asserting its rightful place in the German sun, and to be allowed to retain its Jewish identity within a multi-religious Germany.

Since the characteristics, forms, motives, and manifestations of anti-Semitism in other states seem hardly to contain significantly more unexplored variables, the present study may claim to possess an importance and relevance beyond Germany and beyond the years covered by the study (1870–1914). In fact, it may be reasoned that a behavioral approach to the study of the phenomena of anti-Semitism could furnish, on the basis of the facts presented here, with due amplification, a more generally applicable and wider set of propositions from which *all* minorities could profit. The author himself, merely on the basis of his almost exclusively historical data, sets up a number of available alternative tactics and strategies which have served some of the prominent German-Jewish defenders, at one time or another, with some sequence and interacting effects. Among these proposed alternatives are: (1) to ascertain whether Jewish assimilation in Germany could effectively and unqualifiedly be pursued while preserving Jewish autonomy, identity and self-respect; (2) to think of ways and means of improving the set-up of Jewish counter-anti-Semitic activities (this is, by the way, the largest portion of Schorsch's book); (3) to increase them by shifting, changing and otherwise experimenting with integrative efforts co-ordinating the various organizational units nationally, regionally, locally and spiritually as well as secularly; (4) to contrive the most workable methods toward the solution of tensions, such as (a) by greater, deliberate efforts of assimilation on the part of the Jews (ultimately at the expense of substantial Jewishness and self-effacement); (b) by securing full integration from the German Gentiles in return for a promise to cooperate more fully in a total national unity—Heinrich von Treitschke and others' plan and obsession; (c) by systematically organizing and professionally directing protests and appeals to the seat of government, the courts, certain receptive political parties, and even non-German humanitarian organizations; (d) by relying and depending on more liberal, progressive, and ethically minded non-Jews, in return for obliging them by giving up some of the Jewish forms of exclusiveness and selectiveness, engaging in voluntary "reformism" and self-purges of "undesirable Jewish traits"; (e) by counting on the ultimate workings of a progressively developing sense of abstract justice; (f) more militantly, by collecting the fullest energies and dynamically asserting the essence of creative Judaism; (g) by Jews defending and propagandizing their cause and more forcefully asserting their legal and constitutional rights; (h) by more stoically and realistically withdrawing into their own cultural, religious, and ethnic shell, re-charging their batteries (at the risk of being condemned as "particularists"), as a sublimation for dignity denied or a device for regaining self-respect or recuperating from rejections and insults; (i) by ending an unequal and futile battle, surrendering to a more or less forced or at least strongly "recommended" conversion and facilitated system of intermarriage; or finally, (j) by giving up altogether, packing and leaving their fatherland in order to return to their Palestinian ancestral home.

The strength of this study rests in Schorsch's analysis of the major defense organizations built by German Jews, culminating in the Central Union of German Citizens of Jewish Faith. He evaluates their organizational make-up, problems of financial resources, membership policies, the acceptance or rejection by other Jewish organizations, the German reactions thereto, the ultimate success and failure, immediately and in hindsight, and the general impact of these organizational approaches on German Judaism. It is here that the author displays prudent impartiality by not glossing over the obviously far too frequent and divisive intra-Jewish quibbles, terminological confusions and disagreements, petty theological disputations and other forms of consuming civil strife, the lack of grasp of the resulting reactions and consequences on the part of the non-Jewish community. In fact, this study could have been titled also "The Trials and Tribulations of the Resistance to Jewish Disabilities: A Study of Organizational Complexities."

This study, which the author should extend to the Weimar era in order to ascertain whether his theses and projections continue to apply, gains by his balanced inclusion not only of pro-Jewish episodes in modern German history, but also of possible contributions to anti-Semitism by German (and non-German) Jews themselves. This latter argument, which usually comes from the adversaries themselves, is presented by the author, however, in its proper perspective. In his concluding chapter Schorsch points the finger at the German governments (and to a lesser extent the German society) which, with notable exceptions, failed in the Second Reich to assist the overwhelmingly willing and anxious German Jews in their demands for full integration into the German society *without* thereby having to surrender ignominiously their Jewish unique religious and ethno-cultural heritage. In fact, the author emphasizes throughout his book, and particularly strongly in his concluding paragraphs, that, largely through the efforts of the Central Union, the anti-Semitic forces in Germany contributed to the transition from passivity to active self-defense, from lethargy to revitalization, and from disunity to unification of German Judaism—the result of the "constant pressure of a hostile environment." The greatest merit of this study rests in its keen analysis of the trials and failures in German-Jewish attempts to counteract anti-Semitism, or, conversely, in demonstrating that nothing seems to exist effectively to eradicate ingrained anti-Semitism, particularly in face of disunity in the ranks of the Jewish people themselves.

FRITZ NOVA

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The Expansion of England. By J. R. Seeley. Edited and with an introduction by John Gross. (Chicago: The University of Chicago Press, 1971. Pp. xxvii, 248. $9.50.)

In the year of the United Kingdom's concluding parliamentary debates about joining the European Community, the Chicago series, Classics of British Historical Literature (series editor, John Clive, who contributes a preface) fittingly republished after nearly fifty years this still eminently readable and quotable book. In his prophetic interpretation of history, Seeley assigned technological innovation its world-changing efficacy and supported the ideal of imperial federation, upon an example which Europeans have since often flattered by aspiring to imitate:

> ". . . let us compare it to the United States, and we shall see at once that, so far from being of an obsolete type, it is precisely the sort of union which the conditions of the time most naturally call into existence. . . . The federal system has been added to the representative system, and at the same time steam and electricity have been introduced" (p. 236).

As John Gross rightly declares, the book's title suggested extending the bounds, but Seeley was ". . . exclusively concerned with tightening the bonds," of empire. Within what Seeley called "Greater Britain" (i.e., "the old Commonwealth") his book, made from two sets of lectures to undergraduates at Cambridge, 1881–82, taught teachers who educated the intervening generations of schoolchildren until it went out of print in 1956. It transmitted a tradition faithful to the facts of their historical identity and a benign persuasion of the worldwide unity, rather than the foreign conquests, of the British peoples. Seeley foresaw that an awakening consciousness of nationality would some day make India into a great independent state or states; but he also perceived that ". . . England and her colonies taken together make, properly speaking, not an Empire but only a very large state . . . because the population is English throughout and the institutions are of the same kind" (p. 237).

The 1881–82 lectures, especially the first course, explained historically how that had come about. India occupied all but the first and last of the second course. Both courses gave historical substance to more than Seeley's notable phrase that the Empire had been acquired in "a fit of absence of mind." For all their aphoristic and tendentious qualities, these lectures carry the mark of a professional historian. Celebrated as soon as published, they make genuine if not great historiography.

On the other hand it is as the proponent of a characteristic *idea* of historiography, and consequently a prognosticator, that Seeley may interest contemporary political scientists. R. G. Collingwood was anticipated by his statement that ". . . in history everything depends upon turning narrative into problems" (p. 139), while the doctrine underlying Seeley's ". . . history is concerned, not mainly with the interesting things which may have been done by Englishmen or in England, but with England herself considered as a nation and state" (p. 242), had itself long since been anticipated by Hegel. One may at first sight suspect an infinite regress in ". . . the true test of the historical importance of events . . . is their *pregnancy*, . . . the greatness of the consequences likely to follow from them" (p. 115), until noticing that Seeley is there attributing highest importance not to the event's consequences, but to that in the event itself which, whether or not apprehended by its contemporaries, *could* initiate great changes—e.g., as something so apprehended, the first test of a thermonuclear weapon; as something not, the conversion of Saul of Tarsus. The historian's business, wrote Seeley (p. 114) is "to point out to us that this event, which absorbed the public attention when it happened, was really of no great importance, and that event, though it passed almost unnoticed, was of infinite consequence."

Seeley looked out for those events at once dynamic and problematic, but also to be plotted on the time-scale of the rise and decline of states seen as powers. He was convinced that England was then poised between reducing herself to "a purely European Power" or uniting herself with "Greater Britain's" English stock to become a continent-sized state populated by scores of millions. Thus his lengthened perspective on the future let him see that the Russia and the United States of his time were in any case set to become fifty years later just such giant Powers, and would ". . . surpass in power the states now called great as much as the great country-states of the sixteenth century surpassed Florence" (p. 237). Yet he foresaw no threat of conquest from the Continent—". . . what enemy could invade us but France?" (p. 230).

Seeley, the historian of early nineteenth-century Prussia, the biographer of Baron vom Stein, the admirer of German "intelligence and organization," apparently had no inkling of the rise of united Germany (nor, less remarkably, of that of Japan). Thus he could foresee, after emigration to the colonies "on a vast scale" the evolution of ". . . some organization . . . which might make the whole force of the Empire available in time of war" (p. 235), but not the ironic fulfillment of those hopes in the two great German and world wars, after the second of which his united "Greater Britain," having fought through decisive months alone, was quite eclipsed in power and wealth by the two later-involved giants whose growth he had foreseen, and then by the formerly defeated Western Europeans whom he had underrated, and into whose community, nearly a century after *The Expansion of England,* Britain is now contracting.

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Frankrikes författningar. By Ragnar Simonsson. (Stockholm: Almqvist & Wiksell, 1971. Skrifter utgivna av Statsvetenskapliga föreningen i Uppsala genom C. A. Hessler. Pp. 617. Kr. 55.–)

Persisting tension between liberty and power is the dominating theme of Simonsson's account of constitutional history in France from 1870 to Pompidou's election as president in 1969. The book is a massive undertaking—more than 600 pages in length including a 19-page resume in French—in the best Swedish tradition of meticulous historical scholarship. It yields no surprises or significant new historical insights, permitting instead the compelling record of political change in one of the world's leading (and repeatedly troubled) democracies to speak for itself.

The author, a political scientist at the University of Uppsala, has approached his subject with commendable thoroughness. He begins with the constitutional debate concerning the choice between monarchy or a republic in the wake of Napoleon III's capitulation at Sedan; proceeds through the consolidation of the Third Republic, the creation of the Vichy regime, and the constitutional struggles of the postwar period; and concludes with a cautious appraisal of future constitutional change. To document his study, Simonsson cites 606 entries in his bibliography, including references to parliamentary records, minutes of constituent committees, collections of electoral programs, periodicals, memoirs, political tracts and speeches, and 287 secondary sources.

The result is an impressively detailed, if excessively dry, political history of modern France. Simonsson is at his best in presenting a comprehensive account of the successive attempts to redefine political authority in a nation characterized by unresolved political and social dualisms, at his weakest in succumbing to narrative description at the expense of explanation. Both assessments derive from the author's choice of emphasis: constitutional-legal development rather than behavioral or class or socioeconomic analysis.

Thus, Simonsson's treatment of the debates leading to the adoption of the constitutions of 1875, 1946, and 1958 (as well as the popular rejection of the proposed leftist document of April 1946) focuses primarily on conflicting ideas. "For or against the principles of 1789, for or against the church, that remains the primary, the decisive question," he argues.

> The great revolution constituted such a break in the country's history, yielded such a cleavage within the nation as a spiritual unity, that no development has been able to fill this gap, smooth out this sharp dividing line. Freedom or authority, those were the great ideas that led to an unprecedented conflict, that divided Europe, that led to the world war of the time: the revolutionary and Napoleonic wars; no victories, no defeats could prevent these ideas from continuing to clash into our own time (p. 533, my translation).

As practiced by Simonsson, this approach ultimately provokes some challenging questions about the substance of modern politics but suffers several liabilities en route. One regrettable consequence is that the dramatis personae of French politics are reduced to shadowy cardboard figures. For example, Simonsson informs his readers that "Thiers loved power and wanted to hold onto it as long as possible" (p. 31), but communicates nothing about the republican leader as a person. Similarly, Marshall Pétain—surely one of the most tragic figures of French political history—remains frustratingly remote. Simonsson recounts Pétain's efforts to create a dictatorial regime after the fall of France in 1940 and then suddenly depicts him as seeking to modify his policies after 1943 because "Pétain was now convinced that Hitler was going to lose the war" (p. 206). One seeks in vain for an assessment, however brief, of the man's convictions, desires, fears. Even de Gaulle fares little better. Simonsson quotes at length from his speeches criticizing the weaknesses of the Fourth Republic but reveals little about the human qualities of the wartime resistance leader and principal architect of the Fifth Republic.

Also neglected is the wider social fabric of politics. Groups, particularly parties and trade unions, are accorded somewhat more systematic attention than individual statesmen but largely only as organized spokesmen for conflicting world-views. All too infrequently does the author reveal a glimpse of what was happening in France outside the circumscribed circle of official actors in parliament; too superficially does he utilize his occasional data on underlying socioeconomic factors of change and perceptions of the political system by common citizens.

What Simonsson does contribute is a detailed recapitulation of historical chronology with careful attention to dates, the terms of successive political controversies, and voting divisions. In accord with his interpretive theme, Simonsson concentrates on recurrent efforts during the successive republics to institutionalize executive-legislative relations in such a way as to insure reasonably efficient decision structures without endangering individual civil liberties. The ultimate failure of the Third Republic to sustain a viable synthesis between principles of authority and freedom was perpetuated in the inability of the Fourth Republic to achieve a working formula in the first place; the culprit in both cases, as Simonsson argues with legions of observers before him, was the multiplicity of parties. By creating a presidential system of government with a parliamentary "facade" (p. 540), de Gaulle achieved in Simonsson's view a stable compromise—albeit one based on "conservative, reactionary elements" (p. 454)—that is likely to endure beyond Pompidou. "Those who would want to take up the constitutional question from the beginning or at least instigate major changes in the existing constitution," he concludes, "have long since discovered that no such possibility exists, nor have they made such an attempt." Because "the people" have grown accustomed to the present institution, "there is unlikely to be any major struggle over presidential power" (p. 528).

More provocative than the author's cautious prediction concerning the limited prospects of constitutional change are his intermittent reflections on the status of individual citizens in advanced society. Although Simonsson does not generalize beyond the French case, he is clearly concerned about the broader political implications of the contradictory demands of power and liberty. Modern technological man requires efficiency and hence a strong government, he maintains; at the same time the normative imperative of "freedom, social balance" (p. 545) imposes restraints on the arbitrary exercise of power. If the French experience is suggestive for continuing efforts elsewhere to strike a balance between these principles, it is because the revolutionary heritage has engendered a tradition of individual critical awareness. This "critical sense," Simonsson asserts, "is a guarantee of effective control, a guarantee against both dictatorship and the power of bureaucrats, a guarantee that the state will remain a democracy" (p. 546).

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Greek and Roman Voting and Elections. By E. S. Staveley. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 271. $9.50.)

Professor Staveley's book is pre-eminently an attempt to assemble and interpret the various kinds of evidence, literary and archaeological, which bear on the nature and functioning of voting in assemblies, whether electoral, legislative or judicial, in classical antiquity. Although Chapter I, "The Origins of Voting in Greece," discusses political developments and behavior in a rather broad context and suggests that the following chapters will likewise treat politics generally, this is not the case. The focus is quite specific in the rest of the book, and the central theme is the description and explanation of the technique of voting, from the announcement of the convening of a voting assembly through the actual methods of checking the voters' qualifications and of registering their choices, to the final official declaration of the outcome of the vote. Consequently the reader will find relatively little about the theory or historical development of voting practices in Greece or Rome, or about the place of voting assemblies in Greek or Roman government (only parts of two out of twelve chapters are devoted to these subjects); on the other hand he will find a comprehensive and detailed analysis of the techniques and mechanisms employed in voting in the classical period.

Staveley has written as much for "the student of psephology or political science" as for the classical historian, and he has therefore tried to keep his work on a level comprehensible to the less specialist reader. He has succeeded to the extent that neither ignorance of Greek and Latin nor lack of detailed knowledge of Greco-Roman history will prevent the reader from following his presentation and analysis. At the same time a basic familiarity with the major developments of Athenian, Spartan, and Roman history, which unfortunately cannot even be expected of a majority of American college students today, is necessary to an understanding of the book. For example, he rightly assumes his readers know about the scholarly controversy concerning the authenticity of the *Constitution of Athens* ascribed to Aristotle, about Cleisthenes and his reforms, about the Roman institution of *clientela* and its operation in politics, etc. But within these limits, there is much to be learned from the first work to discuss both Greek and Roman voting and elections in a single volume that brings together the fruit of the past century of classical scholarship, complemented by the author's good sense and clear arguments.

The book is divided into two equal parts: the first devoted to a study of voting in Sparta and especially in Athens, the only classical Greek states for which we possess sufficient information to say anything definite, and the second to Republican Rome, with a final chapter on elections in the Imperial Age. The section on Greece first discusses the origins of voting in the Greek world, and argues plausibly that the concept of the vote emerged at first in noble or aristocratic councils which sought to limit royal power, and was then transferred to popular assemblies as the concepts of citizenship and the state gradually evolved. Succeeding chapters treat the manner of appointment to office in terms of eligibility (all or some of the citizens), selection (by direct vote or by lot), and the nature and organization of the electorate; the operation of the lot, so important a feature of government in classical Athens; voting procedures, including discussions of how votes were recorded and tallied, and emphasizing the principle of one man, one vote, which underlay Athenian practice, in contrast to the Roman practice of group voting (this subject merits a chapter of its own in Part II); and a final chapter on manipulation and fraud, which examines the various ways in which interested parties could illegally influence the outcome of voting to their advantage.

The section on Roman voting is a bit more unified, in part undoubtedly because Staveley deals here with the practices and institutions of a single state. The opening chapter describes the origins, development, and functioning of the various Roman voting assemblies, the *comitia curiata, comitia centuriata, comitia populi tributa,* and the *concilium plebis.* The next deals with the crucial and important Roman concept of the group vote, demonstrating how the Romans valued the collective opinion over mere headcounting, and how a candidate for office or a legislative proposal could potentially be approved by an inordinately small proportion of total votes; for example, 18 out of 35 tribal unit votes, or 97 out of 193 votes in the centuriate assembly, would suffice to determine a contest, although only a bare majority in any given unit was necessary to carry that unit. Subsequent chapters treat the preliminaries of the vote, its casting, determination and the possibilities for manipulation. The chapter on manipulation provides an excellent picture of the operation of Roman politics during the late Republic.

Staveley does not shy away from difficult questions of interpretation, even when the evidence is ambiguous or unsatisfactory, and this attitude produces mixed blessings. His suggestion that the use of the lot at Athens resulted not from any ideological principle, but rather from expediency based on the concept of rotation in office, is interesting if unprovable. His treatment of the election of ephors in Sparta is highly speculative and does not convince me; although we cannot know for certain, it seems more probable that the Spartan assembly rather than the *gerousia* elected the annual magistrates. The use of the term "feudal" for both early Greek and Roman society is potentially misleading; Staveley apparently uses it to refer to a system of social organization based on the clan and tribe, before the classical Greek *polis* or the Roman monarchical state had come into existence, and it is emphatically *not* to be equated with the meaning the term has for European sociopolitical organization between the ninth and thirteenth centuries. Several topics could profitably have been given more scope, such as a brief appendix on the dates of the elections of magistrates and their entrance into office, and a discussion of the place of rhetoric in Athens and its influence on voting. There could (and perhaps should) be more explicit comparison between Greek and Roman practices, and there ought to be a concluding chapter. Although there is no bibliography, the persistent reader will find many important references to ancient sources and modern scholarship in the notes. Despite these criticisms, this is generally a sound, dependable work on its specific themes, and it makes a useful, if necessarily technical, contribution to its subject.

CHARLES D. HAMILTON
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Maurice Barrès et le nationalisme français. By Zeev Sternhell. (Paris: Armand Colin, 1972. Pp. 396. Fr. 75.)

Ever since his death in 1923, Maurice Barrès has been steeping in neglect. His name recalls the "brain-stuffing" propaganda of the first World War, the savage polemics of anti-Dreyfusism, the formulation of a chauvinistic and populist nationalism designed to replace its liberal and bourgeois forerunner. On all these counts, while new paperback editions of his books sell in the tens of thousands, the intellectual (and especially the academic) community prefers to ignore him: oblivion becomes the penalty of obloquy.

Yet, behind the familiar description, another figure lurks: the young provincial challenging Paris, like the *Déracinés* he wrote about. Barrès was a latter day Rastignac, wresting success from a sharp pen, becoming the leader of his generation, "prince of youth" in the Paris of the 1880s and the early '90s, author of anarchistic avant-garde novels, pungent critic of his elders and of established order, rebel, prophet of a *culte du moi* that inspired a constellation as varied as Léautaud, Mauriac, Malraux and Aragon, ally of the Socialists in Parliament and beyond, admirer of Jaurès and friend of Léon Blum—all this to 1898 and the Dreyfus affair. Two Barrèses appear on opposite slopes of that *fin-de-siècle* watershed; and reconciling the two may be one key to understanding the political evolution of French middle classes since the Third Republic.

In search of this reconciliation, Mr. Sternhell, a student of the late Jean Touchard, himself now teaching at the University of Jerusalem, has written a perceptive and important book. Its careful progression shows an esthete's evolution from egoism to collectivism, from cultural cosmopolitanism to insistent provincialism, from rebellion to conformity, from rejection of the dead "who stifle us" to their adoption as defining and dynamizing agents of communal revivification.

While such a drift may not seem unexpected to students of politics and personality today, the manner and the logic of it, as Mr. Sternhell traces them, prove illuminating. The search for a remedy to the humiliation of defeat in 1871, to the feeling of decadence among the young, to the shortcomings of liberal democracy, to the social and economic injustices of "the bourgeois order," makes Barrès look for remedies: abroad; in himself; in a popular savior—General Boulanger; in "the people" still uncorrupted; in a socialism still vague enough to be (almost) all things to all men; and in national socialism too, before the Dreyfus affair precipitates him into conservatism. At this point, latent or minor aspects of his earlier thinking (anti-Semitism, racism) come to the fore, and inspire a doctrine of organic nationalism: as aggressive, as exclusive, as elitist, on behalf of the nation as it had been in defending the singular self.

All of this, superficial in my account, is convincingly detailed and documented by Mr. Sternhell. I take issue with some of his views. Among other things, Barrès's anti-Semitism appears to me less central and more opportunistic than it does to Sternhell. Barrès's racism seems closer to contemporary conventions, and certainly less exclusive than the term would suggest today. And I wonder if some of the things Barrès penned in the heat of political struggle should not be viewed in the light of Talleyrand's remark that all that is exaggerated does not count. French intellectuals (even anti-intellectual intellectuals) have long pretended that words are acts. But they know better.

Yet disagreement is a form of attention. And Mr. Sternhell has given us a book that deserves attention, respect . . . and an early translation into English.

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Nationalism in Eastern Europe. Edited by Peter Sugar and Ivo Lederer. (Seattle: University of Washington Press, 1969. Pp. 465. $15.00, cloth; $4.95, paper.)

The study of the origins and evolution of indigenous political ideologies in Eastern Europe has been a neglected field of inquiry by area specialists and students of comparative politics alike. As a result, the literature on the dynamics of postwar politics, particularly on themes of political socialization, elite perceptions, and heterodox ideologies, has failed to account for some of the crucial aspects of that area's political culture, particularly nationalism, and the pervasive influence of traditional beliefs on strategies of Communist nation building.

The image of a total ideological vacuum and the disregard of native—democratic, radical, and non-Communist Marxist—intellectual traditions on the eve of the Soviet takeover has been the hallmark of recent Western writings both of the revisionist historian and the behavioral political science variety. The former (Gabriel Kolko would do for an example) somehow envisages a cluster of backward countries incapable of building democratic societies on their own, hence expendable pawns in the coming cold war; while the latter, such as Kenneth Jowitt's study on Rumania, blithely ignores the heritage of indigenous political ideas and comes out approving of Stalinism as developmental strategy suitable for that country's condition.

Although nationalism is a notoriously difficult concept to operationalize and fit into the framework of a neat comparative model, the subject is too important to ignore when making a serious effort to understand the immensely complex pattern of politics and political ideas in that part of the world. Therefore, on the subject of East European nationalism, any objective study is welcome; and the Sugar-Lederer symposium is certainly no exception. In this book eight historians are seeking to provide a capsule history of nationalism and nationalist political ideologies from the beginning—and the dates range from the seventh century through the eighteenth—to the 1960s.

The main findings of the volume are summarized by Peter Sugar in his introductory essay, "External and Domestic Roots of East European Nationalism." Proceeding from a lucid reconstruction of the origins and basic philosophical dimensions of modern Western European nationalism, Sugar systematically outlines and examines those area-specific factors that have contributed to the subsequent transformation of the ideas of Herder, Rousseau, and the spokesmen of German cultural nationalism by East European intellectuals into doctrines of national independence, social emancipation and preservation of cultural identity. Ideas, as Sugar points out, are fragile commodities that tend to lose much of their original meaning when transplanted into an alien environment that is inimical to their continued growth. The transformation of the original premises of Western European nationalist ideologies into indigenous East European political theories eventually crystallized into four basic types that Sugar calls bourgeois, aristocratic, popular and bureaucratic nationalism. Although neither Sugar nor the individual authors of the country studies offer any systematic or empirical support (none of the latter seem to make use of this typology, anyway) for the classification of the seven countries under these categories, it is an interesting attempt to impose some kind of rational order on the meaning of this elusive term.

While there is much to commend the individual studies for providing useful descriptions of the East European countries' modern intellectual history, they are on the whole quite disappointing as sources of systematic or original information on political ideas. First, there is the erroneous and, among the authors, rather widely shared assumption that nationalism has been the main if not the sole motivating intellectual force in modern East European history. Second, none of the essays pays any attention to economic and social factors, let alone trying to establish a linkage between them and the rise of nationalist and other political ideologies. Third—and in my opinion this is the most important shortcoming of the entire enterprise—the volume fails to demonstrate the nature of the "fit" or the congruence of traditional political ideas, particularly various schools of native, Marxist, and agrarian radical ideologies, and those imported Leninist-Stalinist philosophies of socialist nation building that the respective East European Communist parties introduced and institutionalized upon assuming power after the Second World War.

If we assume, as we should, that the origins of national communism and other ideological manifestations of "domesticism," to use Brzezinski's apt phrase, have their origin in a still insufficiently studied mix of communist and native radical ideas, then this last shortcoming of the volume may be viewed as overwhelming and thus rendering the volume only marginally useful for anyone interested in learning more about these matters.

In conclusion, it may be argued that history and the history of ideas are too important to be left to the historians. Although this book does provide much valuable information, it is no substitute for a set of systematic studies—either country-by-country or across the board comparative, analyses of the genesis, subsequent evolution, and current state of the "normative dimension" of East European politics. Without works of this kind, East Europe will remain the domain of area experts and political journalists whose writings still pass for scholarship in this field of comparative politics.

RUDOLF L. TÖKÉS

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North for the Trade: The Life and Times of a Berber Merchant. By John Waterbury. (Berkeley: University of California Press, 1972. Pp. 241. $10.75.)

Like a Moroccan celebration, John Waterbury comes on firing his flintlocks and playing his *ghaita* (Moroccan oboe) at the same time, leaving the onlooker the choice of the type of entertainment he likes best. The reviewer is challenged to fire back, but that drowns out the music. Can the reviewer, like the reviewed, be permitted to do both, with the audience understanding that it is all part of a good show in the first place?

Waterbury's story concerns Hadj Brahim al-Soussi, a trader from the Anti-Atlas region of Morocco, former UNFP member of parliament, founder of the Moroccan Union of Wholesalers of Food Products, above all one of that small group of Berbers who have left their rocky villages between the High Atlas and the Sahara since World War I and have gone north to the commercial coastal cities from Casablanca to Tangier to constitute the transitional commercial petite bourgeoisie. He is pious, homespun, crafty, clever, moral, honest, loyal, trustworthy, flexible, realistic, devoted to his enlightened self-interest, an elder of the tribe.

Through him, we are introduced to the agnatic organization of the Ammiln tribal cluster and local life, the out-migration from there to the modern commercial centers and relations with the modernizing grande bourgeoisie from Fes, and the ethics of business, the practices of politics, and the beliefs of Islam—the three pillars of his public life. Sometimes the story is told in the Hadj's own words, other parts are the author's own analysis well supported by other authorities on the Swasa (men from the Souss region), Morocco, or traditional market or bazaar economies. The result is a fine portrayal of Soussi ways and values, full of life and insights, an informed guide to an understudied facet of Moroccan society and politics, and a real pleasure to read. Without disservice, one could take Hadj Brahim at face value and enjoy it, a portrait with central figure and background rather than a biography. So much for the lyrical artistry, the *ghaita.*

But there is more, underlying this story. Waterbury begins with a quotation from Geertz to the effect that what one sees depends on his choice of lenses, and then continues by asserting that his subject, the Soussi trading community, "is too fascinating to kill off, or perhaps render invisible, through the devices commonly employed by social scientists" (p. 2). The most articulate of the Swasa (pp. 3–5), Hadj Brahim was chosen as a case study of his community, a uniquely typical Soussi, and the trading community is "The Reification of Hadj Brahim . . ." (title of chapter VII).

Thus, the central personage may be really interesting but he is only theoretically interesting when he is used to answer such questions as "Why was it the Swasa, of all people, who made up the transitional commercial petite bourgeoisie?" and "What is their impact on Moroccan society and their role in its modernization?" In tackling these questions at his chosen level of analysis, Waterbury comes in with his guns blazing, challenging above all the McClelland thesis of need-achievement. In a word, without going into the whole argument, he is effective in pointing out the logical weaknesses (pp. 184–187) and moral pitfalls (pp. 198–201) of *n-ach* but, even teamed up with Hadj Brahim, he provides no better alternative. For the first task he did not need Hadj Brahim; for the second he cannot use him.

Waterbury's answer to the question "Why the Swasa?" is "chance," which is scarcely theoretically satisfying; "Other Moroccans could just as easily have made the move into commerce, but the luck of the draw produced the Swasa" (p. 197). His answer to the question, "What is their impact?" is fuzzy. "Hadj Brahim will not save the Moroccan economy" (p. 198), is a judgment larger than the problem, just as its positive corollary is an unsubstantiated leap into the unknown: "the motor force of economic development, . . . if it occurs at all, . . . will come from state planning and public capital formation, international aid-granting bodies, foreign corporate investors, political revolution, or a combination of all of the above" (pp. 201–202). Substantively, one may well wonder what's left. Procedurally, and more interestingly, this is the very kind of answer that the chosen lenses of Hadj Brahim cannot enable us to see.

But that is no note on which to end. If Waterbury had been able to find the key to development in Hadj Brahim's shop, this book would not have to sell for $10.75. But it does do many other things: challenges and debates some well-worn propositions, illuminates an important corner of Moroccan society, allows us to spend an engaging evening with Hadj Brahim.

I. WILLIAM ZARTMAN
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Mittelstand, Demokratie und Nationalsozialismus: Die politische Entwicklung von Handwerk und Kleinhandel in der Weimarer Republik. By Heinrich August Winkler. (Cologne: Kiepenheuer & Witsch, 1972. Pp. 307. DM 34.)

Winkler examines the political objectives and programs of shopkeepers, artisan craftsmen, and other largely urban small businessmen. Much of his evidence is taken from northwestern Germany, although he has constructed a study that successfully brings this evidence to bear upon the general problem of the German *Mittelstand* (the middle class, but literally the "middle estate"). As Winkler demonstrates, the expression *Mittelstand* came to be used after the founding of the German national state in the nineteenth century to distinguish the petty bourgeoisie from both the big bourgeoisie and the working class. He suggests that this distinction, although useful politically to many petty bourgeois, was bad sociology. Yet German social scientists generally came to accept the concept of a *Mittelstand* that included not only artisans and other small businessmen, but also peasant proprietors, white-collar workers, many professionals, and most civil servants. Winkler has sensibly decided to focus upon small businessmen, rather than attempting to investigate all of the diverse groups that were so often regarded as constituting the *Mittelstand.*

His thesis is that organized small businessmen succeeded in obtaining legislation and government action to protect themselves at the expense of other sectors of society. Such measures included restricting entry into certain occupations and granting guilds and trade associations regulatory powers over everyone engaged in a given occupation or line of small business. Winkler argues that concessions obtained during the Second Reich, most notably during the 1870s and 1880s, led much of the *Mittelstand* to expect state assistance, and hence to turn eventually to the National Socialists, who claimed that they would protect and promote the interests of small business. This support, Winkler concludes, contributed greatly to the coming to power of the Nazi party, but he concedes that the Third Reich fulfilled few of these promises.

Although scarcely novel, Winkler's thesis has never been presented with as much evidence as he provides. He explores his topic on the basis of familiarity with a wide range of written sources, including some material from East German archives. His book will furnish a good starting point for further studies of small business in Germany during the first third of the twentieth century.

Much of the literature on the *Mittelstand* is either somewhat dated or a product of the sectional interests that Winkler scrutinizes. Noteworthy among the rare exceptions to both of these generalizations is Herman Lebovics's doctoral dissertation entitled "A Socialism for the German Middle Classes: The Social Conservative Response to Industrialism, 1900–1933" (Yale University, 1965), which contains sections analyzing the social and economic situation of the middle class, but few of these sections are included in Lebovics's *Social Conservatism and the Middle Classes in Germany, 1914–1933* (Princeton: Princeton University Press, 1969). As Winkler notes (p. 15), Lebovics's book is basically a study in intellectual history and deals with ideas that were more attractive to highly educated segments of the middle class than to the small businessmen on whom Winkler focuses.

It is probably significant that Winkler chooses this way of distinguishing his own work from that of Lebovics, for both books assume that the middle class was able to play a largely autonomous role in pre–Nazi Germany. Although in a recent article in the *Vierteljahrshefte für Zeitgeschichte* (October 1969) Winkler examines big business support for and sponsorship of corporatist doctrines which Lebovics and many other historians have identified primarily with the middle class, Winkler does little in his book to explore the political relationship between big business and small business. He also makes scant use of much of the evidence he presents of the multifarious relations of dependence of small upon big business under the institutions of monopoly capitalism.

Important examples of Winkler's overestimation of the ability of the middle class to act independently can be found in his analysis of the 1930s. He depicts the Nazi leaders, whom he terms "a clique controlled neither by it [the *Mittelstand*] nor by more powerful social factors" (p. 180), as standing above society. In Winkler's interpretation the *Mittelstand* is accurately described as less powerful than the bourgeoisie, but is implicitly and incorrectly considered as having been exploited by opportunistic Nazi leaders mainly to their own benefit. The contradiction in Winkler's appraisal is all the more obvious since he harbors no comparable illusions about who benefited from the Second Reich. He perceives that in Wilhelmian Germany, bourgeois interests were carefully attended to, but he proceeds, seemingly unaware of any possible contradiction, to ignore the relevance of this perception to an analysis of the origins of the Third Reich. To acknowledge the social consequences of the Nazi regime, including the thwarting of the *Mittelstand,* and yet to neglect the import of these consequences when considering the establishment and operation of this same regime is not unique to Winkler. A similar methodological inconsistency is common. Avoidance of this inconsistency will help to place on firmer footings the historiography of fascist movements to which Winkler has made a substantial contribution.

Winkler's study is less notable as an explicit contribution to the comparative study of small businessmen. His general assumptions about differences between Germany and the West are at variance with many of his findings. He accepts the existence of a distinctly German path of development diverging markedly from that of the West. Thus he describes legislation favorable to the *Mittelstand* as an "artificial restoration of preindustrial norms and institutions" (p. 40), and in the sections dealing with the middle class before the Weimar Republic he implies that such a "restoration" was peculiar to Germany. But his brief review of legislation on small business in the United States, Britain, and France in the concluding portion of the book indicates that Germany was not so different after all.

The meager index, an all-too-customary brief list of personal names, does a grave injustice to a solid, detailed work. How can we prod West German publishers into serving scholars better?

WALTER STRUVE

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Meiji Japan's Centennial: Aspects of Political Thought and Action. Edited by David Wurfel. (Lawrence, Kansas: University of Kansas Press, 1971. Pp. 105. $5.00.)

Conference volumes have come to play increasingly important roles in the field of modern Japanese history. The work under review is the product of the 1968 meeting of the Midwest Conference on Asian Affairs. It consists of five essays. Grant Goodman, the general editor of *Studies on Asia, Second Series* (of which this is Volume I), tells us it is a "distinguished collection of scholarly papers" (p. v), but most readers may wish to qualify this assertion somewhat, for while there are some distinguished passages in these papers, the collection as a whole is a mixed bag.

The title of this work, *Meiji Japan's Centennial,* is in fact a misnomer. Except for Marius B. Jansen's typical tour de force on the "Meiji Restoration," in which he wrestles with the broader issues of the Restoration and the modern Japanese experience, the other papers deal largely with micro aspects of Meiji history.

Sidney DeVere Brown examines the assassination of Okubo Toshimichi in a paper which gives some significant insights into one of the most important assassinations of the Meiji period but works less well as an explanation of the "dark underside" of "modernization" (p. 19)—an extrapolation obviously forced upon the essay by the title of the book. Barbara Teters's study of the "Otsu Affair," the attack on Crown Prince Nicholas of Russia by a Japanese policeman in 1891, is a well-worked-out study of the issue of judicial independence that prevented the judges in the case from acceeding to the government's demands for the death penalty. Her argument that this is the beginning of the "rule-of-law" and "judicial independence" would be more convincing if it were put into the perspective of what happened in the late Meiji years (with the High Treason Trial of 1910–11 for example), or even the Taisho and early Showa years, when all indications are that the fragile flower of "rule-of-law" and "judicial independence" was fading fast under the pressures of the imperial state. This is not to detract from what is basically a sound and interesting study, but it does point out the dilemma of using micro history for macro ends.

Frank F. Wong's essay on the influence of the Meiji constitutional model on the thought of Liang Ch'i-ch'ao and the Ch'ing Reformers deals with one area of Japan's influence on her neighbors during the Meiji years. His thesis that the Ch'ing Reformers saw in Japanese constitutional monarchy a way to preserve "the exercise of arbitrary authority by the imperial throne" by "strengthening the nation through the strengthening of the monarchical system" (p. 70), while Liang Ch'i-ch'ao saw constitutionalism as meaning that the "influence of those who have held power is restricted, while new powerholders take control" (p. 72) is worth presenting because it shows that the Chinese could be as selective in their interpretations of Japanese institutions as the Japanese had been in dealing with those of the West. Not all may agree with Professor Wong's conclusion that in the final analysis the effect of the Meiji model on the Ch'ing Reformers and on Liang was "chimerical" (p. 75), but one has to agree with him that the post-1911 Chinese experience rudely dashed any illusions that power could be achieved simply by adopting a constitutional order.

If Professor Wong's essay might have been more appropriate for a volume on Chinese intellectual history, E. Thadeus Flood takes us into the world of Southeast Asia, where he explores the careers of a number of expatriate Japanese adventurers in Siam. Far from the mainstream of official Japanese colonial efforts in Hokkaido, Taiwan, and eventually in Korea and Manchuria, according to Professor Flood, the Japanese attempt to gain a colonial foothold in Siam consisted of a series of tragicomedies that were engineered by ineffectual civilian romantics who styled themselves after the Restoration *shishi.* Their type was perhaps best illustrated by Iwamoto Chizuna. Iwamoto, Flood tells us, noted in his memoirs that he was an individual of a "careless disposition" and "impatient with details and trifles" (p. 86). These were hardly the ideal qualities of the colonial administrator, and it is far from surprising that most of Iwamoto's efforts ended as dreary failures. Although the personal inclinations of men such as Iwamoto may have served to undercut the force of the message of Pan-Asianism they sought to export to Southeast Asia, there were other causes as well. Flood writes of "cultural, religious, and linguistic differences" (p. 100), and one wishes that he had explored these more fully. Nevertheless, one cannot help but agree with his general conclusion that the Western presence in Siam was never great enough to generate the type of resentment that would have made Pan-Asianism attractive to the people of Siam.

Even though these papers are often interesting in themselves, they do not form a coherent book. In going over the essays I can only conclude with David Wurfel's introductory remarks that "this volume makes no pretense at grand theory or at dramatic new discovery" (p. xii). When one remembers that "grand theories" and "dramatic new discoveries" used to be the criteria for publication, one cannot help but feel a twinge of disappointment that a volume dedicated to commemorating one hundred years of modern Japan should not at least have tried for these.

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The Introduction of Socialism into China. By Li Yu-ning. (New York and London: Columbia University Press, 1971. Occasional Papers of the East Asian Institute, Columbia University. Pp. vii, 138. $6.00.)

More than a decade before the impact of the Bolshevik Revolution, influential Chinese intellectuals and nationalist leaders included some form of socialism in their plans for China's modernization. Though it did not lead to a well-defined radical movement, this early receptivity to socialist ideas (or to what at that time came under the rubric of socialism) is of more than ephemeral interest. In her meticulously researched monograph, Dr. Li Yu-ning shows that despite their doctrinal inadequacies, these pioneer expositors of socialism familiarized thousands of Chinese youth with some of the vocabulary, concepts, and ways of looking at the world that helped prepare the ground for the more rigorous ideological commitment of the 1920s. Dr. Li's analysis of early discussions of socialism also points up modes of thinking and value preferences that invite comparison and contrast with more recent expressions of Chinese Marxism.

The author begins with a brief review of the earliest references to modern socialism in Chinese writings. By the late 19th century, when exclusive interest in Western military and technological expertise had been superseded by an awareness of the larger context of Western ideas and institutions, socialism too came in for brief but favorable mention. Yen Fu (1854–1921), the famous translator and interpreter of Western thought, noted in 1895 that European material progress had aggravated economic injustice and had spawned parties advocating "the equalization of the rich and the poor." Seven years later the leading reformer, K'ang Yu-wei (1858–1927), blamed the institution of private property for causing economic inequality, but felt that the time was not ripe to publish his utopian notion of the "Great Community."

The author tells us that at the same time, K'ang's disciple, Liang Ch'i-ch'ao (1873–1929), probably the most influential Chinese disseminator of modern ideas, predicted the bankruptcy of *laissez-faire* capitalism and the triumph of socialism in Europe. Writing in Japan, where he and other reformers and revolutionaries had taken refuge, Liang relied upon Japanese sources which had in turn been translated from contemporary Western works to expound upon socialist theories and movements. In 1902 he introduced the name of Marx to Chinese readers. In 1904 he analyzed modern imperialism which he found to be generated by excess capital in the West. While Liang believed that nationalization of capital was inevitable throughout the world, he advocated state socialism as the most practical policy for immediate application to China. By 1905 other articles as well as books and pamphlets popularized socialist ideas among Chinese youth who felt that the key to their nation's survival lay in adopting modern ideas and institutions. In the meantime, competition between the K'ang-Liang reformers and Sun Yat-sen's (1866–1925) revolutionary party for the allegiance of the large Chinese student community in Japan added a new dimension to discussions of socialism.

By raising the banner of socialism, says Dr. Li, the revolutionaries proved that they were just as modern as Liang, and by advocating simultaneous social, political and nationalist (anti-Manchu) revolutions, they were able to propose a fast and facile way of overtaking the West and Japan. Eschewing drastic socialist measures, they advocated a modified version of Henry George's land-value taxation plan plus nationalization of public utilities. Pre-industrial China, it was argued, could settle for relatively mild socialism since its purpose was mainly preventive. But the West would not be let off so easy. There, unplanned modernization had created acute inequality which made violent social revolution inevitable.

More for polemical than ideological reasons, Liang retreated from his earlier stand and questioned both the practicality of socialism in general and his opponents' diluted adaptation. His criticisms, which appeared in his journal, *Hsin-min ts'ung-pao* (People's Miscellany), and the rebuttals of Sun's scribes in *Min-pao* (People's Tribune), went on for two years (1905–1907), and they provide the main substance of Dr. Li's study. Though the author does not go beyond 1907, she draws some significant conclusions from this first debate on socialism.

The rejection of *laissez-faire* capitalism and the search for indigenous socialist precedents indicate that Chinese intellectuals had a strong predisposition toward government intervention on behalf of social justice and equality, according to the author. It also reflects their urge to surpass the West and not merely to emulate it. The obsession with speed and the search for short-cuts to modernization show that Mao Tse-tung's "great leap" mentality is not new to nationalist thinking. And when early socialist sympathizers speak of the advantages of China's poverty, and transform it into "an asset rather than a handicap for national salvation" (p. 66), we are also reminded of Mao's boast that the peasant masses are like a "blank sheet of paper."

On the other hand, this study shows that the early socialists were too concerned with the prophylactic function of socialism and tended to overlook the existing social problems of agrarian China. There is no hint here of Mao's emphasis upon struggle or his formula for peasant mobilization.

The author rounds out her excellent survey with a chapter on linguistic evidence that reveals the full extent of the Japanese role in spreading socialist ideas in China.

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U.S. International Business and Governments. By Jack N. Behrman. (New York: McGraw-Hill Book Co., 1971. Pp. 244. $3.95, paper.)

National Interests and the Multinational Enterprise: Tensions Among the North Atlantic Countries. By Jack N. Behrman. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970. Pp. 194. $8.50.)

There is by now a growing literature on the multinational enterprise, although unfortunately much of it appears tangential to the needs of the political scientist. A corrective effort however may be in sight. Recently a group of political scientists have chosen to direct their attention to the phenomenon of the multinational enterprise.

In this area of inquiry, Jack N. Behrman is clearly a central figure. Trained as an international economist, Behrman is presently Professor of International Business at the University of North Carolina. In these two most recent volumes, as in much of his earlier work in this area, Professor Behrman continues to illuminate and assess the impact of the multinational enterprise. Here, however, he chooses to examine their role in contributing to tensions and disputes between nations and to investigate the modes of communication between these firms and the governments in which they are incorporated or choose to operate. As in the past, Professor Behrman remains chiefly interested in describing the web of relationships between the multinational firm, the home government, and the host governments and in exploring the nature of their complex interactions.

In his *National Interests and the Multinational Enterprise,* Professor Behrman directs his attention to the activities and practices of multinational firms that have led to tensions among the nations of the North Atlantic region. As these tensions multiply and grow more visible, Behrman points out, host governments are often provoked into action. While these responses vary, in general they appear to stem from host government concern about the size and aggressiveness of the multinational firm, its superior technological capacity, and its ability to hamper the national economic planning efforts of host countries.

For Professor Behrman, the host country's concerns and fears stem from a single, central fact; namely that these firms are neither domestically owned nor controlled. The possibility that some foreign government, and particularly the U.S. government, can affect the behavior of the affiliates of multinational enterprises serves to intensify these concerns. While Professor Behrman challenges the legitimacy of some of these fears, he nonetheless recognizes their reality for many host countries. Convinced, however, that the multinational enterprise is a beneficial force in international economics and world affairs, he proposes a series of alternative policies for host governments aimed at relieving prevailing tensions and fears, and he recommends a broader dissemination of information regarding the activities of these firms.

In *U.S. International Business and Governments,* Professor Behrman turns his attention to developing a strategy aimed at improving communication between international business and both home and host countries. Improved communications, he believes, would mark a substantial step toward alleviating the atmosphere of suspicion that currently surrounds the activities of the multinational firm. Undoubtedly he is also interested in improving the attitude of the U.S. government toward these enterprises. Freeing the channels of communication between the multinational firm and the U.S. government, Professor Behrman maintains, would not only reduce the unnecessary friction between them, but it would ensure a greater coordination of their joint efforts to improve the conduct of international economic relations. To achieve this coordination, Behrman offers a set of recommendations including a plea for a redirection in the conventional channels of access employed by international business and the establishment of a strong Commerce Department as the principal locus of government-business contact.

The proposals of Professor Behrman certainly merit serious consideration. Improvements in communication between business and government, particularly in an area as critical as foreign economic policy are obviously welcome. More careful thought, however, should be given to the long-range implications of these recommendations. Movements in the direction of improving the coordination of international business interests with those of the U.S. government might reduce the friction between these entities, but at the same time produce a set of new and even more intractable problems. The alleged claims of independence by the multinational firms would be seriously compromised. The flexibility of their operations, long proclaimed as a principal asset, might be readily abridged or considerably blunted. And the antagonisms of the host countries could well be accentuated rather than curtailed. Some might also see in this development the confirmation of a long-held suspicion that in the United States, at least, the interests of business and government are one and indivisible.

On the whole, Professor Behrman has succeeded masterfully. He has provided us with a wealth of information on the behavior of the multinational enterprise, an insight into the nature and sources of the threats these firms allegedly pose for host and home governments alike and an exploration of the principal channels and patterns of communication between international business and governments.

A number of problems however either have not been raised or have been left unanswered. First, in assessing the impact of the multinational enterprise, Professor Behrman offers us few clues as to how we might distinguish the economic from the political dimensions of this impact. While we now possess considerable information regarding the multinational firm's impact on balance of payments and trade, incomes policy, capital flows and as well as in other areas, we lack a corresponding set of political categories analogous to these economic dimensions. The question, for example, of the impact of multinational firm activities on the power relationships among nation-states has hardly been pointedly raised let alone answered. Nor has there been any attempt to deal with the impact of these firms on the bases and sources of legitimacy within political systems.

Second, although he is more sensitive to the political implications of the behavior of these economic institutions than are most scholars in the field, Professor Behrman apparently declines to see the multinational enterprise as a "political actor." Such a view seems to reveal an extraordinarily limited conception of the nature of politics, political institutions, and political behavior. Given its resources and the growing recognition of its increasingly autonomous status, the multinational enterprise certainly seems to satisfy the criteria often employed in the literature to denote a "political actor." Moreover, in the light of the recent revelations of ITT's behavior in Chile, could one still reasonably maintain that the multinational firm is in Behrman's words a "nonpolitical animal"?

On the question of the status of the multinational firm, Professor Behrman's position is far from clear. While some scholars deny the independent status of the multinational firm, preferring to view it as another instrument used by dominant nation-states such as the United States to consolidate or establish prevailing or new forms of economic imperialism, the issue is by no means settled. If future study should confirm the independent status of these enterprises, how might this development affect the international system? Behrman offers us little guidance in this area. A program of research based simply upon an examination of the conflicts between these firms and nation-states would be clearly inadequate. But a strategy of research flowing from the elaboration of a conceptual scheme of the international system which incorporates the multinational enterprise as an independent political actor might considerably deepen our knowledge of international relations and processes.

Finally Professor Behrman's treatment of the role and impact of the multinational firm on U.S. foreign policy is not wholly adequate. Past, present and potential conflicts between the foreign policy interests and objectives of the U.S. government and that of the multinational enterprises are seriously underplayed. Even Behrman's plea for improved communications may not ensure coordination of these policies. Moreover little effort is expended in exploring the significance or implications of the development of a private multinational corporate foreign policy distinct and separate from that formulated by the constitutional organs of foreign policy making. Professor Behrman, it appears, might have been surprised by the ITT memorandums. This area of research badly needs attention, and studies of the international structures and processes of foreign policy formulation within the multinational firm should produce significant dividends in deepening our understanding of the complex processes of foreign policy making.

Meanwhile the literature on the multinational firm continues to mount, and interest among political scientists is certain to increase. Professor Behrman's work should serve as a major stimulant to these investigations. But unless an effort is made to ground these entities within a firm theoretical perspective, investigations of the behavior and activities of these firms will produce little new knowledge nor will it significantly broaden our understanding of international processes and structures of decision making.

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World Society. By John Burton (Cambridge: Cambridge University Press, 1972. Pp. vi, 180. $10.95.)

This short book seeks to present in nonspecialist language the case for an analytical, interdisciplinary study of world society, some basic analysis of world society and the methodological problems of studying it, and some prescriptions for conflict resolution.

The principal theme of the book is that it is important to study "world society," in the sense of the total complex of religious, language, scientific, commercial and other social relationships that are worldwide, rather than merely "international relations" in the sense of the relations among state authorities.

Some other theses that are aired in the book are the superiority of the "steering" or cybernetic model of world politics over "the power theory"; the virtues of analytical, and especially multidisciplinary approaches, over purely historical or descriptive approaches; the unlikelihood that world society will ever become "an integrated whole," given the desire of local groups for participation in decision making and their need for identity; the self-validating nature of our images of a hostile world political environment; the "self-defeating" nature of strategies of isolation, self-defense, power balances, and collective security; the virtues of face-to-face discussion in the presence of a mediator in the resolution of conflicts.

These are large and important questions, and there is much to be said for the answers Dr. Burton gives to some of them. The weakness of the book is that these answers are for the most part mere assertions of a loose and general nature, not accompanied by any careful argument or any recognition of the complexity of the issues being discussed. How are we to compare the merits of the "cybernetic model" and "the power theory" if the competing theories are not spelled out and if we are given simply the slogans? How are we to determine whether or not it is correct to conclude that "A friendly gesture invokes a friendly response. . . . A hostile attitude creates hostility (pp. 89–90) if we are given no evidence of this, and the contrary point of view is not expounded and analyzed?

The author states on p. 22 that "international relations" requires descriptive study, while "world society" requires study that is analytic and conceptual. We are given no reasons for this statement, which is *prima facie* implausible. In the chapter on "Self-Defeating Strategies" we are told that states in their relations with each other tried first isolation, then national defense, alliances, balance of power, collective security, deterrence and nonalignment, in that time sequence. But again, no evidence is provided for this mystifying statement.

There is much rhetoric in this book about the need for putting hypotheses to the test, but the author does not take much account even of the principles of logic, let alone those of empirical science.

Dr. Burton's approach combines the political outlook of nonalignment doctrines of the 1950s with an intellectual outlook that derives in part from Karl Deutsch. Both of these points of view deserve a more substantial exposition than they are given here.

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Civil Aviation Development: A Policy and Operating Analysis. Prepared for Arthur D. Little, Inc. by Robert C. Fraser, Alan D. Donheiser, and Thomas G. Miller, Jr. (New York: Praeger Publishers, 1972. Pp. 202. $12.50.)

The International Air Transport Association. By Richard Y. Chuang. (Leiden: Sigthoff, 1972. Pp. 156, plus appendices. $12.25.)

A book can have both a purpose and a reason for being, and they need not be the same. *Civil Aviation Development* is the outgrowth of a study contract, signed in 1970 between a private consulting firm, Arthur D. Little, and the U.S. Department of Transportation, (DoT). The study's stated purpose is to identify factors inhibiting civil aviation growth, to suggest alternative ways to remove the inhibitors, and to discuss the pros and cons of each of the alternatives. In contrast to its purpose, its reason for being must be inferred from its timing and from those things treated or ignored in its analysis. Evaluated in this way, the study was a weapon in an internal struggle between segments of DoT for domination of the newly created Department. In addition, the study served to persuade the Congress to adopt Department positions on policy-making power back to the line segments. which DoT felt it needed.

Within the DoT, during 1969, Dr. Paul Cherington, in his personal position as Assistant Secretary for Policy and International Affairs, had taken power away from the normal line organizations. The Arthur D. Little study seizes the opportunity created by Dr. Cherington's departure in early 1970 to propose a transfer of policy-making power back to the line segments.

A second management objective of the study is to reduce the autonomy given by the Congress to the Federal Aviation Agency (FAA) within the DoT. Congress had been chary of breaking up the FAA when it included the FAA as a subordinate agency of the DoT, and, in the eyes of senior DoT administrators, too much policy autonomy had been left with FAA. The Arthur D. Little study becomes an attempt to persuade Congress to reduce the FAA's autonomy.

Arthur D. Little had apparently been warned off the concurrent and far more interesting intragovernmental battle among DoT, the Department of State, and the Civil Aeronautics Board for policy control of U.S. international aviation matters. Dr. Cherington's office had been established principally to provide DoT with a strong negotiating position in the battle with State. The Arthur D. Little study ignores the DoT-State-CAB battle; its willingness to suggest reducing the power of DoT's negotiating focus implies that DoT had, by the end of 1970, accepted defeat.

To return to its purpose, the Arthur D. Little book makes assumptions about the state of Civil Aviation Research and Development (R&D) without attempting to justify them. Among the assumptions are, one, that more R & D is automatically a good thing; two, that expansion of Air Cargo is necessary; three, that Air Transport in general needs to be expanded; and four, that Short Take Off and Landing (STOL) aircraft represent the wave of the future. For some people, these assumptions need to be justified, and without justification the study's recommendations for coordination and expansion of R & D are less than persuasive to readers who are not already aviation buffs. The study does a more thorough job in calling for a vigorous attack on the problem surrounding airport location and airport ground access. The recommendations made in this field, both political and organizational, are well founded.

There is no doubt that an outside consultant of the reputation earned by Arthur D. Little carries more weight with policy makers than do "in-house" staff members. Yet, the Little study suggests the testimony of a paid witness, and, as such loses its impact regardless of the logic or the truth of its findings and recommendations. From the viewpoint of a student of government in action, the book is most valuable as a demonstration of the use of presumably disinterested outside consultants to reinforce the efforts of one of the participants in an internal struggle.

The International Air Transport Association book also bears the clear imprint of its reason for being. In this case, the book resulted from a doctoral dissertation at the University of Minnesota. As such, it is carefully researched and documented and portrays accurately the official organization and objectives of the International Air Transport Association (IATA). The book draws conclusions only when they can be clearly documented through published actions. Actions taken by governments are used guardedly to infer underlying policies, but no attempt is made to go further by estimating objectives from policies. The IATA has been subjected to attack recently by protagonists of the purely consumer viewpoint, particularly by K. G. J. Pillai's *The Air Net* and by Ralph Nader's Aviation Consumer Action Project. Mr. Chuang's book is completely neutral on the pros and cons of the IATA but in the process does not help much in evaluating the logic or the need for an international airline cartel. Nevertheless, as a handbook of the official organization and objectives of the IATA, it is an excellent source book.

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The Bracero Program: Interest Groups and Foreign Policy. By Richard B. Craig. (Austin: University of Texas Press, 1971. Pp. 233. $7.50.)

In this volume, Professor Craig analyzes one of the more controversial domestic programs of recent years—the bracero program, which involved the contracting of Mexican farmworkers. The legislation for contracting on a large scale during World War II was to overcome a manpower shortage, but it soon became an institution under Public Law 78. This recruitment lasted for twenty-two years and involved more than 4.5 million Mexican nationals. Throughout, the program remained the focal point of an intense interest-group struggle. This struggle and its combatants—agri-business, organized labor, and the Mexican government—provide the central concern of the study.

According to Craig, the bracero program was for Mexico a source of foreign exchange and a safety valve against the greater rural unemployment. For the United States, the author sees imported labor serving to develop the Southwest and being indirectly responsible for improving domestic labor conditions. But he is less willing to add that Public Law 78 became an institution essentially to benefit the farmers.

International events external to United States-Mexican relations are considered by Craig as crucial to the content and functioning of the program. He views Mexico as timing its demands for improved labor conditions to coincide with crisis periods (World War II and the Korean War), and the United States as waiting until after the crises to force compromise—clearly to favor agri-business, though Craig dilutes the point under the vague topic of national interests.

Though strong domestic group pressure bought various extensions for the bracero program, the disproportionate access to decision makers enjoyed by employer interests is not considered by Craig as detrimental to foreign policy. Rather, he interprets the subsequent demise of the program in terms of countervailing powers. That a small minority of southwestern growers could prolong for years a program deemed injurious to labor is not viewed as a cause for alarm about the democratic process. Craig explains its longevity by the particular group processes surrounding Mexican labor, with the superior forces representing the general interest eventually counteracting the impact of the growers.

The main reasons Craig offers for the growers' effectiveness are the control of the agricultural committees by the pro-bracero congressmen and the congruence between grower interests and that of the Mexican government. But it is the access of the anti-bracero forces to the secretary of labor that the author considers as decisive. The labor secretaries, with the contradictory role of recruiting Mexican labor and protecting domestic farmworkers, eventually moved to counter the negative impact of what Craig labels as increasingly unpopular legislation.

Yet Craig should have given more importance to access to the U.S. President. Without Eisenhower's support Secretary Mitchell failed to restrict the bracero program. But the next labor secetaries carried out President Kennedy's determined efforts to begin phasing out the program. Kennedy was drawn to this issue because of his combined interest in labor, ethnic, and humanitarian groups. When Kennedy started to cultivate support in 1958 for his presidential bid, he was reminded about the bracero issue in his meetings with Chicanos in the Southwest—a matter not touched upon in the book. By this time, Cesar Chavez was already organizing farmworkers north of Los Angeles to protest against the inequities of the bracero program by field sit-ins, marching, and the burning of work cards issued to domestic workers. The impact of such organizing activity and the campaign dialogues are hard to assess neatly by means of interest-group analysis, but might have influenced the importance given the issue by the Kennedys, who went after the Chicano vote.

It is somewhat deceptive to talk about Mexican farm labor by focusing, as Craig does, on international agreements and national legislation. Mexican labor has been an integral part of the development of the Southwest, not for twenty-two years, but for more than one hundred and twenty-two years. The author fails to note the cooperation and collusion of employers with the immigration authorities and other officials that has allowed a flow of Mexicans, legally or otherwise, to fill farm needs at lower cost, with inhuman, forced repatriation when domestic economics dictated.

The binational framework of the study is also questioned. The grower knows no nationality when seeking labor that comes cheaper. The Mexican worker knows no border when choosing between little at home or exploitation further north. The purchaser and investor in farm goods deals as freely in northern Mexico as in the Southwest. And the Mexican government can consider exporting crops or sending workers. Agriculturally, we are talking about a larger and common Southwest, to a great extent developed out of the exploited skills of the Mexicans and their descendents. Even many members of the United Farm Workers (UFW) are Mexicans with permanent resident status ("green-carders").

Interest group theory does not provide an explanation for the irony, noted by Craig, that minimal protection for imported workers served to improve some working conditions for domestic farmworkers. Neither does the author convincingly explain the lobbying success of the growers in terms of their supposed superior organization. What does seem to be the case is that the business community has many options allowed it by our system of government and economics to offset occasional restrictions imposed by officials friendly to labor. That is why growers have lost little with the demise of the bracero program. The program may be gone, but labor importation continues in other ways and working conditions remain about the same. At the same time, profit-interest groups, with the aid of the Nixon administration, are intensifying their opposition to the threat of farmworkers unionization which so often in the past ninety years has been suppressed with violence.

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The Literature of Isolationism: A Guide to Non-Interventionist Scholarship, 1930–1972. By Justus D. Doenecke. (Colorado Springs: Ralph Myles, Publisher, 1972. Pp. 89. $1.85, paper.)

In this guide Professor Doenecke attempts "to aid scholars venturing into the study of American isolation, pacifism and related ideological aspects of policy-making" (p. 5). He succeeds admirably. Doenecke's bibliography concentrates on the debate over American foreign policy in the crisis-ridden decade preceding Pearl Harbor. He serves up a rich fare of contemporary articles and books, historical works including numerous doctoral dissertations, and even a few Master's theses. Doenecke evaluates a wide selection of manuscript and oral history collections and includes information as to any restrictions on use of the material.

Professor Doenecke's commentary ventures into the literature of the opposition to the interventionist policy of the Cold War. The discussion of the works of these "neo-isolationists," however, is far thinner than that of the pre-World War II period. The compilation does not include the literature pertaining to isolationism in the 1920s.

Doenecke's bibliography is an excellent starting place for any student of the 1930s and 1940s interested in the roots, ideas and fate of isolationists and pacifists. The author's commentary and citations are helpful to scholars interested in the transformation of the American Right in the middle third of the twentieth century. Especially intriguing is the phenomenon, alluded to by Doenecke, that sections of the Right and Left shared opposition to the Wilsonian policy of global intervention which has dominated American foreign policy at least since 1941.

FRANK COSTIGLIOLA
University of Rhode Island

The Politics of Assimilation: Hegemony and Its Aftermath. By Charles Doran. (Baltimore: The Johns Hopkins Press, 1971. Pp. 223. $12.50.)

There appears to be an inverse relationship between academic theorists and statesmen in their response to power. E. H. Carr highlighted the relevance of power at precisely the time when British and French statesmen held it at a discount. Hans Morgenthau was reviled by many of his colleagues for claiming international politics to be a struggle for power at the same time that statesmen embraced his doctrines. Today, while diplomats are seeking new currencies of international politics, there has been a recrudescence of academic interest in power politics. Hinsley, Organski, North, Singer and others have added their names to those who seek to account for basic changes in the international system in terms of changes in power relationships.

Charles Doran's name can now be added to their number. Briefly stated, his interesting work argues that the relative power of a state in actualized and latent senses determines the degree of hegemony that it seeks. The power of a state may be considerably modified, however, in conjunction with the termination of a major war or attempt at hegemony. If peacemakers are sensible, they will design a peace which rehabilitates the former hegemonic aspirant while containing his power within strict limits. At Westphalia in 1648, the peacemakers succeeded in breaking up the Hapsburg family complex that controlled both Spain and Austria. In so doing they put an end to the Hapsburg threat, but also practically eliminated Spain as an international power. The aspiring hegemon was overassimilated to the system, paving the way for a power imbalance that led France to become the new disturber of the peace. Louis XIV's France, however, was not so disciplined at the peace of Utrecht in 1713 that she could no longer harbor hegemonic ambitions. Indeed, Napoleon's move to empire in 1800 was sanctioned by the Utrecht settlement. France remained the premier power of Europe during the eighteenth century, and Napoleon merely capitalized on that power.

In 1815, however, hegemonic France was correctly assimilated to the system. France was rehabilitated, but her power diminished. The rise of Prussia-Germany in the latter part of the nineteenth century could not be ascribed to a failure at Vienna; it was based on relative power development following the peace treaties. After 1914, however, Germany was under-assimilated to the system, and her undiminished relative power was the cause of World War II. After World War II, German military predominance was eliminated.

Professor Doran argues that imbalances in latent war potential are even more important than imbalances in actualized military forces. Until 1890, France was the dominant state in Europe in terms of existing military forces, but her latent economic and industrial power had been surpassed by England and Germany long before. Indeed, Doran contends that attempts at hegemony are most likely to be engendered just before and just after a power reaches its peak in relative latent war potential. This phenomenon appeared to occur in the two French cases (pre-1713 and pre-1815), though it did not fully apply to the two German cases. (Both German attempts at hegemony can only be said to have been made during the downward slope of German relative latent power.)

If this theory is correct, one has only to look for the nation whose relative latent power dominance is in the making: this nation will be the aggressor of the future. Unfortunately, however, the conclusion is by no means so straightforward as Doran implies. Alliances modify the power equation. In the seventeenth century it was the alliance of Spain and Austria, not the individual power of either which determined the issue. A number of medium-sized powers firmly aligned together could present difficulties that none of their members would offer individually. Even where alliances do not compound the problem, however, it is *not* true that relative latent power points to the aggressive or hegemonic state. From 1740–1763, the most aggressive state on the European continent was Prussia, one of the weakest of the great powers in either latent or actualized terms. Conversely, by 1850–1860, Britain had attained a superiority in relative latent war potential that should have led her to assert her hegemony in military terms; it did not. By 1914, the United States of America produced twice as much steel as Germany and five times as much as Great Britain. Her margin in coal production was almost as great. Yet the United States did not become embroiled in a quest for world hegemony at the beginning of the twentieth century. And it was Germany, a power whose sun was already near the horizon, which precipitated World War II, not the United States. In short the crucial difficulty with Doran's book, as with similar attempts, is that it tries to reach conclusions by means of estimates of crude power; it inquires not at all into the means of exercise of power. Here is where the key to war may be found.

RICHARD ROSECRANCE
Cornell University

Holocaust or Hemispheric Co-Op: Cross Currents in Latin America. By William O. Douglas. (New York: Random House, 1971. Pp. 215. $5.95.)

To analyze some of the causes of underdevelopment in Latin America, particularly as they relate to United States foreign policy, to respond to the conclusions of Governor Nelson Rockefeller's Presidential Mission to Latin America (*The Rockefeller Report on the Americas,* Chicago: Quadrangle Books, 1969), and to offer a model for development accompanied by policy prescriptions for Latin Americans and North Americans are the objectives Mr. Justice Douglas has set for himself in *Holocaust or Hemispheric Co-op*. First and foremost, Mr. Douglas expresses a post-industrial disillusionment with economic growth and increased consumption. Implicitly he defines development in terms of increased employment and social welfare. He faults the United States, the Soviet Union, Western Europe, and Japan for offering a model of development based on increased consumption of packaged goods and an industrialization based on mechanization and automation, both of which are said to lead to unemployment. This basic assumption colors the book's analysis of United States foreign policy and activities in Latin America and its presentation of an alternative to the region's present course of development, the two substantive areas in which the author pursues his objectives.

The book's analysis of United States foreign policy and activities in Latin America is mixed in quality. It presents a standard post-Vietnam catalogue of sins including overemphasis on anticommunism and security, misdirected economic aid, and unjustified intervention by the U.S. in the internal affairs of Latin American countries. Douglas calls for an end to all military aid and withdrawal of all military assistance missions. He refers to the Alliance for Progress as "a new version of an old American fable: that the triple formula of private enterprise, grassroots democracy, and the family farm will make every people happy" (p. 142) and condemns the aid extended for bankrolling oligarchies and impoverishing the recipients with debt servicing requirements.

However, Chapter VII, entitled "The Shadow of U.S. Industry over Latin America" is an excellent, nontechnical, nonhysterical, nonideological discussion and analysis of how American investment and foreign policy have promoted underdevelopment by perpetuating a pattern of trade in which Latin America exchanges raw materials sold at declining prices for manufactured goods imported at sky-rocketing prices. He explains and demonstrates with selected, documented statistics how the United States has maintained this situation with policies ranging from tariffs which discriminate against exports of manufactured products from Latin America, through the Hickenlooper Amendment's requirements that aid and credits be cut off to any country expropriating a U.S. investment without compensation.

Douglas's alternative model of development for Latin America is a version of the Chinese experience. Village renovation, land reform, labor-intensive industrialization, agricultural development, and education geared for basic technical skills and trades instead of preparation for the university are put forward as the keys to mass improvement. Reluctantly, Douglas admits the need for a parallel industrial revolution incorporating the most advanced technology to satisfy political and economic aspirations already created. He notes that the key question in this area will be the ownership of the nearly workerless machines. Unfortunately, he does not tell us how his preferred forms of growth will survive this admittedly more potent economic competition or resist the ambitions of a technocratic elite.

In the final section the author presents his policy recommendations. He urges the United States to disengage militarily and economically and to end all forms of trade discrimination. He praises Latin American integration as practiced in the Andean Common Market, the controlled and balanced exchange of marketing and production opportunities to stimulate industrialization. To the hemisphere he recommends establishment of true balanced interdependence involving tolerance of ideological pluralism and the exchange of mutually beneficial economic opportunities, initially among Latin American countries but eventually including the United States. His hemispheric co-op is a controlled common market in which economic planning and political agreement assure reciprocity.

What Mr. Justice Douglas has sketched here is by his own admission a romantic vision intended to provoke discussion. Its contradictions and uncertainties reflect the misgivings of both North Americans and Latin Americans as they confront a technological and social revolution neither side understands. In the final analysis this book is a welcome break with two schools of thought: the omnipotent-paternalism school of foreign policy studies which describes how the United States will design programs for sweeping changes and incidentally secure the region against hostile forces, and the technological-utopian school of development studies which maintains that Latin America must and will inevitably be made over in the image of the North Atlantic, Japan, or in its Marxist version, the Soviet Union. For both of these reasons it deserves critical attention.

EDWARD S. MILENKY

Boston College

The Almanac of World Military Power. Edited by T. N. Dupuy and Wendell Blanchard. (New York and London: R. R. Bowker Company, in association with T. N. Dupuy Associates, 1972. Pp. xii, 373. Maps, Glossary, Index of Countries, Regions, and International Organizations. $22.50.)

For many years anyone interested in international and military affairs has been able to rely upon publications as *The World Almanac, The Statesman's Year Book*, Brassey's *Annual*, Jane's *Fighting Ships*, and Jane's *All the World's Aircraft* for basic geographic, political, economic, and military information on the states of the world. More recently *The Military Balance* and *Strategic Survey*, each published annually by the International Institute for Strategic Studies (London) provides "intelligence" data and analyses said to rival those of most advanced government services. In addition, the State Department's *Background Notes* provides basic information on all the states of the world, updated frequently for each state.

These and other similar reference works and press reports are the main sources for this *Almanac of World Military Power*. Its advantage is that the compilers, experienced military historians and political scientists, have sifted this information from such various sources, and have organized it in a systematic way to serve well demands for quick reference and needs for comparative studies. Its disadvantage is that in today's rapidly changing world, much of the information is obsolete before it is published. This, the second edition of the *Almanac*, appears only two years after the first, and that represents an admirable attempt at currency. But the information upon which it relies must have been already a year older when it was collected, and several months older when published, although the editors have attempted to record major changes through July 1972. The articles themselves, written by a group of eight writers, are carried over from the first edition, with revisions of data as indicated.

This is a useful compilation, but does not replace *Background Notes*, or the *World Almanac*, or *The Military Balance*, or *The New York Times* for current information. Perhaps the greatest use of the work can be made in the college classroom and military schools where it can serve as the basic source for international and strategic studies, and in particular for academic war games.

The volume is organized according to ten geographical regions: North America, Central America and the West Indies, South America, Western Europe, Eastern Europe (including the Soviet Union), Middle East, Africa, Central and East Asia, South and Southeast Asia, and South and Southwest Pacific.

Within each regional section, the states are listed alphabetically, and the information for each is organized according to the same general headings, as appropriate: Power Potential Statistics, Defense Structure, Politico-Military Policy, Strategic Problems, Military Assistance, Alliances, Army, Navy, Air Force, Paramilitary. In addition to the basic statistical data, the short essays provide valuable background and explanatory material.

JAMES A. HUSTON

Lynchburg College

Arms, Yen and Power: The Japanese Dilemma. By John K. Emmerson. (New York: Dunellen Publishing Co., 1971. Pp. 420. $15.00.)

The rise of Japan, within three decades, from the ruins of defeat to economic superpower status, surely must rank as one of the major events in this century. Yet, somehow Japan has remained "a strangely invisible giant," as Edwin O. Reischauer so aptly puts it (p. ix) in his foreword to this excellent study. Perhaps this is not surprising, for in international affairs the Japanese have chosen to be passive observers rather than actors. Without arms and seemingly also without any ambition to wield influence, they have consistently retained a low profile and posed no threat to their neighbors.

Paradoxically, the very success of postwar Japan's inward-looking policy is now compelling it to play a more active international role. Today, the economic power of Japan is felt throughout the world. Increasingly, Japanese interests are enmeshed with the fate of other nations. Meanwhile, the U.S.-Japanese alliance has entered an era of readjustment, and Japan's relations with China and the Soviet Union are in a state of flux. At the same time, the mood of Japan favors a more independent stance in world affairs. What direction the Japanese will take in the years to come, and how they will employ their impressive material and intangible resources are questions of profound significance for the future not only of Asia but also of the United States.

The author of this book set himself the task of exploring these important questions. He is exceptionally well qualified to do so. Recently retired from the U.S. Foreign Service, John Emmerson began his diplomatic career in Japan on the eve of the Second World War and has been intimately concerned with Far Eastern affairs ever since—a background that lends his work exceptional strength. This solid research study draws on a wide selection of Japanese and Western sources, supplemented by the writer's personal experience in Japan, where he served as a young language officer, as vice consul, Third Secretary, and in the 1960s as Deputy Chief of Mission. The account is free from the inaccuracies, mistranslations, distortions, and glib interpretations so often encountered in books on Japanese affairs. In analyzing the complexities of postwar Japan, the author shuns dramatic forecasts. He does not claim to possess all the answers, but offers the reader a well-organized body of relevant information (including useful statistical tables). From this base, he draws a set of sensible conclusions regarding probable courses the Japanese might take in the future.

The study is divided into four parts. The first and shortest of these provides the setting for the subsequent detailed information of trends and forces expected to determine Japan's future role in world affairs. Succinctly, the author presents a survey of the domestic political scene, evaluates the factors responsible for the Japanese success story and identifies the problems now faced by Japan. Even looking with the benefit of hindsight—the book was written before Tanaka succeeded Sato as prime minister—we find that the author's interpretations stand up remarkably well. If there is any flaw in this introductory section it is its brevity. The author attempts perhaps too much in compressing an analysis of Japan's domestic condition into a few pages. As a result, he can say little about the decision-making process in Japan, the characteristics of the bureaucracy, the significance of the cleavages within the ruling party, or the outlook of the young generation. These omissions are regrettable, because the author's personal experiences would no doubt have made such discussion particularly valuable.

The second and most substantial part of the study is devoted to Japan's security problem. Much has been written on this subject, but probably no other single work can give the Western reader so detailed and lucid an account of Japan's national security policies and programs or so careful an analysis of the dilemma faced by Japan as it seeks to reconcile its "no-war Constitution" with greater independence from the need for foreign protection. In reading these chapters one is struck by the absolutely crucial role the American factor plays in Japan's national security policies. Hence this part of the study concerns itself as much with U.S. actions and policies as with those of the Japanese side. In the process, the reader also gains a better understanding of the nature of Japan's American alliance and of its present difficulties.

The third part of the book constitutes a *tour d'horizon* of Japan's relations with its non-American neighbors: China, the Soviet Union, Korea, and Taiwan, as well as Southeast Asia. The author did not anticipate the solving of at least one aspect of Japan's "China problem" by the rapid resumption of diplomatic relations between Tokyo and China. Nevertheless, his analysis of the basic features of the interaction between Japan and its neighbors appears sound even under the present changed conditions.

The final part of the study analyzes the major problems confronting Japan in the 1970s and the role Japan might be expected to play. The author views the questions of Japanese rearmament, i.e., of nuclear weapons, and the future of U.S.-Japanese relations as the two vital—and interrelated—issues for the coming decade. He appears convinced that the Japanese have learned from their past experience and will take up the nuclear option only in an international environment that leaves them little choice to do otherwise. His analysis suggests that a remilitarized Japan is unlikely to emerge during the coming decade. But neither will the Japanese be acting as an "economic animal": their international involvement is now too deep for that. The author admits (p. 398) that "there could be many scenarios of what Japan *may* do and what Japan *should* do in the 1970s, but no one, Japanese or American, can make such judgments with certainty." He limits himself therefore to a more modest listing of the probabilities in the 1970s, pointing at the same time to those international developments which could upset these estimates. If any single important point emerges from these concluding remarks, it is that "the essentiality of our relationship with Japan is absolute" (p. 403). Although written two years ago, this statement retains its validity. As the author suggests, the policies of the United States may well determine whether Japan will be able to demonstrate that power need not come out of the barrel of a gun.

PAUL F. LANGER

The Rand Corporation

NATO in the Seventies. Edited by Edwin Fedder. (St. Louis: Center for International Studies, Univ. of Missouri, 1970. Pp. 215. $2.75, paper.)

NATO in the Seventies, edited by Edwin Fedder, is the result of a conference held at the University of Missouri at St. Louis in April 1969 to mark the twentieth anniversary of NATO, and to see where the Alliance was going militarily. Most of the participants were American, although there were several Europeans present. All were "out of government," but some had served at NATO, most notably Timothy Stanley, who had just returned from having served on the U.S. Delegation to NATO.

The most controversial paper is that of Jules Moch, former French Defense Minister, who posed two questions: Was NATO necessary, and was Germany rearmament necessary? The answer to the first is affirmative (although he points out that NATO has not succeeded in achieving a true alignment of member states' foreign policies under a strategy that could have led to global war). But Moch questions the wisdom of rearming West Germany—rather he argues that it was not desired by the German people themselves, and was unnecessary for the defense of the West in the 1950s, given American nuclear superiority.

As to the other papers, most reflect the perceptions of European/American relations, which are now in a state of flux caused more by the American diplomatic and economic initiatives of the Nixon/Kissinger foreign policy than by initiatives of the other Alliance members, including France. Oil and the most recent Middle East crisis have only exacerbated these relations.

Bernard Morris summarizes the overall situation in a timely way when he writes:

> The Soviet Union and the United States are pursuing two major strategies which are at the same time complementary and conflicting. On the one level, they are pursuing a neo-Yalta strategy that is based squarely on mutual power interests and the recognititon of respective security spheres. On the second level, they pursue an alliance policy that pits western bloc against eastern bloc. As an instrument of mutual pressure, the alliance policy tends to undercut the Yalta policy. At the same time, by reinforcing spheres of interest and providing military balance, the alliance policy complements and even reinforces the Yalta policy (p. 29).

The analyses of West Germany by Lewis Edinger and of France by Edward Kolodziej stand up well in the interval since they were written. France especially, as Kolodziej points out, emerged in the 1960s as instrumental in setting the stage for the "Eastern policies" of West Germany and the United States in the 1970s. Walter Goldstein's paper pretty much places Britain in the role of a "Middle Power" moving toward Europe and away from the United States, and heavily dependent on its financial condition as a prime determinant of its future role in international politics. Three papers—those of Paul Schratz, Richard Rosecrance and Alden Williams—deal directly with NATO, and as such perhaps suffer from appearing overtaken by the rapidly evolving bilateral and multilateral diplomacy now taking place between and among the two European power blocs and their respective superpowers.

The summary papers by Donald Puchala and Edwin Fedder are less policy-oriented prognostications and more generally analytical, both stating clearly the need for NATO to adapt to the rapidly changing international political environment in which it finds itself, or else atrophy and eventually go out of business. In connection with Edwin Fedder's paper, one should take note of an expansion of his thoughts in his book NATO: *The Dynamics of Alliance in the Postwar World* (New York: Dodd, Mead, & Co., 1973).

Many published reports or conference papers have proved short-lived (rather like books published by vanity presses); this report does give the reader a shorthand summation of the major issues facing NATO, and the East and West generally as regards Europe. Even though contributions to the book possess aspects of the "period-piece" as they discuss highly dynamic international political conditions, the larger perspectives are still germane.

ROBERT S. JORDAN

SUNY, Binghamton

The International Coffee Agreement: A Study in Coffee Diplomacy. By Bart S. Fisher. (New York: Praeger Publishers, 1972. Pp. 310. $17.50.)

After petroleum, coffee is the second most important commodity (in dollar terms) in global trade. Coffee is big business and is important to the producing countries, where for many, it constitutes the single most important earner of foreign exchange. In the consuming countries, large corporations and trading companies are involved in the transformation of coffee into roasted and instant (soluble) coffees. Finally, the consumer of coffee wishes to get the best quality at the lowest possible price.

Even though coffee is a major commodity in world markets, the coffee market itself has, over time, been very unstable. The instability has been in the supply of coffee (as opposed to demand) and comes about because of intermittent frosts in the largest producing country Brazil combined with a yield variability of the individual coffee tree worldwide. The unpredictability of coffee supplies led to great price variability in wholesale and retail markets. The book by Dr. Fisher, a Washington, D.C. attorney, is a record of an international effort to introduce production and price stability in the global coffee market.

Dr. Fisher begins his book with a historical summary of the numerous early attempts to control the coffee market by Brazil; he also records the reactions of the U.S. government to these attempts at control. An early agreement by the U.S. to participate in a minor control agreement (1940–1945) was followed by a period in which there was virtually no regulation (1945–1954).

A series of bilateral and multilateral control agreements among the Latin American producer countries helped to set the stage for an International Coffee Agreement. By the year 1958, the interests of the producing countries were clear: controlled markets which would assure export earnings over time and would allow for orderly economic development programs. The U.S., as the major consuming country, changed its position on commodity agreements as a result of the cold war and the potentially explosive political situation in Latin America. Powerful consumer country industrial interests worried about political upheaval as well: their concern however was that political upheaval could lead to coffee shortages and breaks in the supply chain. As Fisher notes (p. 22): "The mix of industry sentiment in the United States, then, included self-interest (assurance of supplies), ideology (an anti-Communist Latin America), and affection. It appeared to add up to a majority of the U.S. coffee 'establishment' in favor of some sort of international regulation of the world coffee market."

Political developments within the United States contrived to favor participation in a commodity agreement. President Kennedy's Alliance for Progress finally opened the way for U.S. participation in an agreement to stabilize prices. A recurring theme sounded by anticommodity agreement individuals in the U.S. was that an agreement would tend to raise coffee prices. Against this agreement the pro-commodity agreement forces argued that prices would not rise but would be stabilized and if prices rose the increases would be slight and could be regarded as a form of foreign aid.

Fisher notes that commodity agreements can be analyzed in three different ways: (1) by examining relationships between nation-states each pursuing its own policy; (2) by analyzing the behavior and activities of the group constituted to effectuate the terms of the agreement, i.e., the Secretariat of the Agreement; or (3) by examining the dynamics of the respective governments and the various interest groups within the country which are directly interested in the form and shape of the agreement.

Fisher chooses the first approach, focusing on relations between various nation-states. I would rather have seen more attention given to the manner in which the interest groups within the respective large producer and consumer countries (i.e., Brazil and the United States) have helped over time to shape national policy.

The remainder of the book deals with the difficulties involved in creating a workable agreement. Fisher clearly details the events which led to the initial Agreement of 1962 and the accommodations and compromises which allowed for minor price fluctuations and for some flexibility on member nation production quotas. Further attention is given to the way in which the evolving mechanisms were written into the Agreement of 1968. The growth of the African coffee producers is recorded as is the manner in which Brazil, Colombia and the other Latin American countries perceived and/or reacted to this new and growing force in the international coffee market. An entire chapter is devoted to an event which almost wrecked the agreement: the production of instant or soluble coffee in Brazil and its export in finished form to the U.S. and elsewhere.

Fisher has written a descriptive and interesting book which documents the ways in which one international commodity agreement has evolved. His book closes with a recommendation that the International Coffee Agreement be renewed in 1973. Unfortunately, at this writing (May, 1973), the Agreement is wracked by internal dispute and is virtually not working as intended. Consequently, renewal in the form which finally evolved in late 1972 does not appear likely when the renewal talks which are scheduled for September, 1973 take place.

ARTHUR J. CORDELL

Science Council of Canada

China and the Overseas Chinese: A Study of Peking's Changing Policy, 1949–1970. By Stephen Fitzgerald. (Cambridge: Cambridge University Press, 1972. Pp. 268. $19.50.)

This is not a book for the general political scientist, and even many Asian specialists will find it tedious reading. Nevertheless, it is a very significant study, and one hopes that even if it is not widely read, its thesis will at least be widely cited.

The problem involves Chinese attitudes toward nationality and national identity. The American policy of supporting Taiwan's propaganda efforts in Southeast Asia was based on the assumption that the Peoples Republic of China (PRC) was unwilling to give up its claim to the "Overseas Chinese" (persons of Chinese extraction, most of whom have been born outside China), just as Nationalist China had historically claimed all Chinese living abroad. These Overseas Chinese, in the minds of many policy makers in America and elsewhere, would remain "Chinese," by their own desires and by the intent of the two Chinese governments, and thus the alternatives were "Nationalist" or "Communist." Considerable information has been available on the policies and practices of the Kuomintang (KMT, or "Nationalist") Government for some time, and it is clear that the KMT was, and remains, reluctant to recognize any change of nationality. The KMT permits Overseas Chinese representation in its legislative assembly; the Overseas Chinese Affairs Commission in Taipei is large and active; Overseas Chinese education (in Chinese) is encouraged, supported, or controlled (depending upon the attitude of the particular local government); and Overseas Chinese are encouraged to visit and maintain contacts in Taiwan. In short, we have known that Taiwan's policies were intended to keep the Overseas Chinese "Chinese," and many journalists, policy makers, and scholars (with some notable exceptions, particularly of late), subscribing to what is often described as the "fifth column theory" (see pp. 1–2), have imputed similar policies to the PRC.

The importance of this study by Dr. Stephen Fitzgerald, formerly a fellow of the Department of Far Eastern History of the Australian National University and now Australian ambassador to the People's Republic of China, is that for the first time in English we are exposed to a mountain of evidence that disputes these assumptions. According to the author's findings, since 1956 the PRC has not claimed the Overseas Chinese as it own, and indeed in 1957 it began an active policy of "decolonization." Decolonization as a policy encouraged Overseas Chinese to opt, if possible, for the local nationality. It also discouraged Chinese-language education, encouraged the use of local languages, and generally urged minorities toward assimilation. For those who proved to be unassimilable, the PRC recommended that they not dabble in local politics and that if possible they consider repatriation to China, where they would be assimilated into the Chinese society of the contemporary mainland. In brief, the PRC looked forward to the time when those officials responsible for the implementation of decolonization would work themselves out of their jobs.

This study represents such a marked departure from the still popular views of the Cold War era that any critic must ask at least three questions of the study: What are the author's sources? How has he used these sources? And, finally, if these findings prove reliable, how can one explain such seemingly "un-Chinese" behavior?

The author has utilized the holdings of the Union Research Institute in Hong Kong, and he has drawn heavily from *Jen-min Jih-pao,* from *The Overseas Chinese Affairs Journal,* and from the Overseas Chinese press agency. Dr. Fitzgerald, an Australian, was also able to visit China briefly and to discuss his research with several helpful officials. In summary, it seems unlikely that the author has missed any significant and reliable material that might have been available, though it must also be recognized that scholars writing on contemporary China seldom if ever have access to more than a fraction of the total evidence in existence.

About the use of his sources I can make only two mildly critical observations. First, the author's prose is sometimes tendentious. He is obviously bent on setting the historical record straight, and he should be applauded for this, but this zeal will prove offensive to some readers. Second, Dr. Fitzgerald might have devoted more attention to the many discrepancies between the PRC's policies and her propaganda during this time. Peking's actions seem more in harmony with the former, but Peking's rhetoric also deserves closer attention. Nevertheless, I find the author's treatment of his subject thorough and convincing.

If we accept the validity of the author's conclusions, we must also ask, "Why"? Here, again, I think the author's analysis is sound, though I personally would have appreciated more of his own insights. The Overseas Chinese were simultaneously an asset and a liability to the PRC, and on balance they proved more costly than beneficial. They could not be utilized as a "fifth column," for the communities are not politically homogeneous, and many are simply apathetic about both Chinas. When the PRC, as a result of its pre-1957 policies, succeeded in gaining and holding the political allegiance of a Chinese minority it found itself facing requests for protection or patronage that could not be honored, and the results could be embarrassing. The overseas communities also sometimes blocked better state-to-state relations in much of Southeast Asia (which is the home of most Overseas Chinese and thus the major focus of this study). In most Southeast Asian states the domestic Chinese are not popular, and the sooner the PRC government could divest itself of this liability the better its chances would be of improving inter-state relations. Finally (and this line of reasoning Dr. Fitzgerald might have pursued further), the PRC probably had no alternative if it wanted to break the hold of the KMT over the domestic Chinese in many Southeast Asian states. KMT control of Overseas Chinese institutions emerged at the time of colonialism and early independence. In the Cold War environment many newly independent countries were willing (and some, eager) to see this control strengthened as an alternative to the threat of Communist control. As attitudes toward the PRC began to soften in Southeast Asia (and with few exceptions this has been a very slow process, in part because of the domestic Chinese minorities), most countries had launched programs of national development in which paternal foreign protection of a domestic minority, particularly an economically powerful minority, was anachronistic. Thus, even though some leaders might have been sympathetic to the PRC cause, should KMT control have been broken it would not have been transferred to Peking. In short, the policies Dr. Fritzgerald describes may have been un-Chinese in a traditional sense, but there seems to have been no viable alternative for the Peking government.

This is a significant, indeed outstanding, study. Southeast Asian political leaders should read it carefully, just as should all scholars, diplomats, and policy makers concerned with contemporary Chinese affairs.

ROBERT O. TILMAN
North Carolina State University

Aid to Russia, 1941–1946: Strategy, Diplomacy, the Origins of the Cold War. By George C. Herring, Jr. (New York: Columbia University Press, 1973. Pp. xxi, 365. $15.00.)

This is an excellent, readable study of the interaction of strategic, diplomatic, and political influences in the shaping of American policy for aid to Russia from 1941 to 1946.

Between the introduction and the conclusion, Herring, handling judiciously the influences of the times on other scholars and their preoccupations with the Cold War, rejects the notion that the United States attempted to buy Soviet goodwill and the more recent contention that the United States used its economic power to force the Russians to comply with American postwar designs.

The guts of this book is that many previous authors have ignored the context in which major decisions were made—"they have failed to judge the diplomacy of the period on its own terms" (p. xix). "Finally, both the revisionists of the forties and those of the sixties and seventies share an implicit acceptance of what D. W. Brogan has termed the 'illusion of American omnipotence.' They assume that the United States had the capacity to prevent the Cold War" (p. xix).

Like Stalin at one point of time, the author does not ever underestimate the American contribution to Soviet military victories, even though the $10 billion worth of equipment and supplies may have represented only 10 per cent of Russian production (p. 286).

In noting Roosevelt's deep concern about aid as applying to immediate strategic necessities, Herring comments on the abrupt and shortsighted policy of the United States in 1945 and 1946, and holds that Truman was much less sympathetic to Stalin's security concerns than Roosevelt was. American handling of lend-lease between V-E day and V-J day, he adds, did contribute to the rise of Soviet-American tension in 1945.

A small point. Herring reminded me of that puzzling rhetoric of Churchill's about lend-lease: "the most unsordid act in the history of any nation." And the author concludes: ". . . The story of American aid to Russia constitutes one of the most noteworthy chapters in the history of American diplomacy" (p. 293).

This sane book will hold its own for some time to come, but it will certainly arouse other scholarly pens.

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The Soviet Union and the Emerging Nations: A Case Study of Soviet Policy Towards India. By Harish Kapur. (Geneva: Michael Joseph, Ltd., for the Geneva Graduate Institute of International Studies, 1972. Pp. 124. £3.50. Distributed in the U.S. by Humanities Press, New York.)

The evolution of Soviet policy toward the Indian subcontinent has made a major contribution to the diplomatic realignments taking shape in Asia and the Far East. Testimony to the importance of India in Soviet Asian policy is the Soviet support of India in the Bangladesh crisis against a Pakistani-Chinese-American axis. Mutuality of Indo-Soviet interests has prevailed over various stresses in their relationship for now almost two decades—too brief a time span, perhaps, for crucial documentation to have become available, but time enough for a critical assessment of Indo-Soviet politics.

Professor Kapur provides a readable but not intensely analytical account of post-World War II Indo-Soviet relations from the transfer of political power to the Indian National Congress in 1947, to the late 1960s (before Bangladesh). His previous volume, *Soviet Russia and Asia,* 1917–1927, a study of Soviet policy toward Turkey, Iran and Afghanistan (noted by Robert A. Rupen in *APSR,* 61. [September, 1967] 831–832), dealt with Communist strategy toward national revolutionary movements within the context of the requirements of the security of the Soviet state, much along the lines of E. H. Carr's concept of "dual policy." For all its involvement in Asia, the Soviet leadership continued to be primarily European-oriented until the aftermath of World War II, when it involved itself progressively in Asian politics in the late 'forties. The Soviet leadership chose to treat the independence of the former colonies as a sham, a neocolonialist ruse for continued imperialist exploitation in new conditions. It was a line that complemented the hardening of Soviet policy toward Western Europe during the Cominform period within the framework of which the Western European Communist parties attempted to obstruct and sabotage the Marshall Plan and rearmament, and the Asian parties moved to insurrection and civil war. This "forward" strategy toward the newly-independent states played itself out in the early 1950s; within a brief few years the turn of Soviet strategy brought the U.S.S.R. into cooperation with the nonaligned movement of which Nehru was a prime mover.

The Indo-Soviet connection appears to have pivoted on the seizure of power by the Chinese Communists, first by influencing India's shift from a prowestern to a nonaligned policy, and subsequently by moving the Soviet leadership into closer collaboration with the Indians, as the Soviet cleavage with China developed. Yet it was the United States' policy of containment (or containment-plus)—the decision in 1954 to supply arms to Pakistan and enroll her in the Southeast Asian Treaty Organization—Professor Kapur suggests, that gave the Soviet leadership the leverage to embark on a policy of friendship and cooperation with India. (The Russians seem to have been the chief beneficiaries of John Foster Dulles's policy; cf. Egypt and the Aswan dam.)

In general, Professor Kapur awards high marks to Soviet diplomacy on the subcontinent for its flexibility in attempting to maintain good relations with both India and Pakistan and achieving its limited objectives in counter-balancing China. As for India, the author holds that its stock has declined since the Sino-Indian War of 1962, when it became evident that India would not be an effective counter to rising Chinese influence in the area. Although India no longer occupies the central position in Soviet strategy that she did during Khrushchev's tenure, Professor Kapur concludes that she remains an important factor in international relations and in Soviet policy. Who can quarrel?

The volume recommends itself as a fair and readable overview of Soviet policy towards India but it does not add much to J. A. Naik's *Soviet Policy Toward India, from Stalin to Brezhnev* (Delhi: Vikas Publications, 1970), which covers much of the same ground in a more detailed and analytical manner. The latter's account of Sino-Soviet relations and India is, for one, more substantial. And as Professor Surendra K. Gupta noted in his review (*Problems of Communism,* 22 [May-June 1973] 65–68), Dr. Naik's linkage of Soviet economic aid with India's responsiveness to Soviet overtures in the 1950s adds an important dimension to understanding the evolution of the Indo-Soviet relationship. Of the two books, a specialist on Soviet (or Indian) policy would find the Naik book more useful.

BERNARD S. MORRIS

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The Limits of Power: The World and United States Foreign Policy, 1945–1954. By Joyce Kolko and Gabriel Kolko. (New York: Harper and Row, 1972. Pp. xii, 820. $15.00, cloth; $5.95, paper.)

In *The Politics of War,* published in 1968, Gabriel Kolko produced the most thorough and extensive of the "revisionist" views of American foreign policy during World War II. Kolko's interpretation of America's wartime policy flowed from his fundamental thesis that the need of American capitalism for a compatible world political economy was the basic motivating force of United States policy, which led directly not only to repression of the Left in Europe but to the intense conflict with the Soviet Union known popularly as the "Cold War."

In *The Limits of Power,* Gabriel Kolko, Professor of History at York University in Toronto, and Joyce Kolko have carried forward this interpretation by writing an orginal and heavily documented analysis of the first postwar decade. It is not, however, a history of the "Cold War," which they regard as an "egregious term that burdens one's comprehension of the postwar era with oversimplifications and evokes the wrong questions" (p. 6). Indeed, the Kolkos do not regard the American-Soviet confrontation as the central fact of the postwar world. On the contrary, they believe that the most important force shaping the world of 1945–1954 was the search of American leaders for a viable political strategy that would implement their plans for a world economic order in which United States capitalism could survive and prosper. Anti-communism and anti-Sovietism became convenient rhetorical devices by which the Truman Administration successfully gained support in Congress and among the public for its basically economic objectives. Thus United States leaders discovered in 1946 that Congress could only be induced to vote a loan to Britain—designed to integrate the Sterling Area into the American-dominated economic order—by fear of Russia and communism. Later, anti-communism was used to get congressional approval for the Truman Doctrine, although Russia was by no means the source of the problems with which that Doctrine attempted to cope.

In presenting the Marshall Plan to Congress and the American public, the Truman Administration also "used a Red Scare and crisis atmosphere in a distinctly Machiavellian way to secure its economic aims, not because it feared a danger of the Soviet Union in Europe, but because it feared the danger of a budget-conscious Congress more or less responsive to an indifferent American public" (p. 376).

This is the central theme of *The Limits of Power,* although accompanied by a secondary theme expressed in the title of the work: the ultimate inability of the United States to control history, particularly "the ascendant movements of change we may loosely associate with the Left" (p. 3). The "basic source of the world crisis was an expansive American foreign policy's interaction with revolution and national autonomy, capitalist as well as socialist" (p. 714); yet in its "monumental undertaking to contain and reconstruct the world according to its own needs, the United States was prepared to destroy itself—morally, socially, and economically—in a deepening trauma whose effects began to weaken American capitalism far more than the attainment of its expansive, unattainable goals might ever have strengthened it" (p. 716). The interpretations offered by the Kolkos of particular events—in Latin America, the Middle East, China, Korea, and Japan, as well as Europe—reflect this theme of United States willingness to use its power to serve its economic objectives, and the ultimate failure of this attempt.

This is revisionist history with a vengeance, and *The Limits of Power* is therefore highly recommended as a stimulating challenge to orthodox historiography. Stimulating or not, however, the question remains: Is it good history?

The Kolkos are quite self-conscious about the two major premises of their historical approach: that the United States can properly be viewed as a unitary, rational actor in the world arena; and that its "fundamental" motivations for major policy decisions in the period under consideration were economic.

> [United States] policy and practice has been deliberative and quite consistent because the goals, structure, and requirements of the United States' social system have remained durable throughout the period. The notion of American leadership's errors or misperceptions is reassuring to those who believe the society can be redeemed merely by abler, superior men. . . . But articulate men, with a clear sense of purpose and objectives, invariably are at the helm; even their obfuscations often have reasons. And where the insight and articulated purposes of actors do not suffice, descriptions of the constancy and patterns in their actions will (p. 8).

The last two sentences of this quotation reveal the most serious difficulty with the Kolkos' historical method. When policy makers say what the Kolkos believe *a priori*—that their decisions were motivated by economic requirements—their words are quoted as evidence. Where they say something different, their "obfuscations" may be quoted as evidence. Where they say the opposite, the Kolkos ignore their statements but point to the "constancy and patterns in their actions." With this triple chance at "verification," "evidence" for the Kolkos' hypothesis can always be found. But if the hurdles that the hypotheses must cross are so low, how seriously should we take the conclusions? Where policy makers justify their actions not only to the public but to each other in military-political rather than economic terms, powerful rather than weak evidence for an opposite view is particularly necessary.

No such evidence is presented in *The Limits of Power*. Instead, the Kolkos' characteristic technique is simply to juxtapose statements indicating concern about economic conditions, at home or abroad, with statements about foreign policy, merely asserting that the former reveal the motivation for the latter. Thus the proposition that the Truman Doctrine reflected "the crisis of the world economy" is supported by simply asserting that "what was really on the mind of the President and his advisers was stated less in the Truman Doctrine speech than in private memos and in Truman's March 6 address at Baylor University" (p. 341) on foreign economic policy. *A priori*, economic problems must be the chief motivations for political actions. Whereas Hans J. Morgenthau, a quarter-century earlier, "looked over the shoulders of statesmen" and saw only the pursuit of the national interest, the Kolkos peer over those same shoulders and see only the interests of capitalism. The principles of monocausal history are the same, although the interpretations are diametrically opposed.

Kolko and Kolko present the same kind of "evidence" about the motivations for rearmament in 1949–50. The authors contend that the failure of the Marshall Plan to revive the European economies led to decisions in 1949 and 1950 to solve the "dollar gap," and therefore to finance the exports necessary to maintain the American economy by massive rearmament. The review of American military and political objectives and requirements in 1949–50, "NSC-68," is interpreted in this light, although absolutely no evidence is presented to indicate that its drafters were aware of the "crisis of capitalism" or that they were responding to it. The preponderance of the evidence, indicating that security concerns were paramount in policy makers' minds, is simply ignored. The Kolkos merely imply that the State Department's request for a $50 billion defense budget reflected the fact that it was "far more keenly aware (than the JCS and Defense Secretary Johnson) of the world economic trends and armament's role in advancing United States economic goals in Europe" (p. 508).

The Limits of Power, although poorly written, is an interesting book precisely because of its revisionism. It calls attention to economic problems and economic motives, which have indeed been frequently ignored by traditional strategic interpretations of the early postwar period. Its interpretations are often ingenious and arresting: the reader is certainly enticed by "the failure of the Marshall Plan" or an account of Syngman Rhee's "retreat to victory," by which he allegedly drew the United States into Korea in force when he might well have been able to repel the limited North Korean attack with his own forces. Yet in the end this volume is disappointing because its authors so often rely on interpretation by assertion. It is not surprising to learn that American leaders were concerned about domestic economic issues and their relation to foreign economic problems. But quotations from American leaders about the importance of an open, integrated world economy, which the United States certainly favored, do not indicate that the principal architects of postwar American foreign policy—Acheson, Marshall, and Truman—based their policies fundamentally on economic considerations, much less that they attempted to cope through foreign policy with a crisis of American capitalism. That they did not ignore economic issues has become evident. Yet that the "overriding social reality" of America as a highly stratified, capitalist society "determines the definition and implementation of American foreign policy in the world" remains unproven. Postrevisionist history will need to avoid relying entirely either on the alleged necessity of responding to the "Soviet threat" (as in much conservative literature) or on the "crisis of American capitalism" as the "key" to contemporary history. As Acheson says about himself, historians have made their points "clearer than truth." It is to be hoped that the sophistication of truthful complexity will now replace the ingenuity of spurious clarity.

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Détente in Europe: Real or Imaginary? By Josef Korbel. (Princeton, N.J.: Princeton University Press, 1972. Pp. 251. $10.00.)

Détente in Europe covers familiar ground for anyone who has followed European affairs for the past ten years; it is balanced, if altogether conventional, in its conclusions. As a former diplomat, Professor Korbel, not surprisingly, stresses the political and security issues of détente. He includes a chapter, however, on the importance of economics for East-West relations. He concedes the importance of economic motives in the policies supporting a European détente, but remains skeptical about the prospects for extensive developments in economic ties between eastern and western Europe. Germany remains, as in the origins of the Cold War, the central issue of détente. For Korbel, the difficulty of resolving the German question and the continuing ideological differences in Europe make détente, however real, uncertain and perhaps illusory.

The difficulty that one has with this book is not with its contents, which are obviously carefully researched and thoughtfully examined, but with the author's approach to the subject. The point of view is that of the lingering issues of the Cold War. Here once again are all the reminders of the deviousness and aggressiveness of the Soviet Union, the divisions within NATO, and the unfavorable contrast between the societies in eastern and western Europe. The preoccupation with "East-West relations" misses the far more fascinating changes that are taking place in Europe. Nationalism, as Professor Korbel points out, has been an important motive supporting policies of rapprochement in Eastern Europe. He might have pointed out that it has been equally important in Western Europe. The nation-state has been rejuvenated, not superseded, by the European communities. Moreover, historic political and economic ties between eastern and western Europe have once more reasserted themselves. European history, as de Gaulle always pointed out, did not begin in 1945. Indeed, the shape of the new Europe may well embrace a rejuvenated nation-state system including both halves of Europe. The idea of a Europe "from the Atlantic to the Urals" may not be so far-fetched after all.

Furthermore, the concentration on the problems of the Cold War obscures perhaps the main force presently encouraging integration in Western Europe as well as détente with the East, namely the threat of an overweening American economic power. The United States, which originally supported plans for European integration and then détente, now finds itself increasingly in opposition to these developments. As Korbel points out, Brandt, like de Gaulle before him, has had to contend not only with suspicious and sometimes intransigent negotiators in the East but with implicit if not explicit opposition from his American ally. Indeed, the United States may now come to catalyze European cooperation negatively the way the Soviet Union once did.

While Professor Korbel has included an admirable chapter on the economic issues of détente, he ultimately dismisses what is perhaps the most important dimension of his subject, concluding his section on economics with "The market place is clearly one thing; politics is another" (p. 139). In other words, détente has been primarily a political process having little to do with economics. So much for the problems of economic development in Eastern Europe, for the "energy crisis," and for the growing fascination in the Soviet Union with computer technology. This is not to say, of course, that détente has been exclusively motivated by economic factors. But, if Marx has taught us anything, it is that political and economic motivations are inextricably bound up with each other. Moreover, John Maynard Keynes pointed out a long time ago in his *Economic Consequences of the Peace* that eastern and western Europe make up a natural economic and political entity and that efforts to divide it are bound to lead to trouble. The Cold War, it might be argued, has been the great aberration of European history not, as it often has been described, the beginning of a "new" system. Détente is, therefore, not a risky alteration of this system but the inevitable reassertion of more natural and historical ties between the two halves of Europe.

In discussing the evolution of West Germany's *Ostpolitik,* Korbel, quite rightly, stresses the domestic origins and significance of these policies. The "adjustment to reality" on the part of West German public opinion has been painful and difficult for many whose stake in the eastern territories was personal as well as political. Korbel, however, also stresses the European and particularly French antecedents to *Ostpolitik.* While there no doubt are tensions between German and French approaches to the East, a fundamental agreement over the importance of rapprochement reinforces the eastern policies of both countries. On the other hand, particularly in France, the concern is that German initiatives should be part of a collective West European approach rather than a unilateral assertion of German interests. Efforts by Germany to pursue an independent course in Eastern Europe, it is feared, could revive the risks of the Cold War and endanger Western European unity. Thus, Germany's unilateral diplomacy during the Czech crisis of 1968 only served, as the French complained at the time, to exacerbate an already difficult situation. From this perspective, the Russian intervention in Czechoslovakia is proof not merely of the overbearing nature of Soviet rule in Eastern Europe but also of the instability of the Cold War confrontation.

It is important, therefore, to stress, as Korbel does not, the connections between Germany's eastern policies and those toward the West, principally toward the European Communities and the Atlantic Alliance. The enthusiasm for *Ostpolitik* that Brandt and the other leaders of the ruling coalition in Germany have displayed since 1969 has been coupled with a reliance on the Atlantic Alliance and American support. This "special relationship" with the United States has increasingly conflicted with the aim of building an independent European economic and security system. Ironically, the German Socialists have been among the staunchest defenders of an American military and economic hegemony in Western Europe. For all the rhetoric, Germany's commitment to an independent Europe remains ambiguous. This, rather than Soviet motives, as Korbel suggests, could become the real divisive force among the states of Western Europe. Ultimately, Germany must decide between its Atlanticist and European orientation and must reconcile its eastern and western policies. Germany remains the key, not only to the future of détente, but also to integration in Europe. The unresolved issues of détente are, therefore, closely tied to future developments in the West.

Professor Korbel, it seems to me, defines his subject too narrowly. No doubt the Russians will remain as intractable and dangerous as ever. Conservative imperial powers usually behave that way. However, present efforts at rapprochement in Europe need to be seen in a more historic and economic context.

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Europe Since World War II: The Big Change. By Norman Luxenburg. (Carbondale: Southern Illinois University Press, 1973. Pp. xi, 260. $6.95.)

In the preface to his book Professor Luxenburg states, "It is the intention here to review in a readily comprehensive manner European developments in the period since 1939 and to depict graphically the tremendous political, sociological, economic, industrial, and psychological changes which have been taking place in Europe since World War II" (p. xi). This is a hardy task, perhaps too hardy for a book of only 260 pages.

Following a discussion of the interwar years in the first chapter, the next seven chapters comprise a rather conventional history of WW II, a discussion of the origins and conduct of the Cold War, a description of postwar European recovery and the repercussions resulting from Stalin's death. This material is presented in a generally lucid manner and serves to dramatize the devastating impact of the war and the tensions which gripped Europe in its aftermath.

Toward the end of the book the author makes his most significant contribution to our understanding of contemporary Europe. Through cogent narrative and clear tables, substantial data are presented depicting the phenomenal progress the European nations have made in the areas of industrial output, energy production, agriculture, communications, housing, tourism, education, and health.

The concluding chapter gives the author an opportunity to speculate on Europe's future. Luxenburg notes that the continuing question for the 'seventies is whether free societies can continue to make socioeconomic progress in a manner that will offset the admitted progress made by the Communist and authoritarian governments of Europe. The author is disturbed particularly by elements within West European countries, specifically persons in intellectual circles who seem to place a greater emphasis on economic advances than on fundamental human freedoms such as speech and free elections. The implication seems to be that if the representative political systems are to survive, they will, among other things, have to continue making significant economic progress.

While this book has obvious strengths, it also has limitations. The author's view of Europe as engaged in continuous socioeconomic competition between varying economic and political philosophies is perhaps too narrow. Certainly there are still considerable Cold War tensions between East and West. On the other hand, little if any attention is paid to those forces encouraging European integration. For example, there is no consideration of Willy Brandt's *Ostpolitik*. It is difficult to understand why this most important subject has been largely ignored in a volume professing to depict political changes in Europe since WW II. Similarly, there is no assessment of rising United States-Soviet détente and its impact on European relations.

Even synthesizing histories such as this one should not be exempt from the basic scholarly requirement of documentation. This book is inadequately documented, and the reading list at the conclusion of the book while admittedly selective, is very limited. For use as a summary review of postwar changes in Europe, the book should contain more thorough recommendations for further reading either in the bibliography or in footnotes.

This book might be used as a background text at the outset of a course on European politics and government to provide students with a broad overview of postwar developments, or it might have some relevance to a course on regionalism. The emphasis on systemic socioeconomic factors is important for students of politics to grasp and in that sense the book makes a positive contribution. But overall, the utility of the book for political scientists is limited.

DOUGLAS W. SIMON

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The Community of Oil Exporting Countries: A Study in Governmental Cooperation. By Zuhayr Mikdashi. (Ithaca, New York: Cornell University Press, 1972. Pp. 239. $8.00.)

It is said that the oil business involves more politics than economics. According to *The Economist* (London, July 7, 1973), a barrel of Persian Gulf crude oil selling for $2.50 costs about 10 cents to produce. Two-thirds of the $2.40 difference goes to the governments of oil-producing states via taxes and royalties. The Organization of Petroleum Exporting Countries (OPEC) is the international organization whose function is to protect and enlarge that governmental component of the price of oil.

Zuhayr Mikdashi is a professor at the American University of Beirut who has acted as a consultant both to OPEC and to the United Nations. While the author's major discipline is economics, this useful book will serve as an important tool to political scientists interested either in the transnational politics of oil or in functionally specific international organizations.

The eleven countries of OPEC account for 50 per cent of world oil production, 70 per cent of world oil reserves, and 85 per cent of the oil that moves in international trade. OPEC was founded in 1960 as a response to weakening oil prices that resulted from the import quotas that isolated the American market and from the increased production by new independent firms (i.e., firms not members of the eight major companies that accounted for 80 per cent of crude oil trade in 1970). OPEC was not notably successful in the early 1960s. At the end of the decade, however, with the closing of the Suez Canal and a tightening of oil supply, the conditions for success improved. By the time of the Teheran negotiations of 1971, the OPEC countries were able to hold to a hard line which led to a highly favorable settlement.

A critical and difficult question which Mikdashi addresses (though not wholly successfully) is what difference the existence of OPEC as an institution makes. Can market forces alone explain the changing pattern of distribution of gains? Or market forces plus the industry's oligopoly structure? Mikdashi argues that "it is unlikely that some of these gains could have been realized had member countries not coordinated their policies and actions within OPEC" (p. 201). He points out that since 1963, when OPEC tax rates started paying off, "government revenues per barrel started to rise in current dollar value, while company earnings per barrel persisted in their downward trend until 1970" (p. 141).

On the other hand, Mikdashi is a careful analyst and points to the various failures of OPEC solidarity in the 1960s and to the low level of institutionalization of the organization: decisions are by unanimity; new institutional tasks and procedures for settlement of disputes have been rejected; and the secretaries-general have been restricted to one-year terms. Indeed, national distrust prevented filling of the deputy-secretary-general's position.

The major difference that OPEC made seems to have been in the provision of information that led oil-producing countries towards similar definitions of their self-interest so that they were able to reap the maximum gains that market and oligopoly forces permitted. Mikdashi points out that OPEC's research capacity provided information to counter the secrecy within which oil firms tend to operate. Equally important, OPEC analysts, taking a system-wide perspective, were able to show certain countries that actions (such as the encouragement of the independents) which were rational for the given country in the short term could lead to lower gains over the long term.

In short, the major role of OPEC seems to have been in speeding up the learning process. As with any cartel, its success is far easier when swimming with the tide of a seller's market. This makes it unlikely that OPEC will prove to be a good model for other commodities for which the market tides are less favorable. And it remains to be seen how well OPEC's lessons have been learned should the tide turn again in oil.

J. S. Nye

Harvard University

Bismarck and the French Nation, 1848–1890. By Allan Mitchell. (New York: Bobbs-Merrill, 1971. Pp. 152. $10.00.)

The author states that he had two objectives in writing this book: "to survey the Bismarck scholarship of the past century and to augment it in one specific area where it seems to me insufficient. A recently published bibliography of Bismarckiana lists more than 6000 titles; yet there has been to date no single statement exclusively concerning the special relationship of Bismarck's Germany to France during the entirety of his public career" (p. 4). Whether the absence of a "single statement" justifies adding to the Bismarck bibliography, which seems already to have reached some sort of critical mass, is dubious, for we do not lack works from which the information provided by this extended essay can be readily obtained. Those for whom the Junker statesman remains a fascination will always find reason for doing what they want to do (I admit to sharing in this vice) and will probably find a publisher. Although hardly in the same league with "Hitler," a title containing "Bismarck" (if only "Germany After Bismarck") seems to command a market of respectable size—perhaps because of the number of Bismarck researchers who feel the compulsion to buy it.

Mitchell's approach is biographical and political, although he is somewhat defensive about writing this kind of diplomatic history. "Nothing is to be gained, then, by accepting a false polarity of old-guard and new-wave history. Historians need to continue traditional forms for research and writing, while seeking to improve them; and they need to break new ground both conceptually and methodologically" (p. 3). Certainly there is something irritating about the new trend in German historiography represented by such writers as Helmut Böhme and Hans Ulrich Wehler, who, having finally discovered Karl Marx, Karl Mannheim, and Georg Lukacs, insist that they have blown the final trumpet that will tumble the walls of traditional historiography. Many angles of vision are necessary to gain a full perspective of the past, and it is foolish to assert that the old ones have nothing more to contribute. Hence Mitchell's defense of his approach is refreshin. Within the limits he has set for himself, moreover, he has done his work well; the writing is succinct, forceful, and accurate.

There is reason to doubt, nevertheless, that a work so narrowly conceived as this can reveal the complex character of the diplomatic game, particularly as played by someone as astute as Bismarck. For example, his desire to cultivate close relations between Prussia and France from the Crimean War to 1866 was but one part of a complicated system of pressures by which he sought to coerce Austria into granting a greater role to Berlin in German affairs. The visit to Biarritz in 1862, the Alvensleben Convention, the Danish war, and the Gastein Convention were successive moves on a diplomatic chessboard that included the whole of Europe. By concentrating on Prussia's relationship to France, Mitchell leaves some of the squares and pieces out of sight.

Otto Pflanze

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The Weary and the Wary: U.S. and Japanese Security Policies in Transition. By Robert E. Osgood. (Baltimore: The Johns Hopkins University Press, 1972. Pp. 95. $2.00.)

This essay, divided into a series of ten topics, analyzes Japanese-American security policy as it appeared to be just prior to the Nixon visit to Peking in February of 1972. The topics include trends in American policy in Asia, the meaning of the Nixon Doctrine, the inhibition of Japan's military role, Japan's conception of external interests, the perceived threats to these interests, Japan's views of America's security role, the attitudes of other states toward Japanese power, Japan's defense policy, international power structure, joint security relationships, devolution of power, and the limits of American power and interests. Many of these cover some of the same major points from different points of view. The treatment is especially good in indicating the thinking of Japanese leadership in government about defense at the time it was written. The author feels the United States is weary and ready to let its role of checking communism devolve upon Japan, while Japan is wary and wishes to restrict itself to its own home defense.

Professor Osgood expected the substantial withdrawal of American forces from South Korea, South Vietnam, the Philippines, Taiwan, and Japan which has now been carried out to reduce United States influence in Asia and weaken its security pledges in Japan: Together with the hostility expressed toward Japan and its nuclear buildup, China is thought to be likely to stimulate Japan into taking on military responsibility for South Korea and Taiwan and eventually perhaps some in Southeast Asia where its economic interests are involved. Even Prime Minister Chou En-lai is reported to have professed fear that such a scenario might eventuate. Almost the opposite has occurred, however. President Nixon maintains American influence in Asia with continued naval and air power and even continues the battle to keep non-Communist regimes alive in South Vietnam, Cambodia, and Laos. Threatened by a Soviet pre-emptive attack, China has done the opposite of what the author expected by adopting a friendly policy toward the United States and Japan. It has come to depend upon American presence as a check, not only on Soviet power and influence in Asia, but on expansion of Japanese power as well. Rather than being threatened more than before, Japan has even less incentive to arm further or take on additional military responsibilities.

The present situation has moved beyond that anticipated by the author at the time of writing. Further tensions might result in Chinese cooperation with the United States in favor of Japan or even the Soviet Union and Japan cooperating against the other two—situations in which communism is less relevant. In establishing diplomatic relations with China, Japan has all but repudiated any responsibility for Taiwan, and the United States shows that it is willing to let the Chinese regimes settle their own quarrels, provided there is no open challenge to the existing U.S.-Taiwan defense treaty.

The book contains a few minor errors. On page 51 the yen figure for the Fourth Defense Plan is given as 5.8 billion instead of 5,800,000 million. When the Tanaka Cabinet finally approved the Plan in late 1972, the overall target was set at 5,300,000 million yen or about $20,076 million at current exchange rates. On page 56 the temporarily frozen aircraft funds should read $294 million, not $294 billion. The occasional reference to a Japanese consensus on foreign and defense policy suggests that the present government has a broader public or partisan support than it in fact has, but elsewhere the sharp opposition expressed by the left-wing parties and even some within the governing Liberal Democrats is clearly indicated. Any overseas military commitments now appear even less likely than they did before. Professor Osgood's analysis was thorough and applicable at the time of writing; but new conditions now call for a less pessimistic assessment and do not support an expanded military role for Japan.

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America's Outward Thrust: Approaches To Foreign Affairs, 1865–1890. By Milton Plesur. (DeKalb, Ill.: Northern Illinois University Press, 1971. Pp. 276. $12.50.)

Professor Plesur is dissatisfied with the portrayal some traditionalist historians have presented of a purely isolationist *post-bellum* United States. He contends that foreign affairs in general and expansion in particular were the concerns of many Americans during the period known as the Gilded Age, and he has produced an extensively documented survey of the initiatives for and the debate on extending American influence overseas as well as within the borders of our neighbors both north and south.

The debate over expansion and foreign involvement is the key to Plesur's study. In order to express the public's sentiment on foreign affairs, he adopts the social historian's "attitudinal approach," which is essentially an examination of correspondence, statements, editorials, journal, periodical and trade publication commentaries of the day. Professor Plesur divides the debate into two parts. The first seven chapters deal with a variety of topics: commerce, the State Department, the Navy, missionaries, and the overseas literate and travelers. The second part concentrates upon American attitudes toward Europe, sub-Saharan Africa, Latin America, Canada, the Pacific and

Asia. The style is cyclical, each topic being covered from approximately 1865 to 1890.

In his discussion, Plesur provides an enlightening account of the slow yet steady revival of American foreign involvement, the personalities associated with it, and the opposition encountered. The three main trends he uncovers are: (1) the growing interest in overseas commerce; (2) the cautious acceptance of commercial expansion, particularly on the part of the diplomats, coupled with a pronounced reluctance to engage in international political issues; and (3) an erratic tendency to push the United States into a great-power role. Plesur stresses the extension of trade as the strongest impetus in the development of foreign policy. He places the other two trends, which entail protection of American nationals, engaging in international conferences, enforcing the Monroe Doctrine and facing up to the great power rivalries, in a secondary level of importance.

The trends are evidence that some people were moving toward increased foreign involvement, but they are not strong enough to totally discredit, as Plesur is wont to do, the traditional historians. The Secretaries of State, the functionaries, missionaries, and businessmen were not particularly successful in their endeavors to move the nation outward. The debate proves this. We had a piecemeal rather than a coherent foreign policy which was the product of special individual concerns, self-serving economic objectives and an ad hoc approach to external events. Systematic planning was not a principal feature of policy formulation. It seems that Plesur occasionally loses sight of this important fact in his eagerness to revise history. There was no "thrust" during the Gilded Age, but there was a burgeoning of interest in foreign affairs. As Plesur more aptly states in the epilogue, the period represented a seedbed for the more pronounced imperialistic ventures which were to follow.

Professor Plesur's exuberance is not as serious a problem as his style and method, which I believe contribute to a loss of continuity and a certain amount of superficiality and unevenness. Presidents and Secretaries of State appear every place; no administration is analyzed in a contiguous fashion. James G. Blaine receives a great deal of attention, primarily because of the stress placed upon the late 1870s and 1880s, while Hamilton Fish, whom Plesur acknowledges as a contributor to foreign involvement, is rarely mentioned. Piecing together the policies and activities of the administrations is difficult. It could have been accomplished in Chapter 3, "Stirrings in the State Department," but Plesur was more interested in detailing the problems of the foreign service. Depth could have been provided had he been less encyclopedic and more analytical. The study needs more emphasis upon domestic politics, which in this transitional period of American history had a profound effect upon foreign policy.

This book will not appeal to those who analyze the foreign policy makers as much as it will to those who study the forces which affect the direction of foreign policy. Despite the weaknesses, Plesur's social historical survey is informative, and his effort should stimulate further research. Those who seek imperialistic motives for American foreign policy will find it a worthwhile contribution to the literature.

DONALD M. BOROCK

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The Warsaw Pact: Case Studies in Communist Conflict Resolution. By Robin Alison Remington. (Cambridge, Mass.: The M.I.T. Press, 1971. Pp. xix, 268. $10.00.)

Since the appearance in 1960 of Zbigniew Brzezinski's book on the Soviet bloc, many studies on the various disputes among the Communist states as well as the general evolving nature of the Communist alliance have appeared. In fact, as political scientists from the Communist countries at times acknowledge, more has been published on this subject in the West than in the East. Robin Remington's volume represents a valuable addition to the existing literature. It is the first study which focuses on the role played by the Warsaw Pact. As the author admits, "it is not a definitive study" (p. xiii), but it definitely fills an important gap in our understanding of the functioning of the Communist alliance.

The book is written in a crisp, at times almost telegraphic style. It conveys the message without belaboring it—generally through understatement—but it is quite effective. All the arguments are supported by rich and carefully selected evidence. Like some other volumes in the MIT series, this one devotes excessive attention to press and Radio Free Europe reports which, useful as they are, suffer from short-range perspective, while it ignores many relevant scholarly studies. The volume contains a selection of the key documents essential to the understanding of the transformation of the Warsaw Pact, especially in the late 1960s.

Dr. Remington's study starts with the description of the origin of the Pact in 1955 and its first internal trial in the following year. The treatment of the revolution in Hungary and the upheaval in Poland in 1956 is the least satisfactory part of the book. No serious attempt is made to compare the circumstances which led to the Soviet intervention in Hungary in 1956 with those in Czechoslovakia in 1968. Little attention is paid to the Polish efforts to secure more elbow-room in its foreign policy in the late 1950s—even before the Sino-Soviet split took place. This is particularly regrettable because there is evidence that Warsaw's delicate maneuvering vis-à-vis Moscow at that time influenced the policies of other Communist states in the 1960s, notably that of Rumania.

The next part treats the Soviet disputes with Albania, Rumania, and Czechoslovakia. The subject has been already extensively covered in the various case studies dealing with each one of these incidents—one of them by Dr. Remington herself. The author, however, emphasizes those aspects of the quarrels which relate directly to the Warsaw Pact. In cogent and penetrating fashion she traces the developments which led to the successful defiance of Moscow by both Tirana and Bucharest and the failure to do so by Prague. Comparison of these events in the conclusion section is most illuminating.

The most novel part of the book is its review of the dispute between the U.S.S.R. and the G.D.R. in the late 1960s and the early 1970s. The author successfully demonstrates that differences between Moscow and Pankow "were no mirage" (p. 134). Ulbricht was not only apprehensive about Soviet rapprochement with the G.F.R., but did his best to sabotage it. The chapter is interesting both as an illustration that the smaller Communist states may "differ from Moscow on the side of dogmatism rather than reform" (p. 176), and also as an indication of the trend dominant in the entire bloc. In 1971 Ulbricht was sacked and was replaced by Honecker who, although no liberal, accommodated to the growing spirit of détente in Europe.

The unifying theme of the volume is that the Warsaw Pact has been perceived in two opposing ways. The first, advocated by Molotov in 1955 and revived in the "Brezhnev doctrine" in 1968, views the Warsaw Treaty Organization as an instrument to integrate Eastern Europe more closely with the Soviet Union —basically as an institutional framework to deal with recalcitrant satellites. The second perceives the Warsaw Pact more in terms of a traditional alliance—as a genuine channel of conflict containment and resolution among its different participants. Dr. Remington aptly observes that these two conceptions are often linked to internal struggles for power in the Kremlin. The developments in international politics—Soviet conflict with China and the growing détente in Europe—however, have played into the hands of the moderates. So far as the Warsaw Treaty Organization is concerned, "there seems to be a growing understanding in Moscow . . . that compromise is preferable to open conflict" (p. 176).

The analysis of the nature of the Warsaw Pact is remarkably well balanced. Dr. Remington maintains that the role of Communist ideology as unifying force in the Warsaw Treaty Organization is declining. She cautions against ignoring ideology altogether; a notion of "a community of interests" among the Communist states is still there and, especially in the case of the weaker Communist regimes, the presence of Soviet troops continues to be an important source of support. At the same time the author believes that "popular nationalism has infected the [Eastern European] regimes" (p. 175). They are likely to pay more attention to their own national interests, and "the Warsaw Pact can be expected to reflect a more genuine balance of interests in the future" (p. 189). The events since the publication of the book—Albania's refusal to take part in the Conference on European Security and Cooperation, Rumania's independent stance in Helsinki, and Poland's eagerness to exploit the climate of a détente to expand its relations with the West—all confirm this conclusion.

Dr. Remington is quite right, then, when she points out that it is "increasingly necessary to reevaluate the stereotyped image" of the Communist alliance system (p. 189). She feels that "contrary to postwar prediction, there is no Soviet Empire in Eastern Europe, the invasion of Czechoslovakia notwithstanding. East European Communist states have retained their national identities, have resisted extensive economic integration, and have kept national control of their armed forces" (p. 6) Moreover, in the case of those countries which feel threatened by the revival of German power, notably Poland, the Warsaw Treaty Organization serves as a genuine instrument to protect their national interests.

Robin Remington's volume was written even before East-West negotiations about the future of Europe acquired their full momentum. At present, with the Conference on European Security and Cooperation (CESC) and the Mutual Forces Reductions (MFR) negotiations under way, her wise advice is more apt than ever. In the era of negotiations the Cold-War clichés of Eastern Europe should be discarded. New concepts, depicting accurately the current problems in that part of the world and paving the way for their imaginative solutions, must be found. We need more perceptive studies like *The Warsaw Pact.*

ADAM BROMKE

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Nepal: Strategy for Survival. By Leo Rose. (Berkeley: University of California Press, 1971. Pp. 310. $10.00.)

Leo Rose's main concern in this, his most recent volume on Nepal, is with the content of Nepal's foreign policy from 1770 to the present. His book, in sum, deals with Nepal's physical setting and the country's complex ethnic structure. Throughout, an effort is made to assess the special psychological aspects of Nepal's world view.

Rose finds a basic similarity between the analysis of Nepal's role in the Himalayan area of the 18th century King Prithvi Narayan Shah and his 20th century successor King Mahendra Bir Bikram Shah Dev. The limited number of alternative policies available to a country in Nepal's position is carefully noted and Rose takes as his theme the consistency of choice by the country's decision makers from the mid-18th century to the present.

The book has two outstanding characteristics. One is the splendid documentation given Rose's treatment of what he calls "confrontation politics" in the Himalayan area from 1770–1845. A second is the penetrating portrait we are offered of the late King Mahendra. Rose does not seem to have met Mahendra face-to-face. But in his description of Mahendra's calculated experiment with "diversification" of Nepal's political, economic, and cultural relations with countries other than India we discern as well the King's character and the implicit relationship between his external and domestic tactics.

For most, Nepal is not a very important country. But Nepal seems to have solved its "national identity problem" and its experience in doing so may provide others with useful lessons. This is a good book, a pioneer in the study of the foreign policies of "mini-states."

MERRILL R. GOODALL

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Soviet and American Policies in the United Nations. By Alvin Z. Rubinstein and George Ginsburg. (New York: New York University Press, 1971. Pp. 211. $8.95.)

It is commonplace to hear international relations specialists talking of increasing interdependence, of the global scope of economic, political, and ecological problems, and of the consequent difficulties inherent in uncoordinated, unilateral solutions. It has simultaneously become almost routine to note the decline in both the effectiveness and the centrality of the international institutions whose task it is to regulate precisely these kinds of situations. The United Nations is no exception, as recent criticisms of its effectiveness in peacekeeping and economic assistance programs indicate. While part of its apparent failure is attributable to a set of faulty goals too optimistically formulated and too little examined for feasibility, many of the explanations rest with the behavior of the superpowers—the US and USSR—both in the organization and in the foreign policy role they assign to the UN. In attempting to make sense of the shifting international pattern, then, one might initially examine the role of the superpowers in the UN, their attitudes toward it, the expectations they have entertained for it and the presumptions and biases which underlay their organizational commitments. This seems to be the aim of the collection of essays in *Soviet and American Policies in the United Nations.*

As a theoretical analysis, the book is disappointing. It is by and large a collection of primarily descriptive essays that recount positions and provide historical summaries of policies on several key issues, including disarmament, decolonialization, law, and economic development. Little attempt is made to relate these topics to developments in international relations theory or to the various approaches to international organization. Neither is there a systematic discussion of the theoretical dimensions of foreign policy and its relation to the United Nations. Yet if the book's theoretical contributions are weak, its descriptive and pedagogical value is more mixed. To assess these areas one must examine the essays themselves.

The first article, Robert Wesson's "The United Nations in the World Outlook of the Soviet Union and the United States," seems the most difficult to evaluate positively. It presents an oversimplified view of American and Soviet attitudes toward the UN, which is strongly biased toward the American viewpoint. The historical account of the UN's founding treats the US position as one motivated by transnational interests in "world order," without considering the extent to which the concept itself was an extension of Anglo-American values and interests. In contrast, the Soviet position is portrayed as an instrumental one, motivated by the UN's possible utility for Soviet foreign policy aims. Drawing such a sharp contrast obscures the extent to which the UN in its conception served a utilitarian function for the US and the UK, as well as down-playing the interests the USSR expressed in the organization as a basis for continued cooperation among the allied nations, their chief hope for future world order. While there is little doubt that this conceptual orientation was once a common American view of US-USSR orientations toward the UN, one might expect the post–cold war treatments of both Soviet and American foreign policies would require a scholar in the 1970s to probe more deeply into this issue. A similar approach carries over into the discussion of the Soviet position in the United Nations. Having identified the Soviet orientation toward the organzation as a rhetorical one, the article goes on to detail the major Soviet policy stands on decolonization, disarmament and Western behavior. In the presentation these positions are treated rhetorically, and are given no serious attention as issues. Again, the experience of the past decades of aid programs and the role of Western political and economic influence in foreign countries should be a caution against treating such issues as *merely* rhetorical.

Arthur Lall's essay, "The Superpowers, the UN and Disarmament: A View from the Third World" presents a more balanced view. While recognizing and discussing the fundamental ideological differences between the US and the USSR, he also sees the similarities they share as a result of their "superpower" status. Within this framework, he looks at the norm system of the UN and in so doing draws attention to the important dichotomy in the normative position of the superpowers and the smaller nations. While the organization, and the super powers within it, advocate universal normative rules, great powers have the implicit, structurally sanctioned right to reject those norms which are not applicable to their aims (p. 51). From this vantage point, Lall examines the preference for bilateral discussion, and the effect of this preference on the UN's ability to act as a forum for key issues such as disarmament. The implication seems to point to détente as providing the possibility for a Congress of Vienna mode of problem solving, e.g., "managed problems" within a limited and contingent consensus.

Daniel S. Cheever's discussion of "Economic Development: The Great Powers and the Sea Bed" reinforces and details similar themes to those Lall develops: a preference for bilateral agreements as more efficient, an unwillingness to consider political solutions proposed by those not technologically competitive in the area at issue (e.g., nuclear technology or deep sea exploration) and a general impatience with the wide range of political questions which emerge in the larger discussions. Cheever then attempts to demonstrate the existence of the same themes in many of the economic development programs. The article is well researched, and is a good addition to discussions of the issue of the sea bed, but is perhaps not sufficiently broad to be the only discussion of economic development or redistributive issues included in the book.

Harold Jacobson's article on "Decolonialization" carries through the mix between similarity and difference among the superpowers. He presents a well differentiated view of US and USSR policies in an area in which neither was intimately and directly involved. He details, for example, the Soviet Union's attempt to push positions delegating the organization stronger authority to handle decolonialization issues (p. 77), in part in the service of its policy of becoming the vanguard of the anticolonial position. This contrasts with the US position of gradualism, and the attempt not to only create independent states, but maintain Western influence after independence. The asymmetry carries over into policy making. The USSR had less opportunity to translate its preferences into policy for it had significantly less direct involvement in UN policy making about colonialism, a fact which is cited as decreasing Soviet interest in the UN's anti-colonial action. The US, on the other hand, developed more direct conflicts of interest—especially vis-à-vis the Southern African situation and its classification as "colonial," and Portugal's refusal to graciously give up its African colonial holdings. As the conflict became more clear, the US became less willing to push UN anticolonial action. With this background laid, the descriptive account of policy and position shifts is intelligible and interesting.

The remaining articles are less solid. James Sewell's "Keeping the Peace: An Interpretation of Soviet and American Security Policies" adds little to our knowledge of peace keeping; after the initial historical treatment, the article rapidly loses strength. It presents various force plans without making the significance of their drawn-out chronicling clear. In contrast, the several illustrative cases are so sketchily done that each can only point to an analysis of peace keeping. He does, however, raise the important and interesting question of how to compare the benefits and influence of the US and USSR in peace-keeping efforts. Edward McWhinney's "The Rule of Law and the Peaceful Settlement of Disputes," while giving interesting descriptions of the US and USSR position toward international law and organizations, is too hastily done to support the generalizations he derives.

The postscript is especially disappointing. It does not attempt to integrate the articles into a theoretical framework, nor does it provide insights beyond the contents of the articles themselves. This is an especially significant failure; since several issues clearly emerge which could be the basis for interesting comparison with neofunctionalist organization theories. In particular, the Lall and Cheever articles provide the raw material for raising the question of how much the perception of "technical" issues legitimates a bilateral superpower solution to problems and becomes a basis for increasing the function of the UN as a forum while decreasing its effectiveness as a problem-solving organization. An analysis which raised the issues of science and technology in the UN as the means for superpowers to depoliticize issues which are central to superpower-lesser power relations could have been insightful and crucial to understanding some of the impasses in current UN action. No such issues are raised, however, and the effect is to leave the reader with the impression of a lack of cohesion and overall perspective. The result is that the book, like many edited collections, contains some articles which are either interesting to specialists or suitable as teaching material, but the book as a whole fails to make a significant contribution to our understanding of the UN, its evolution and its prospects.

CHERYL CHRISTENSEN
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Comecon and the Politics of Integration. By Henry W. Schaefer. (New York: Praeger Publishers, 1972. Pp. xiv, 201. $15.00.)

Western scholarship has not exactly been blessed with high-quality studies of economic integration in Eastern Europe. In contrast to Western Europe, Africa, and Latin America, the best work on the subject has been done by the economists, with the political scientists remaining largely silent. Why this should be so is not too difficult to discover.

Despite concentrated attacks on the persistent traditionalism and parochialism of Communist studies, especially in political science, the field continues to be dominated by scholars who have neither the interest in, nor the grasp of, modern social science methodology. The younger, more theoretically inclined generation, appears to focus its attention on various aspects of domestic rather than international politics, with the result that the international scene is either largely neglected or discussed only in the context of bipolarity versus multipolarity, East-West rapprochement, or de-Stalinization versus desatellization. In either case the research tends to be descriptive rather than analytical, ignoring for most part the existence of a substantial body of theoretical and methodological literature. In the same vein the study of integrative processes in Eastern Europe has concentrated so far on enumerating the various obstacles to closer economic unity or on contrasting the lack of progress in the Council for Mutual Economic Assistance (Comecon) with the dynamism of the European Economic Community as if the two organizations were strictly comparable.

In the past it could have been argued that the lack of data prevented a meaningful analysis of economic integration in Communist Europe. This excuse, however, is no longer valid. Not only do the various East European countries publish ample economic data in their statistical yearbooks but, as shown by the study under review, content analysis of the East European press and radio broadcasts yields a surprising amount of factual information indispensable to anyone conducting in-depth research on regional integration in the area.

The author, a long-time observer of the East European scene, attempts to produce a detailed account of the developments within Comecon in the period 1968–1971. As he points out with refreshing modesty in the preface, his objectives were quite limited: He had no intention of writing a scholarly treatise heavy on conceptualization and analysis, but wanted simply to give a chronological sketch of the negotiations among Comecon's members aimed at improving the functioning of that organization. Within this restricted context, Mr. Schaefer succeeded admirably in producing a comprehensive record of the discussions which led to the proclamation of the Comecon's "complex integration program" in 1971.

Perhaps the most interesting conclusion that emerges from the study is the basic lack of consensus on fundamentals exhibited by Comecon's members. To be sure, even those whose scholarly interests lie outside the Communist camp, have been aware for some time of the maverick Romania which has refused to toe the line and to submit to the dictates of Comecon. It has been taken for granted, however, that the rest of the member-countries presented a more or less united front with respect to the various policies intended to reform Comecon. Mr. Schaefer demonstrates persuasively that this was hardly the case in the late 'sixties and early 'seventies. Not only was there a lack of agreement on certain basic reforms but, in addition, the process of negotiations was characterized by shifting alliances with different countries changing their positions and switching their allegiances in accordance with their national interests. A far cry indeed from the traditional image of a Communist monolith which still tends to haunt us in the literature.

In line with the restricted scope of his study, the author does not attempt to analyze the causal factors responsible for the clearcut loosening of intra-Comecon relations. By providing an impressive body of factual information, he manages to produce enough raw material to enable other scholars to investigate the problem in greater depth and to postulate and test some interesting hypotheses regarding the future of regional integration in Eastern Europe. It is here that Mr. Schaefer makes a real contribution to knowledge and we should all be grateful to him for undertaking this indispensable task.

ANDRZEJ KORBONSKI
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Austrian Neutrality in Postwar Europe: The Domestic Roots of a Foreign Policy. By Thomas O. Schlesinger. (Wien-Stuttgart: Wilhelm Braumüller, 1972. Pp. 158. $10.00.)

The author of this book organizes his material on Austria's foreign policy in five chapters: Occupation, Negotiation and State Treaty; Reaction to Intervention near Austrian Borders; Minority and Border Problems; Austria in International Organizations; and Neutrality versus Neutralism. He calls it a value-oriented case study. What Schlesinger presents amounts to a great number of interesting instances of Austria's foreign policy since 1955 and an equal number of interesting commentaries by the Austrian and international press. Everybody interested in Austria's policy of neutrality will find useful material in this book. I disagree, however, with the author both about what he did not collect or pay attention to and also about his final proposition.

The final proposition in the book is that "if an independent Austria is to last, its citizens must accept and understand neutrality as a vital part of their mythos" (p. 139). In 1955, when Austria announced its neutrality, especially toward the Soviet Union, Austrian politicians had an understanding of neutrality very much akin to the present rational type of neutrality "à la Suisse." Typical for their understanding was that permanent neutrality consists in more or less well defined rights and obligations under international law, deriving their substance from the eventuality of an international war. After 1955, however, and especially in recent years Austria's foreign policy occasionally points to a new understanding of neutrality of a somewhat mystical sort styled "peacepolicy." According to this understanding, the main function of permanent neutrality consists in times of peace in the conduct of a so called "active neutrality" or "peacepolicy" with no definite legal obligations. The meaning of permanent neutrality flows here no longer from the calculation of the eventuality of an international war, but from the desire to do everything to prevent any tensions which may finally lead to war. The eventual result is the development of a neutrality-ideology (cf. Daniel Frei, *Dimensionen neutraler Politik*. [Geneva, 1969], pp. 166) or a neutrality-mythos as Thomas Schlesinger would call it. Within the context of European diplomacy the Soviet Union has supported such an approach especially with regard to the European Security Conference and Austria's attitudes toward it.

Schlesinger neither takes into consideration Austria's Memorandum with regard to the European Security Conference of July 24th, 1970, nor does he give much thought to pertinent theoretical questions of international law and relations. He neither elaborates the theory of neutral conduct in international relations to which the Swiss author Daniel Frei has recently made a major contribution (see above), nor does he deal with the Soviet doctrine of neutrality and peaceful coexistence. Schlesinger therefore fails to undertake a critical evaluation of what he understands as essential pieces of Austria's foreign policy in terms of neutrality, and he does not treat questions considered essential by those in Austria to whom his book obviously addresses itself.

In this connection it should be born in mind that Austria's foreign policy already once stood under the impression of a mythos. Austria's history in the interwar period is marked by the so called *Anschluss-mythos*. Schlesinger correctly stresses Austria's progress toward a sense of identity after the Second World War. But what he in practice suggests is to substitute for the Anschluss-mythos - a neutrality-mythos. There are certainly signs for such a development in the past and especially so in more recent phases of the unfolding of an Austrian self-understanding on the international level. One may wonder to what extent such a development will in future have genuine domestic roots in the face of the Soviet doctrine and policy of peaceful coexistence. Peaceful coexistence has become the ideological or rather mythological embroidery of modern Soviet expansionism picturing the forces of light and peace in their struggle against the forces of the darkness and war. Neutrality is given a place there as a strategy to increase the strength of the forces of peace. Under this aspect it seems very lighthearted and unbalanced to suggest a neutrality mythos as the only means of survival.

Thomas O. Schlesinger, now professor at Plymouth State College of the University of New Hampshire was born in Berlin and served in the years 1954 and 1955 as an interpreter to the United States forces in Austria. His special interest in Austrian affairs dates from this experience. If he had not avoided a more theoretical approach to the questions of neutral conduct in international law and relations, his book for me at any rate would have been perhaps more than an interesting review of Austrian diplomatic and internal practice.

KONRAD GINTHER

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Le Secrétaire Général des Nations Unies: Travaux et recherches de Science politique, #16. By Marie-Claude Smouts. (Paris: Armand Colin, 1971. Pp. 299. 55.00 F.)

The author of this study points to the fact that since 1946 a new actor has made his appearance on the international scene. He is the Secretary General of the United Nations.

This book is, in the main, a chronological survey and evaluation of the role and influence the Secretary General has exerted in dealing with international conflict—the search for, and the maintenance of peace in the world.

Starting with the nomination and election of the first Secretary General, the book traces the involvement of this United Nations Official in efforts to play the part of peacemaker in numerous conflicts during the incumbencies of three Secretaries General, namely, Trygve Lie, Dag Hammarskjoeld, and U Thant.

It is pointed out that the office of the Secretary General during Dag Hammarskjoeld's tenure took on new responsibilities for peacekeeping among nations, in pursuance of which such institutions as the United Nations Emergency Force and the United Nations Observation Corps were created. This action was regarded as necessary by the Secretary General, inasmuch as the creation of a United Nations Military Force, as projected by Chapter VII of the Charter, had failed to materialize because of the "cold war." The role of the Secretary General in the Congo crisis and "preventive diplomacy" are discussed. It was the opinion of Hammarskjoeld that the United Nations should give concrete expression to an active concern with regard to the future development of Africa.

Finally, during the U Thant years, an example of containment of international conflict through the U.N. was its interposition between the two NATO allies of Greece and Turkey in the island of Cyprus. Also under U Thant's tenure the withdrawal of the United States Emergency Force in The Middle East took place, in May 1967, at the request of President Nassar of the U.A.R. It was during U Thant's time as Secretary General also that the Caribbean crisis and the war in Viet Nam took place—events which were to demonstrate clearly that the United Nations can take no significant peacekeeping action without the consent of the great powers.

As regards the role of the Secretary General in peacekeeping, the author points out that the text of the Charter furnishes few indications of what that role should be. Another difficulty is the diversity of opinion held by the member states regarding this matter: some hold that the function of the Secretary is purely an executive one, while others do not hesitate to recognize in the office the right to take a broad initiative in matters involving world peace.

The author concludes that the influence of the Secretary General in the peacekeeping process experienced a steady rise, starting with the creation of this office in 1946—in spite of a leveling off between 1950 and 1953—to a culmination with the activities of Dag Hammarskjoeld during 1956–1957. After a decline in 1960–1961 as a result of the Congo affair, the influence of the Secretariat rose again during the first years of U Thant's tenure, but experienced a new decline reaching a low point with the withdrawal, in 1967, of the United Nations Emergency Force from the Middle East; the continuance of the war in Viet Nam; the Biafra disaster; and the invasion of Czechoslovakia.

The author concludes that the Secretary General, under present conditions, can do little for the simple reason that he does not represent a super power that could intervene in the midst of a conflict for the purpose of dispensing peace and equity. He is the highest functionary of an organization who is dependent upon a minimum consensus of the members to insure his own survival.

The general assumption on the part of sovereign nations is that the Secretary General must be neutral with respect to the subjective criteria of what is in the national interest of the members of the organization. It is desirable, according to the author, that the Secretary possess certain intellectual qualities, such as a faculty for comprehension, synthesis and imagination, etc. Other qualities that are required of an individual who assumes the political and administrative functions of Secretary General are, above all, devotion to the task and much patience.

There are few new facts or insights in this book, yet it is not without value. Its value lies in the historical overview of the activities of the Secretary General during the tenure of that office by Trygve Lie, Dag Hammarskjoeld, and U Thant. The author concludes at the end that "placed as it is between the imperative of universal peace and the selfish interests of states, the position of Secretary General of the United Nations is likely to constitute, for years to come, *the most impossible job in the world*" (p. 281).

OSCAR SVARLIEN

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Toward a Politics of the Planet Earth. By Harold Sprout and Margaret Sprout. (New York: Van Nostrand Reinhold Company, 1971. Pp. x, 499. $16.75.)

Although textbooks and other introductory offerings for students of international politics are plentiful, even the best are not altogether satisfactory. Undergraduate students deserve readings which, on the one hand, depict the wealth of pertinent materials from the various disciplines relating to international politics and reflect the latest theoretical and methodological insights—but, on the other hand, also appear "relevant" and "fascinating." Even the best standard textbooks available today may well seem somewhat tedious to a good number of students before they reach the sixteenth chapter; at the same time, some of the current, quite brilliant, mini-analyses of "new approaches" (in 94 pages or so) may be too brief to be meaningful, and confusing effects may also follow the perusal of even the most imaginative "readers." Students are likely to get more enjoyment out of such lively, scholarly monographs as have been written recently by, for example, Graham Allison or Robert Jervis, but these kinds of studies naturally do not provide all-round introductions to the field.

The present book by Harold and Margaret Sprout is, in the main, yet another textbook. However, it does have considerable sparkle because of, what the authors call, the ecological perspective. This perspective, familiar from previous writings of the Sprouts, offers "a mode and style of thinking about human affairs in general," and is applied here with specific reference to international politics. "This approach is ecological in the sense that it is constructed around the four integral ideas of environment, environed populations, environmental relationships, and interrelated complexes, or communities, that compose in the aggregate an ecosystem coterminous with the earth's surface" (p. 30). The authors see their ecological perspective as a useful point of entry for the study of the milieu in which international statecraft is conducted, particularly because it directs attention "explicitly to unforeseen as well as anticipated, unintended as well as intended, consequences resulting from imperfect perception and assessment of environing conditions and events" (p. 193). The ecological emphasis, however, provides more than just a methodological point of entry. The current "creeping approach to ecological catastrophe may portend consequences as grim as a war fought with all the nuclear weapons in existence" (p. v), a catastrophe which can be avoided only through creative national and international statesmanship and through outlays far greater than current military spendings. All countries, rich or poor, Communist or capitalist, developed or developing, find themselves in the throes of an even greater dilemma of rising demands and insufficient resources. It is the major thesis of the book that this dilemma "will require major changes in values, attitudes, economic practices, and styles of living, accompanied by massive changes in the allocation of goods and services, and concerted international cooperation of a scope and on a scale only dimly imagined as yet . . ." (p. 15).

The methodology and general approach of the book can be characterized as up-to-date yet conventional. There are many excellent tables, diagrams, and charts. The authors go out of their way to analyze a rather wide spectrum of theories and approaches without undue emphasis on their own. Explanations are usually derived from a combination of environmental factors and, what are called, entity attitudes. The ecological perspective encompasses human as well as nonhuman, social as well as nonsocial, conditions. Part III (Changing Milieu: Trends and Portents) forms the heart of the book. Among its nine chapters are: Power and Policy: Sources and Limitations; Science-Based Technology: Servant or Master; Limits of the Earth: Old Images and Emerging Realities (including a fascinating account of the psychological impact of maps); Geopolitical Theorists in Ecological Perspective; Malthus in the Twentieth Century: Biological Realities and Behavioral Rigidities; Demands and Resources: The Statesmen's Dilemma. In addition, the Sprouts cover many of the topics familiar to surveyors of international politics texts, with appropriate emphasis on non-Western problems and the need to overcome national sovereignty ("persistent tribalism"). Not much in evidence are the "frontiersmen's" labors in the field. A few of them are severely criticized—for example, game theorists, who are accused of disregarding what politics is all about and of "dehumanizing the whole business" (p. 75). This text, like many others, is not always tersely written; as a result, some of its coverage seems cumbersome yet not profound.

The last chapter, "Scenarios and Possibilities," challenges the conservatism and lack of imagination of the other forecasters in the field. The Sprouts anticipate that a more integrated, less nationalistic, "politics of the planet earth" will be accomplished through cumulative increments of change. Decision makers in all the capitals are still playing two sets of games—one with foreign adversaries for such stakes as security, expansion, influence, or prestige; another set with their fellow citizens for such stakes as public order, education, housing, welfare, or rate of taxation. Insufficient resources will soon cause crises of priorities everywhere, and these crises will lead to the progressive convergence of domestic and external politics, possibly with "sensationally revolutionary" implications for the future of international politics. The Sprouts are fascinatingly "relevant" in these kinds of passages which abound throughout the book without, however, appearing as mere sermons. The book has many of the characteristics of the solid text, favorable as well as some unfavorable ones—but the ecological perspective provides purpose and sparkle with considerable benefit, one would think, to the students.

HENRY L. MASON

Tulane University

The Evolving United Nations: A Prospect for Peace? Edited by Kenneth J. Twitchett. (New York: St. Martin's Press, 1971. Pp. xvi, 239. $8.95.)

Of the several collections of essays that have been published during the past few years surveying the United Nations' record during its first quarter century of existence this is clearly one of the best. The contributions by D. W. Bowett, Rupert Emerson, J. E. S. Fawcett, Geoffrey L. Goodwin, Ernst B. Haas, David Mitrany, F. S. Northedge, and Susan Strange are of high quality, and many of them are exceptionally good.

Taken collectively the essays strike a judicious balance between stressing the importance of the United Nations in the broad evolution of political institutions and delineating the limited capacity of the organization to affect contemporary poltical issues. Fawcett, in his essay on international law, underscores the importance of the recognition of the UN as a legal person and argues that the General Assembly shows the characteristics of a primitive legislature. Mitrany, in a concluding essay designed to put the UN in historical perspective, sees the UN succeeding the Hague Conferences and the League of Nations and being the most ambitious and comprehensive effort to move toward the goal of an international system based on the voluntary acceptance of a common law and authority, yet an effort that he feels has been outpaced by contemporary events. Emerson and Strange, on the other hand, lay bare the limited extent of the UN's involvement respectively in the management of decolonization and of global economic affairs. While many writers in collections of this genre would have concentrated on what went on within the UN with respect to particular broad issues, these two authors widened their focus to developments within issue areas and then attempted to analyze how the UN's activities contributed to these developments. The result is a much more modest assessment. Emerson sees the UN's principal role as being the major center for agitation against colonialism while action has remained under the control of individuals in the colonies and the metropolitan countries. Similarly Strange concludes that the UN's essential function in the economic field has been and will continue to be symbolic: in her view it is a vehicle for expressing collective international concerns. Throughout the volume the point is stressed that the most important actions in world politics can only be taken by states, and particularly by the limited number of large and powerful states.

This delineation of the UN's capacity to affect contemporary political issues is incontrovertible, but it falls short of measuring the UN's influence in world politics. To assess this accurately one would have to know how much the UN's symbolic activities affected the policies of states and especially those of the most powerful states. Emerson feels that the answer to this question is uncertain. Strange does not address the question directly, but the answer she gives implicitly is that these symbolic activities have had minimal impact in contrast to the much stronger forces generated by the development of international production. The only essay in the volume that might have been directly oriented toward the question of the impact of the UN on national policies is that by Northedge on Britain and the UN. Its focus, however, like that of most analyses with titles that similarly link countries and international organizations, is more on the policies that Britain has pursued in the UN than on the way in which the activities of the UN have affected British policies. Northedge does note, though, that the Labour Party has generally been more sensitive to the United Nations than the Conservative Party has been.

A major concern interwoven throughout all the essays is to show how the United Nations has changed over time. Two factors are highlighted by several of the authors as being largely responsible for changing the character of both the UN's processes and its substantive output. One factor is the pattern of alignments and conflicts in world politics and the other is the expansion in the UN's membership. Goodwin's introductory essay effectively opens the discussion of the topic. He succinctly lays out the theories—largely generalized understandings of immediately past diplomatic history—on which the United Nations was based. He then shows how in the context of world politics these theories soon became inoperative. In tracing the subsequent history of the UN he portrays the shift from the organization's early days when more frequently than not it served as an instrument for the multilateralization of Western interests, to the later period when it became chiefly an instrument for the application of Afro-Asian pressures. He argues that the effectiveness of Afro-Asian pressures depended strongly upon Cold War tensions, and that as these subsided so did the effectiveness of the pressures. Like most of the authors in this collection, he exhibits a certain disdain for majorities in the contemporary Assembly and skepticism about the efficaciousness and the moral validity of the one-state/one-vote formula. Yet neither he nor the other authors question either the ethical or instrumental appropriateness of the UN's shift of attention to issues of economic development and decolonization. The broad outlines of this shift, of course, are well known. This volume's contribution is in providing several good summary statements of the nature and content of the shift.

Not only has the UN itself changed over time, so has its place in the overall architecture of international institutions. Haas's essay on regionalism is a comprehensive attempt to relate the extensive growth of regional organizations since the end of the Second World War to the United Nations. Haas maintains that regionalism has generated the dynamics that explain much of the functioning of the United Nations, and he illustrates this contention in the field of economic development. He also analyzes the interaction between activities designed to maintain peace undertaken by regional organizations and those initiated by the UN, and he argues that in this respect regional agencies have seldom handicapped the UN and most often have made positive contributions.

Good as the volume is, though, it is a better guide to understanding the UN's past than its future. Scant attention is given to scientific and technological issues which in the years since the essays were written have become more and more prominent in world affairs, and although the authors foresaw the attainment of universality in the UN's membership, they offer only limited speculations about its consequences. Still, understanding the past holds important keys for dealing effectively with present and future issues, and for this reason the book can also be read with profit by those whose interest in international institutions extends beyond the past.

HAROLD K. JACOBSON
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Self-Determination in International Law. By U. O. Umozurike. (Hamden, Connecticut: Archon Books, 1972. Pp. 324. $16.50.)

U. O. Umozurike writes with impressive British credentials including membership in Lincoln's Inn and an Oxford D. Phil. He is also a member of the faculty of Law at the University of Nigeria and intimately familiar with current political and legal thinking in Africa generally. His book, *Self-Determination in International Law,* is especially important for its translation of a major current of African thought into the language and style of Western scholarship. But Western scholarship has not itself been clear, consistent, or free of emotion in its approaches to the problems raised by national minorities or local majorities ruled by governments reflecting a broader spectrum of self-conscious political groups than the local majority may be willing to accept. It is not clear whether the strengths of an able articulation in legal terms of a special and important viewpoint on a pressing political problem, outweigh the weaknesses of an unbalanced (although sophisticated) advocacy and a failure to clarify the central issues in a way that will seem persuasive to the broader international legal community at which the book is aimed. In context, as an expression of a position persuasive to many Africans and others, and influential in arenas where that position may be important, the book is essential reading-matter. As an objective study of the concept of self-determination in international law it is not definitive.

The first five chapters, pp. 1–111, relate a version of history to show that there has been a general progress from European approaches justifying colonial expansion without significant theoretical opposition among legal theorists, through the American and French Revolutions of the late eighteenth century, the "sacred trust" concept of Article 22 of the League of Nations Covenant and the equivalent "equal rights and self-determination of peoples" principle of Articles 1(2) and 55 of the United Nations Charter; from colonies through mandates to trust territories. As a matter of legal history, large parts of the text seem to be based on misconceptions of fact. Far from arguing that red Indians were to be treated like children, and deprived of *dominium* (rights to property, as distinguished from *imperium* or rights to make laws; *imperium* is not mentioned by Dr. Umozurike), the Spanish jurist Vitoria wrote: "The upshot of all the preceding is, then, that the aborigines undoubtedly had true dominion both in public and private matters, . . . and that neither their princes nor private persons could be despoiled of their property on the ground of their not being true believers. . . . The [Holy Roman] Emperor is not the lord of the whole earth. . . . The Pope has no temporal power over the Indian aborigines or other unbelievers."[1] While it is true that "The position in customary law was that a colony was supposed to be part and parcel of its metropolitan territory" as asserted on p. 59, the assertion is hopelessly misleading in neglecting to take into account the fact that the vast number of European-dominated territories outside of Europe were not classed as "colonies" at all, but as "protectorates," "spheres of interest," "spheres of influence," "suzerainties," etc., which were *not* legally considered part and parcel of the metropolitan territories. Umozurike's contention that "the ease with which European colonies in the Far East succumbed to the Japanese contrasted with the resistance put up by the Philippines, to which independence had been promised" (p. 60), is simply untrue. Until after some experience of the Japanese occupation there was no significant Philippine resistance; the defeats of Bataan and Corregidor were suffered by American troops. Thailand, which had never been colonized at all, capitulated to Japanese demands without significant resistance and never did mount a significant guerrilla effort. But there is no point in continuing this catalogue. The opening chapters of the book indicate *feelings* about the history of European expansion, not facts.

Chapter VI is an argument, frankly one-sided, for the illegality of South African rule in Namibia. It was written before the 1971 opinion of the International Court of Justice advised the United Nations Security Council on that subject. A postscript starting at p. 295 correctly cites the decision as supporting the views expressed in Chapter VI. But there is no attempt by Dr. Umoburike to grapple with the dissenting opinions of some of the International Court's judges, or to explicate the many troubling questions that are given less than adequate handling in the majority opinion of the Court. Chapter VII, a survey of some of the practice of the United Kingdom, United States, USSR, and France relating to self-determination, is rather kinder to those powers than a more critical examination might have been, but then, the struggle for independence from them is essentially over. There is no equivalent discussion of Portugal, or of China, whose occupation of Tibet might be regarded as a colonial enterprise.

Chapter VIII argues that the "principle" of self-determination may have become a rule of law through constant reiteration and some ambiguous practice, and gives some reasons in favor of such a rule. Chapter IX, "Economic Self-Determination," argues that expropriation is a right of every state as an expression of its sovereignty and independence. In noting that an expropriation is likely to be regarded universally as legal if it is tied to a reasonable economic purpose, not unnecessarily discriminatory, and provision is made for reasonable compensation, Dr. Umozurike treads safe ground and his argument contains no surprises or new insights.

Finally, in Chapter X Dr. Umozurike examines six recent controversies to conclude that self-determination does not really extend to discontented minorities within a historically bounded state. He thus sides with the national government of Nigeria against the Biafrans, Kenya and Ethiopia against Somali, India against the Naga people, Canada against the Quebec separatists, and the United States against black nationalists who would create a separate state in continental America. He sees plebiscite as a reasonable solution for Kashmir. There is no discussion of why the grand principles of welfare and tutelage should bow before some nationalist sentiment and not before other equally strongly felt nationalist sentiment, leaving the reader to conclude that the ultimate distinction is probably based on intangibles and outsiders' views of what a "nation" should feel in view of racial, economic, and welfare factors and the contiguity of the dominant state.

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[1] Vitoria, *De Indis et de Iure Belli Relectiones* (1532), edition of 1696, John Pawley Bate, transl. (Washington, 1917) at pp. 125, 131, and 137. The misconceptions regarding Vitoria appear on p. 6 of the book under review.

ALFRED P. RUBIN
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The British Year Book of International Law 1970, Vol. 44. Edited by Sir Humphrey Waldock and R. Y. Jennings. (London: Oxford University Press, 1971. Pp. 314. $19.25.)

This volume lives up to the well-deserved reputation of the serial publication regarded by international law scholars as among the best in the field. Political scientists with no special interest in international law may not fully appreciate the significance that some of its contents have for them, for in the British tradition the Year Book largely avoids publishing articles with an overtly political approach to international law. Yet the perceptive reader will note the many implications of value to students of international politics.

In this volume, of particular value to such students is the major article by Professor D. P. O'Connell (now of Oxford University) on "International Law and Contemporary Naval Operations" (p. 19), in which he highlights the tremendous importance of a thorough understanding of current technology in assessing the direction of development of legal norms reflected in naval "rules of engagement," at least among the major Western powers. For example, after a minute and revealing analysis of the problems posed by the progress in naval weaponry and the increasing reliance on missiles, the author comes to this ominous conclusion:

> If naval doctrine develops—as it appears to be doing—in the direction of requiring the destruction of a potentially hostile vessel before it can launch its missiles the concept of a "hostile act" which was earlier referred to as the basis for an exercise of the right of self-defence will have to be revised. It is difficult to see how this can be done without further reflection on the role that international law plays in containing the use of force (p. 64).

The article, however, is by no means confined to the problem of the meaning of self-defense. It ranges far and wide over a large number of situations in which international law is relevant to the decisions and actions of naval commanders at various levels of authority. It is partly based on three case studies—the Middle East, Vietnam, and Algeria. Since at least two of these studies concern situations in which limited wars overlapped or were coextensive with civil wars, Professor O'Connell's article is a valuable and enlightening supplement to the book *The International Law of Civil War,* edited by Professor Falk, which has been reviewed in the *APSR* (March, 1974). Especially interesting is the detailed discussion of materials on the limits imposed by superior orders on the operations of the U.S. Navy off Vietnam, which indicates that international law was taken seriously. Even the exemption from capture or attack of coastal fishing vessels not engaged in military activity, applied by the U.S. Supreme Court in the well-known *Paquete Habana* case in 1900 and subsequently embodied in Hague Convention XI of 1907, was apparently honored in principle despite practical difficulties (p. 35).

Professor O'Connell, however, does not limit himself to discussing operations in time of hostilities. He also deals, with characteristic thoroughness and understanding of relevant technology, with the role of naval vessels in time of peace, particularly in policing the territorial sea and the contiguous zone, including action against foreign fishing boats apparently violating the coastal state's fishery jurisdiction. The article was written before the most recent fishery conflict between Iceland and the United Kingdom involving frequent clashes between the vessels of the two states, but it examines in detail some previous incidents of like nature, particularly the *Red Crusader* case between Denmark and Britain which was amicably settled after a fact-finding report by a special Commission of Enquiry (pp. 73–74). The contemporary significance of this role of naval units and their policy implications needs no emphasis.

Although O'Connell's article is undoubtedly the most important in this volume, there are several other substantial contributions. Dr. Michael Akehurst's "Reprisals by Third States" (p. 1) seeks to clarify one of the murkier politically sensitive areas of the law. Without attempting to examine thoroughly the practice of states, and stressing perhaps unduly the discussions in the U.N. International Law Commission of the proposed drafts of a convention on the law of treaties (which culminated in the Vienna Convention of 1969 on this subject), the author comes to the rather conservative but not necessarily wrong conclusion that as a general rule, third states have no right to engage in reprisals against a state which has violated international law to the detriment of another state, subject to two (or even three) important exceptions. The two exceptions most readily accepted by Dr. Akehurst are, first, reprisals for the purpose of enforcing international judicial decisions against defaulting states (p. 16) and, second, reprisals against violations "of rules prohibiting or regulating the use of force" (pp. 16–18). The reasons for these exceptions are found both in the practice of states and in policy considerations. The third exception is found in Article 60(2) (*a*) of the Vienna Convention on the Law of Treaties, allowing in certain situations in case of a breach by one of the parties of a multilateral treaty the suspension or termination of the treaty by the unanimous decision of all other parties. Although the author doubts that this provision is declaratory of general customary law, and the Vienna Convention is not yet in force, he regards the provision as desirable on policy grounds (p. 16). I am skeptical, however, of Dr. Akehurst's attempt to characterize the application of this emergent rule of the law of treaties as a "reprisal."

The very thorough monographic study by Dr. Manuel Rama-Montaldo of "International Legal Personality and Implied Powers of International Organizations" (p. 111) will be of interest to legal specialists rather than political scientists. Closely reasoned, it is largely theoretical and deductive in the continental manner, although opinions of the International Court of Justice and constitutions of international organizations are freely drawn upon and extensively analyzed. Although the author generally develops his ideas with admirable logic and consistency, a few of his generalizations, especially toward the end, are excessively abstract and hard to understand.

Two of the major articles are concerned with international protection of human rights ("Recent Cases on Pre-Trial Detention and Delay in Criminal Proceedings in the European Court of Human Rights" by D. J. Harris, p. 87, and "The Meaning of Discrimination in International Law" by Dr. W. A. McKean, p. 177). Both are useful contributions, and Dr. McKean's article is especially valuable as a study of the concept and problem of "discrimination" in recent British legislation in comparison with its meaning and treatment in international law enactments and decisions of international organs. The author's conclusions concerning British legislation are critical.

J. E. S. Fawcett's "The Function of Law in International Commodity Agreements" (p. 157) is a comparative study of a legal aspect of international economic relations that should be particularly enlightening to political scientists trying to understand this vastly important area of world affairs.

The seemingly anomalous legal position of India in the international community between 1919, when it became a separate party to the League of Nations Covenant and began to participate in treaty-making activities, and August 1947, when its independence was proclaimed, is not merely of historical interest. Together with evidence of much other state practice inconsistent with notion that only "independent" (or "sovereign") territorial entities are entitled to be regarded as "states" in the "true" sense and have treaty-making power, it challenges the validity of this notion and may have important future consequences in the evolution of the state system by freeing lawyers and statesmen from possible subservience to imaginary legal impediments to reform. (Cf. my Hague Academy lectures, "Territorial Entities Other Than Independent States in the Law of Treaties," Academy of International Law, *Recueil des Cours,* vol. 125 (1968–III) (p. 1, passim.). Unfortunately, Dr. T. T. Poulose's note on "India as an Anomalous International Person (1919–1947)" (p. 201), is disappointing. The author wallows in unoriginal discussions of the concepts of "state," "international personality," and the like, only to reach the rather futile conclusion that during the relevant period India was "an anomalous international person" (p. 204), but not a real "state"; consequently, he holds that after the attainment of independence in August 1947, India should have been admitted to the United Nations as a new member rather than regarded as a member state by virtue of its original ratification of the Charter in 1945. In his analysis of state practice, the author completely ignores the important changes that occurred in the two years before the official proclamation of Indian independence, including the establishment of formal diplomatic relations between India and the United States in a process which began late in 1946 (see, e.g., U.S. Department of State Bulletin, vol. 15, p. 1001 [1946]) and culminated in the accreditation of an American ambassador in April 1947 (see *ibid.*, vol. 16, p. 823 [1947] and Murarka et al. v. Backrack Bros., Inc., 215 Fed. [2nd] 547, at 551–552 [2nd Cir., 1954]). The Interim Government set up in India in September 1946 also began to conclude treaties directly with foreign states, with the British Crown relegated to a much lesser role than previously (see, e.g., the Air Services Agreement with the United States, November 14, 1946, U.N. Treaty Series, vol. 22, p. 55). Finally, the author wrongly asserts that before attaining independence India was the only "non-sovereign" state in the United Nations, except for Byelorussia and the Ukraine (p. 209). In fact, the Philippine Commonwealth was one of the original members of the United Nations, having deposited its ratification on October 11, 1945, although it did not attain independence until July 4, 1946.

The volume, like its predecessors, concludes with a large number of characteristically excellent book reviews.

OLIVER J. LISSITZYN
Columbia University

The Third World: Problems and Prospects. By Y. Zhukov, L. Delyusin, A. Iskenderov, and L. Stepanov (Moscow: Progress Publishers, 1970. Pp. 280. $2.00.)

Since the middle 'fifties, when the Soviet Union began to pay ever-increasing attention to the emerging ex-colonial countries of Asia and Africa, the national-liberation movement has become a favorite theme of Soviet writers on international affairs. As a result, a very substantial body of Soviet literature seeks to analyze the problems of developing nations and to recommend how they ought to be resolved. This volume under review is one of the few recent Soviet book-length studies that concerns itself with the Third World *as a whole* rather than with a particular country, region or continent.

Having assessed in the first chapter the great significance of the national-liberation movement as a component of the world revolution and singled out the factors that have contributed to its success, the authors discuss in Chapter 2 the various classes and strata of the developing countries and their role in the national-liberation struggle. They see the peasantry, by far the largest class in the Third World, as "a formidable revolutionary force" (p. 39), but a force which, due to its backwardness, must be "under the direction of a segment of the society which exhibits a higher degree of unity and organization" (p. 39). Preferably, such a leadership should be assigned to the urban working class, "the fighting revolutionary army's shock force" (p. 49). But because the working class of a great many developing countries is small and not yet sufficiently consolidated, the leadership could for the time being be left to the progressive segments of the intelligentsia, including even "the patriotic, revolutionary officers" (p. 53).

Chapter 3 constitutes a vicious attack on modern colonialism and "neo-colonialism," detailing various "gangster methods" (p. 80) used against the countries of Asia, Africa, and Latin America by the United States and other Western Powers. These methods are said to range from "acts of armed aggression" and "economic blockades" (p. 81) to economic and technical assistance with strings attached, as well as cultural and ideological penetration. Chapters 4 and 5 pursue much the same line by drawing a bleak picture of economic backwardness of most countries of the Third World and blaming it all on ruthless Western exploitation. Discussing in Chapter 6 the path of development that the countries of the Third World ought to choose, the authors strongly recommend a noncapitalistic road which they expect to "overgrow" eventually into a full-fledged socialist system along Marxist-Leninist lines.

Chapter 7, which explores the ideological problems of the national-liberation movement, concerns itself mainly with native non-Marxist socialism and with nationalism. While it concedes that some of the native varieties of socialism may contribute to the eventual transition to "scientific socialism," it does spurn their "inconsistencies, contradictions and eclecticism" (p. 222). It draws a distinction between reactionary nationalism of the "monopolist bourgeoisie [of the West]" and "the nationalism of oppressed peoples [of the Third World]" (p. 225), which is credited with a "progressive trend." The eighth and concluding chapter boils down to a rebuttal of Western assertions that Marxism-Leninism is unsuited for the countries of the Third World and a re-emphasis on the relevance and beneficence of the Soviet example.

The Third World is anything but the "objective scientific analysis" (p. 17) promised by the authors in their introduction. Rather, as is invariably the case with books on politics published by Soviet publishing houses, it echoes and rehashes the officially approved Soviet theses on the subject as they are known from other Soviet or Soviet-sponsored statements and studies, some of them also from the pen of the authors of the present volume. Besides the usual attacks on U.S. and Western imperialism and "neo-colonialism," it rejects also "the fallacious political and theoretical principles" (p. 9) of the Communist party of China and other such "radical leftwing opportunist" (p. 17) viewpoints. The study's main value for western students of communist affairs lies in that it brings together in one convenient, well-organized and clearly written volume the substance of published Soviet thinking on the Third World.

EDWARD TABORSKY
The University of Texas at Austin

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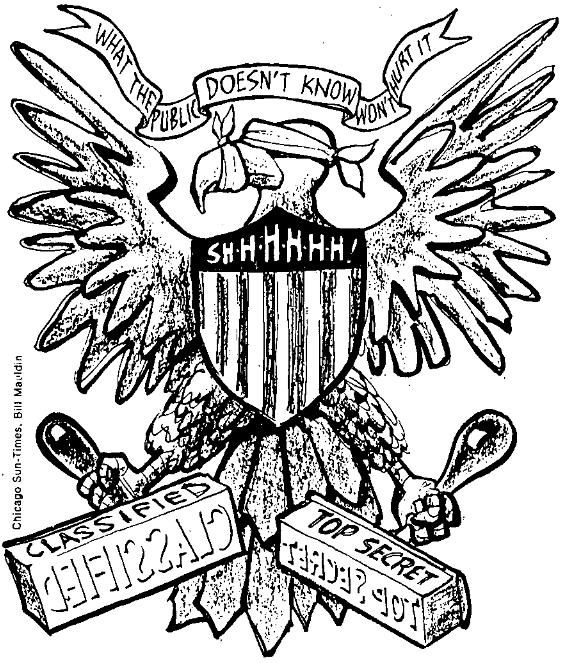
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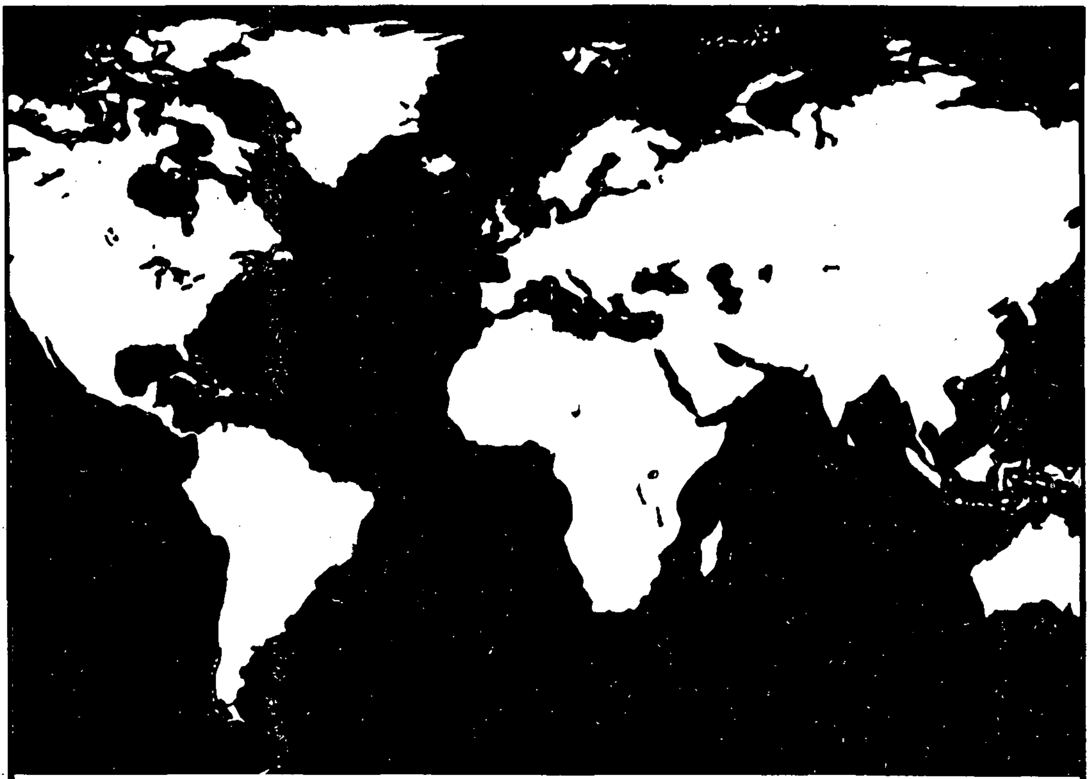

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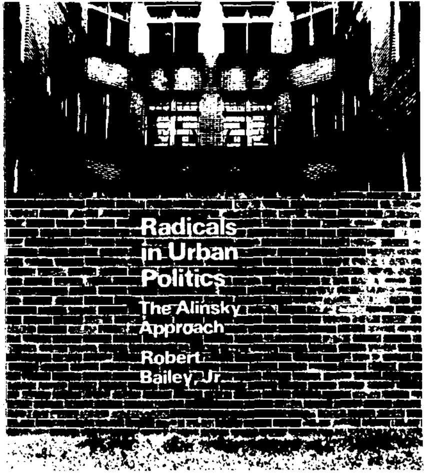

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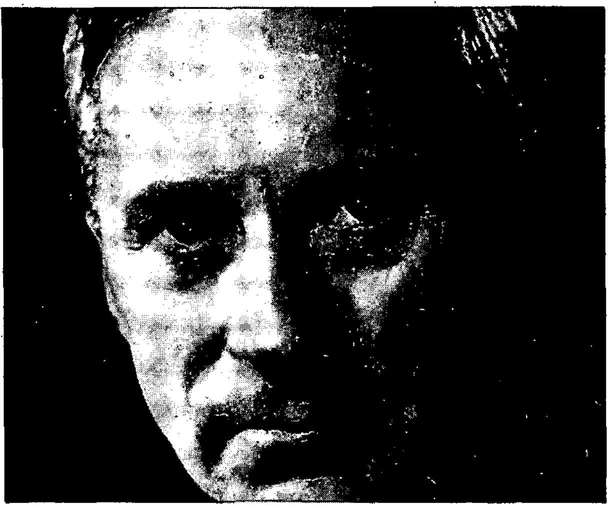

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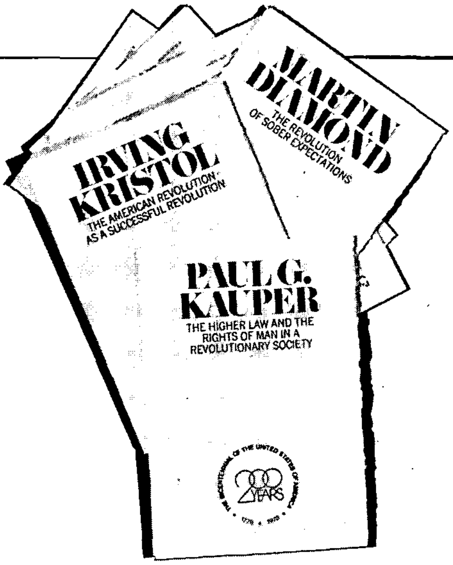
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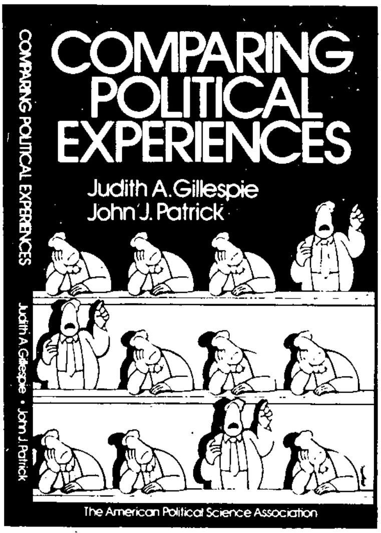
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